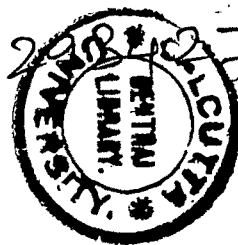


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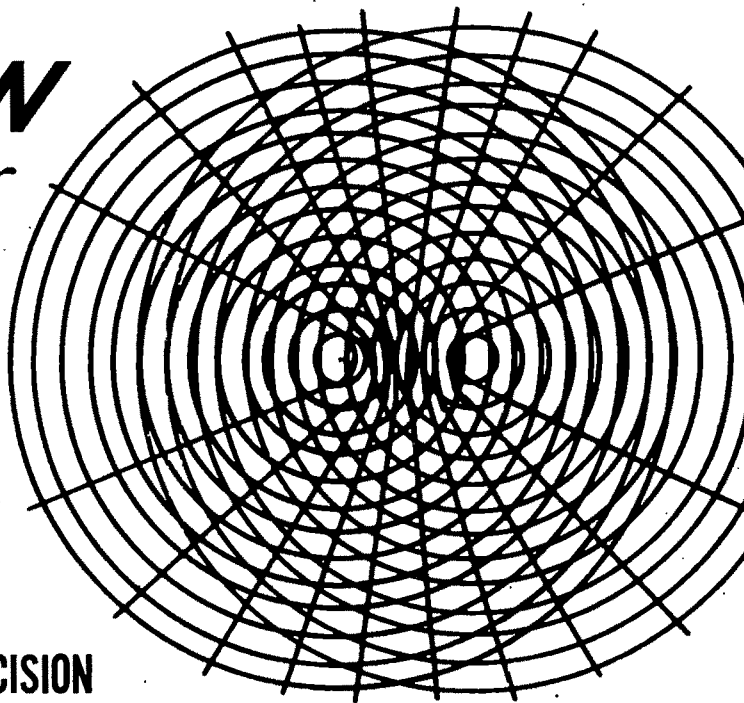
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ARTICLES

- 415 The Structuring Principle: Political Socialization and Belief Systems.** This paper assesses the theoretical significance of data on childhood political learning. Two socialization models are involved. Each confers relevance on childhood learning by linking it with political outcomes. The first is an allocative politics model, which seeks a linkage with policy outputs. The other is a system persistence model, looking toward the stability and continued existence of political systems. Each model incorporates the following assumptions: (a) the primacy principle: childhood learning is relatively enduring throughout life; (b) the structuring principle: basic orientations acquired during childhood structure the later learning of specific issue beliefs.

It is this structuring principle which we examined and tested in the present paper. The data show no or little association between childhood orientations and the later learning of specific beliefs about the most important political issues of the day. Our evidence suggests a need to carefully reexamine the basic assumptions and directions of current political socialization research.

By DONALD D. SEARING, Associate Professor of Political Science, JOEL J. SCHWARTZ, Associate Professor of Political Science and ALDEN E. LIND, Assistant Professor of Political Science, University of North Carolina, Chapel Hill.

- 433 Urbanization and Political Participation: The Case of Japan.** Research has shown that place of residence (urban-rural) has an ambiguous influence on political participation. Japan is one of several major nations in which rural people participate politically more than their urban counterparts. An analysis of urban-rural political attitudes in Japan shows some of the roots of the tendencies in participation. While urban residents are more psychologically involved in national politics, they also tend toward greater pessimism and have lower feelings of the vote being a duty than do their rural counterparts. In contrast, rural voters are highly dutiful in orientation, as well as being strongly involved in local politics and more concerned than urban residents about having their political needs represented. A Coleman effect parameter analysis of the urban-rural attitudes and political participation shows that the attitudes do in fact account for differences in political participation in local politics. But the attitudinal tendencies are less important for national political participation, and it is possible that the older social influence interpretation of Japanese urban-rural differences is most applicable to sectoral trends in this case.

By BRADLEY M. RICHARDSON, Associate Professor of Political Science, Ohio State University.

- 453 Aspects of Coalition Payoffs in European Parliamentary Democracies.** One important proposition about the distribution of coalition payoffs is found in W. A. Gamson's theory of coalition formation: "Any participant will expect others to demand from a coalition a share of the payoff proportional to the amount of resources which they contribute to a coalition." This proposition is tested in a universe of cabinet coalitions existing in thirteen European democracies during the postwar period. Here, payoffs to partners are indicated by the percentage share of cabinet ministries received by parties for their percentage contribution of parliamentary seats/votes to the coalition.

The proportionality proposition is shown to hold strongly. Disproportionality, however, is observed to occur in distributions at the extremities of party size—large parties tend to be proportionately underpaid and small parties overpaid, the larger or smaller they become. This effect, however, is most pronounced when the size of the coalition is small, and tends to reverse itself as size of the coalition increases.

By ERIC C. BROWNE, Assistant Professor of Political Science, University of Wisconsin, Milwaukee and MARK N. FRANKLIN, Lecturer, University of Strathclyde.

- 470 The Concept of Organizational Goal.** The organizational goal concept is important for significant types of organizational research but its utility has been downgraded in recent scholarship. This reviews critically key contributions to conceptualizing the organizational goal and synthesizes of their elements into a more concrete and comprehensive conceptualization. The efforts of E. Seashore and Yuchtman, Simon, and Thompson to bypass the need for a goal concept in evaluation and other behavioral research are unconvincing in important respects. However, they are persuasive in underscoring the importance of viewing organizational goals as multiple and as empirically terminated. Perrow, Gross, and others convincingly suggest a dual conceptualization, so that goals are dichotomized into those with external referents (transitive goals) and those with internal referents (reflexive goals). Deniston et al. contribute the desirability of subsetting the goals of organizations into "program goals" and of differentiating goals from both subgoals and activities. The existence and relative importance of organizational goals and an allied concept, "operative goals," may be operationally determined by current social science methods. The goal concept as presented here has implications for the evaluation of organizational effectiveness, for research on organizational behavior, for organization theory, and for views of the role of organizations in society.

By LAWRENCE B. MOHR, Associate Professor, Department of Political Science, and Research Associate, Institute of Public Policy Studies, University of Michigan.

- 482 **Attitudes of the Arab Elite Toward Palestine and Israel.** Unlike most studies of the Arab-Israeli conflict, this one attempts not only to delineate the attitudes of Arab elite (professionals and university students) at home and abroad on the issue but also and primarily, to investigate the determinants of those attitudes. The sources include the relevant political literature, especially since the 1967 war; a set of depth interviews conducted among members of the elite in the Arab world; and a more structured sample survey conducted among Arab students and professionals in the U.S. Conclusions are based on the data from all three sources. The results show that religion, sectionalism, age, and level of education are important determinants of political attitudes and behavior among Arab elite. Neither locale (urban-rural residence) nor socioeconomic background, however, is found to be a significant factor.

By MICHAEL W. SULEIMAN, Professor of Political Science, Kansas State University.

- 490 **Parties as Utility Maximizers.** The article introduces two models of political party decision making. Both models assume that the parties are solely interested in policy and that winning the election is just a means to that end. In one, the parties are competitive, while in the other the parties collude. The main result, in either case, is that the parties tend to be unresponsive to the interests of the voters.

The models are analyzed in an intransitive case (an election concerned only with income distribution) and a transitive one (an election where all political attitudes can be put on a left-right continuum), and under the assumptions of perfect and imperfect information.

With perfect information the intransitive case results in the parties ending up with all the income; while in the single peaked case neither party will have a position to the left (right) of the left (right) party's most preferred position whatever the attitudes of the voters.

Finally it is shown that it is rational for the parties to collude and present similar platforms.

By DONALD A. WITTMAN, Assistant Professor of Economics, University of California at Santa Cruz.

- 499 **The Learning of Legislative Norms.** The paper focuses on the learning of legislative norms on the part of freshman members elected to the United States House of Representatives in November, 1968. Since a research interest in learning is a longitudinal concern, a two-page panel design was employed. The first set of interviews conducted in late January and February of 1969 and the second set the following May. As the concept of a norm involves the notion of shared expectations, a sample of the nonfreshman members of the 91st Congress was also interviewed.

The main finding of the paper is that the amount of norm learning was surprisingly low; it appeared that freshmen largely knew the general House norms prior to entering Congress. And the extent of attitude change toward the norms once in office was minimal. Freshmen and nonfreshmen generally expressed similar attitudes toward the norms. Support for the norm of apprenticeship was found to be weak, suggesting the need to revise the traditional image of the freshman representative.

By HERBERT B. ASHER, Assistant Professor of Political Science, Ohio State University.

- 514 **The Strange Case of Relative Gratification and Potential for Political Violence: The V-Curve Hypothesis.** Perception of discrepancy between optimum level of achievement with respect to desired values and actual level of achievement is a concept that has figured importantly in explanations of collective violence and its subset, political violence (approval of and readiness to engage in behaviors which constitute progressively greater challenge to a political regime). Hypotheses about relationships between a number of static and dynamic achievement discrepancy constructs (labeled "relative gratification," and built from a variant of the Cantril Self-Anchoring scale) are tested. The achievement discrepancy constructs generally show only a weak degree of association with potential for political violence. However, measures of shift over time in discrepancy show an unexpected and intriguing relationship with potential for political violence: individuals who perceive negative change and individuals who perceive *positive* change show the highest potential for political violence, while individuals who perceive no change show the lowest potential for political violence; and this V-Curve relationship persists in the presence of various control variables. Moreover, *absolute magnitude* of shift in discrepancy from present to future shows a moderate degree of correlation with potential for political violence, and makes an independent contribution to a linear additive model. The data base is a sample of a population in which instances of political violence have been relatively frequent in the past.

By BERNARD N. GROFMAN, Assistant Professor of Political Science, and EDWARD N. MULLER, Associate Professor of Political Science, State University of New York at Stony Brook.

- 540 **The Relationship Between Seats and Votes in Two-Party Systems.** An enduring fact of life in democratic electoral systems is that the party winning the largest share of the votes almost always receives a still larger share of the seats. This paper tests three models describing the inflation of the legislative power of the victorious party and then develops explanations of the observed differences in the swing ratio and the partisan bias of an electoral system. The "cube law" is rejected as a description, since it assumes uniformity (which is not observed in the data) across electoral systems. Explanations for differences in swing ratio and bias are found in variations in turnout over districts,

the extent of the "nationalization" of politics, and, most importantly, in who does the districting or reapportionment. The measures of swing ratio and partisan bias appear useful for the judicial evaluation of redistricting schemes and may contribute to the reduction of partisan and incumbent gerrymandering.

By EDWARD R. TUFTE, Associate Professor of Politics and Public Affairs, Princeton University.

- 555 Regression Analysis and Discriminant Analysis: An Application of R. A. Fisher's Theorem to Data in Political Science.** The conversion of multiple regression analysis to discriminant analysis is not only of theoretical interest, but—in view of the extensive use of these methods in political science—it also has considerable value for applications. It is the purpose of this presentation to explain the underlying theoretical relationship and to demonstrate its application in the form of an example chosen from the judicial process. Specifically, the Supreme Court's acceptance or rejection of the fact that the defendant was not advised of his right to counsel in an involuntary confession case is considered as a function of the appearance, nonappearance, or denial of the fact in lower court records and appellate briefs. Since the acceptance or rejection of the fact by the Supreme Court is a dichotomous dependent variable, discriminant analysis is appropriate. It is shown in this study how discriminant analysis can be employed by initially using regression analysis, not only in the example presented for illustration, but in any situation in which a phenomenon with dichotomous manifestations may be examined as a function of specified variables.

By FRED KORT, Professor of Political Science, University of Connecticut.

- 560 A Research Note on Machine Politics as a Model for Change in a Philippine Province.** The purpose of this research note was to determine whether Philippine politics could be characterized as fitting a "machine politics model" (James Scott, "Corruption, Machine Politics, and Political Development, *APSR*, 63 [December, 1969], 1142–1158). A province was selected which matched the criteria cited in Scott's model, and provincial political leaders and subleaders were asked to evaluate important considerations they used in deciding whom to support for public office.

Scott proposed that in electoral political systems, support moves from a dependence on deference to a dependence on particularistic rewards, and finally to dependence on ideology. Part of the study tested the three-phase model using factor analysis on ten variables generally thought to be crucial in Philippine politics. The factor analysis revealed six factors, three which matched Scott's three phases plus: the chance of winning, the use of threats, and party loyalty. Although Scott's three-phase model was rejected as inadequate, at the descriptive level the general attributes of machine politics (particularly as Philippine politics has moved from a reliance on deference and personal loyalty to a dependence on material reward) could be used to characterize Philippine politics.

By LOUIS P. BENSON, Assistant Professor of Political Science, Kent State University.

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582 BOOK REVIEWS AND ESSAYS

- 582 Woodrow Wilson and the Study of Administration: A New Look at an Old Essay.** Unquestionably, Woodrow Wilson's scholarly essay, "The Study of Administration," (1887) stands as an historic landmark in American administrative thought. As Leonard D. White once wrote, "Wilson's essay introduced this country to the idea of administration." Based upon the recent publication of the Woodrow Wilson papers by Princeton University Press, the present paper attempts to examine the origin and enduring contribution of Wilson's administrative thought. The central thesis of the paper is that Wilson's administrative theories grew out of the salient ideas of late nineteenth century America, particularly, Social Darwinism and the pressing demands for political reform. In many respects, however, Wilson's essay created more issues than it resolved since it failed to delineate clearly the substance and boundaries of the field of administration.

By RICHARD J. STILLMAN, II, Associate Professor of Public Administration, California State College, Bakersfield.

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By EDWARD H. BUEHRIG, Professor of Political Science, Indiana University.

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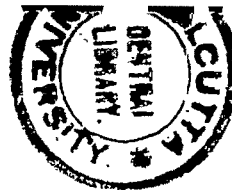
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***What if
they gave
an election
and nobody
came?***



POLITICAL ALIENATION AND POLITICAL BEHAVIOR

by David C. Schwartz

"An outstanding job of synthesis, theory and construction and empirical validation. This book is likely to be judged one of the major contributions to political psychology of the decade."

— Ted Robert Gurr, Northwestern University

This book presents and confirms by extensive research a completely original three-variable psychological theory to explain the causes and consequences of political alienation and how psychological attitudes predict political behavior. The author shows why present theories of social status and economic difficulties do not adequately explain alienation and replaces them with theory based on observations of the following individual psychological phenomena: threat from value conflict, perceived personal inefficacy, and perceived systemic inefficacy. A process model for predicting political behavior is also stated and validated. This book will be of interest to many social scientists and will be especially important to anyone concerned with American politics and more generally with the relationships of economic, social, and psychological forces manifested in political behavior.

David C. Schwartz is presently Professor in the Political Science Department of Rutgers University and is the author of a number of articles published in major political science and behavioral science journals.

1973. 356 pp. \$9.95.

DILEMMAS OF SOCIAL REFORM: POVERTY AND COMMUNITY ACTION IN THE UNITED STATES (Second Edition)

by Peter Marris and Martin Rein

"The 2nd edition of *Dilemmas of Social Reform* is a valuable updating of a book that became a classic in the literature of urban politics and urban studies in just a few short years. It remains the best book on the efforts of various urban wars on poverty to improve the lives of the poor in the city, and its insightful analysis of the failure of these efforts has much to teach us about future such efforts. I especially liked the new final chapter, for it is a thoughtful and constructive analysis of the dilemmas, not just of social reform, but of democratic politics." — Herbert Ganz, Columbia University, Center for Policy Research.

This second edition of a classic work on social reform brings the story of community action up to date from 1965 to 1971. It is an account of the origins and development of community action from its beginnings in the Ford Foundation Gray Area Programs and the President's Committee on Juvenile Delinquency, through the rise and decline of the War on Poverty and the Model Cities Program. In a ruthlessly impartial examination of poverty programs two social scientists — one British, one American — explain why programs of such size and complexity have only a minimal chance of success. They describe the realities of reform and point up how the conservatism of bureaucracy, the rivalries among political and administrative jurisdictions, and the apathy of the poor have often hindered national and local efforts. On the other hand, they show how these obstacles can be overcome by an imaginative combination of leadership, democratic participation and scientific analysis.

Distinguished by an analysis of the major critics of community action, the book provides a balanced perspective of the movement against its many detractors. It is valuable for anyone engaged in community action, whether as organizer, consultant, official or politician.

Peter Marris is a member of the Institute of Community Studies in London. He has been a visiting lecturer in the Department of City Planning of the University of California, Berkeley, and is now working on a general study of social change. Martin Rein is Professor in the Department of Urban Studies and Planning at MIT. He has worked as a social worker with street gangs, has supervised a number of research projects, and has written about poverty, social planning, and the social work profession.

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ARTICLES

- 799 Caste and the Decline of Political Homogeneity.** The relationship between the traditional social organization of India, based on the principle of hierarchy, and the newly introduced democratic institutions and procedures, based on the principle of equality, has been a subject of diverse interpretations. The more significant of these interpretations are that the social organization has subsumed the new political system, and that the various units of social organization, namely, castes, have developed voluntary bodies or caste associations of their own in order to enter into an operative relationship with the new political system. The latter interpretation also implies that the democratic political socialization in India has been taking place by means of the caste associations. This study takes a hard look at such interpretations and points out that the internal cohesion of the social organization materially alters when it moves away from its primary social concerns—ritual, pollution, and endogamy—to nontraditional concerns. This change is reflected in the fact that highly fragmented decision-making processes of castes in nontraditional matters often lead to their substantial vote against candidates of their own castes. Such political differentiation within castes has occurred before the advent of certain caste associations, and in some cases despite them. These and other assertions are substantiated through data collected in a rural and an urban community where fieldwork designed to understand their political dynamics extended over a number of years.

By A. H. SOMJEE, Professor of Political Science, Simon Fraser University.

- 817 The Attribution of Variance in Electoral Returns: An Alternative Measurement Technique.** Swings in district vote for Congress are conditioned by many factors. An attempt is made here to apportion the variance in the partisan distribution of votes for U. S. representative among three levels of influence—national, state, and district—measuring the degree of nationalization, regionalization, and localization of voting. Previous attempts have defined "nationalization" of voting as the degree to which district interelection differences are numerically identical. Here this concept is defined in a two-stage regression model as the degree to which districts behave as if these differences were caused by the same factors. In contrast to previous research, national factors are found to be responsible for more than 50 per cent of the variance in local vote, with state and district forces accounting for 19 per cent and 26 per cent, respectively. Several analytic uses for this measure are suggested and illustrated.

By RICHARD S. KATZ, Ph.D. Candidate in Political Science, Yale University.

Comment. By DONALD E. STOKES, Professor of Political Science and Dean of the Graduate School, University of Michigan.

Rejoinder. By RICHARD S. KATZ.

- 835 Electoral Choice in the American States: Incumbency Effects, Partisan Forces, and Divergent Partisan Majorities.** It is a relatively common occurrence in American politics for state electorates to divide their partisan majorities between different parties, depending on the office contest. Observations concerning these divergent aggregate patterns are usually accompanied by speculation that the theoretical propositions on individual voting behavior, developed and tested in the context of presidential voting, hold less relevance for voting in statewide contests. Evidence presented in this paper does not bear out that view of state elections. The candidate incumbency context of state elections is introduced as an aid in predicting the partisan direction of split-ticket voting at the state level. Setting respondents in various conflict situations with respect to (1) basic party loyalties, (2) net assessments of presidential candidates, and (3) incumbent partisanship yields reasonably accurate specification of split-ticket voting patterns in gubernatorial, senatorial, and presidential contests; it also suggests at least one source of disparate partisan majorities among state electorates.

By ANDREW T. COWART, Assistant Professor of Political Science, University of Oslo.

- 854 The Impact of Party on Voting Behavior in a Nonpartisan Legislature.** The Nebraska nonpartisan legislature serves as a control setting for testing several hypotheses about the impact of party and constituency on voting behavior in legislative bodies. Specifically, in light of the data obtained from a setting where party identification is present but party leadership and organization are absent, the following hypotheses are examined: that political parties are important in structuring voting behavior because of the influence of party leaders and organization; that party is important because party identification is a surrogate for sets of beliefs and attitudes that distinguish members of one party from another; or that party is important because party differences reflect different constituency bases of the party. In a roll-call analysis of five sessions utilizing Guttman-scaling and regression techniques, it was found that in the absence of party leadership and organization, voting is highly unstructured. Further, dimensions of voting that were found are largely unexplainable in terms of standard party and constituency variables. Thus, party identification and constituency influence appear to be insufficient cues for the organization of legislative voting behavior, in the absence of party leadership.

By SUSAN WELCH, Associate Professor of Political Science, University of Nebraska, with the assistance of ERIC H. CARLSON, Law Student, University of Oregon.

- 868 **Foreign Aid and United Nations Votes: A Comparative Study.** This study examines in a comparative foreign policy framework the relationship between bilateral foreign aid allocations and pairwise voting agreements between developed and developing nations in the UN General Assembly. The foreign aid donors considered include the United States, the Soviet "bloc," and the twelve other UN members of the OECD's Development Assistance Committee. Two different measures of aid allocations in two three-year periods (1962-1964 and 1965-1967) are correlated with two different measures of the percentage of agreements in the UN between each aid donor and its aid recipients, with both indices calculated on the basis of all roll calls taken in the 1963 and 1966 General Assemblies.

In general, the results of the analysis were found to be consistent with the hypothesized positive association between aid and votes only in the case of the United States. For many of the remaining donors the association was found to be negative rather than positive, suggesting either that enemies are rewarded more than friends, or, alternatively, that there is little relationship of substantive interest between aid and votes for most donor countries. Even in the case of the U.S., however, which of the two variables should be considered a cause and which a consequence remains unresolved.

By EUGENE R. WITTKOFF, Assistant Professor of Political Science, University of Florida.

- 889 **Democratic Committee Assignments in the House of Representatives: Strategic Aspects of a Social Choice Process.** This paper examines the committee assignment process for Democratic members of the House of Representatives. Unlike previous studies of committee assignments, this paper employs data on the requests for assignments submitted by members to the Committee on Committees. The theoretical perspective employed is one in which all the participants in the process are rational actors who have goals they want to achieve and who choose among alternative courses of action on the basis of which alternative is most likely to lead to the achievement of those goals. We argue that the allocation of committee assignments affects the goals of all the participants in the process, and thus we consider the choices of actors in the process in terms of their goals; specifically the goals of re-election, influence within the House, and good public policy.

After first considering the process from the point of view of the member making requests, we show that the member's requests are related to the type of district he represents, and that the number of requests he makes is related to such considerations as whether he is a freshman, whether he faces competition from a member from his state, and whether there is a vacancy from his state on his most preferred committee.

The process is also considered from the point of view of the members making the assignments. Decisions on assignments are found to be affected by seniority (where success in getting requested committees is inversely related to seniority), margin of election (where members from marginal districts are more successful), and region (where southerners are less successful than members from other regions).

By DAVID W. ROHDE, Assistant Professor of Political Science, Michigan State University and KENNETH A. SHEPBLE, Assistant Professor of Political Science, Washington University, St. Louis.

- 906 **Candidates' Perception of Voter Competence: A Comparison of Winning and Losing Candidates.** This study, based on the two-wave questionnaire data collected from legislative candidates in Iowa, attempts to test the "congratulation-rationalization effect," a highly provocative hypothesis that John Kingdon formulated regarding politicians' beliefs about voters. The hypothesis asserts that winning candidates tend to develop complimentary beliefs about voters while losing candidates tend to develop beliefs deprecating to voters. The results of analysis indicate, however, no significant difference between winners and losers in terms of the direction and magnitude of changes in their beliefs about voters, suggesting that the hypothesis is invalid. When the hypothesis is reformulated in terms of "dissonance states" rather than "election outcomes," the evidence is strongly supportive. Among winners, those who perceive a high degree of dissonance more than those who perceive little dissonance tend to change their beliefs about voters in a favorable direction. Conversely, among losers, those who perceive a high degree of dissonance more than those who perceive little dissonance tend to change their beliefs in an unfavorable direction. Therefore, the "congratulation-rationalization" hypothesis can be sustained only if cast in direct dissonance terms.

By CHONG LIM KIM, Associate Professor of Political Science and DONALD P. RACHETER, Ph.D. Candidate in Political Science, University of Iowa.

- 914 **Residential Location and Electoral Cohesion: The Pattern of Urban Political Conflict.** This study examines the assertions of urban scholars that the spatial arrangement of urban populations is important in determining the amount of conflict displayed within American cities. The article analyzes the spatial distribution of class groups within 18 cities and the degree of voting solidarity and conflict displayed within segregated and integrated sections of each community. Data were gathered from precinct voting returns for several local referenda in each city to test the following hypotheses: (1)

The residential distribution of social-class groups will significantly influence the degree of electoral cohesion these groups display; (2) The spatial distribution of class groups will significantly influence the amount of electoral disagreement between class groups. The study found that communities that displayed segregated class groups had a high degree of class electoral solidarity. Within cities that manifested spatially integrated class groups, however, the electoral cohesion of each class was low. A social-class group located in an area of a city possessing wide class dissimilarity was not likely to vote in agreement with other groups of the same class located elsewhere in the city. The findings of this article suggest that location may be one of the sources of urban political conflicts.

By TIMOTHY A. ALMY, Assistant Professor of Political Science, University of Georgia.

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- 924 The Politics of Redistribution: A Reformulation.** This paper offers a critique and a reformulation of Brian Fry and Richard Winters's policy output study published in this *Review* June, 1970. Fry and Winters focused on the redistributive impact of public policy in the states. After devising a "redistribution ratio" that involves allocating state revenue burdens and expenditure benefits to families across income classes, they developed a model to explain the variance of this ratio from state to state. In contrast to the findings of many earlier policy output studies, they hypothesized that political variables would have more explanatory power than socioeconomic variables.

Unfortunately some methodological shortcomings detract from the potential value of the Fry and Winters study. In this paper, alternative methodologies are used to reformulate a redistribution ratio for each state, and the recalculated ratios are found to vary significantly from those obtained by Fry and Winters.

The shortcomings of the Fry and Winters explanatory model are discussed. Despite these shortcomings, however, the regression analysis employed by Fry and Winters is repeated using the reformulated redistribution ratios in order to test the impact of this reformulation. Again the results obtained in this paper vary substantially from those of Fry and Winters.

By BERNARD H. BOOMS, Associate Professor of Economics, The Pennsylvania State University and JAMES R. HALLDORSON, Accounting Management Trainee, Tax Department, F. W. Woolworth Company.

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- 934 Voting Systems, Honest Preference and Pareto Optimality.** The market is a decentralized system that can bring about efficient economic decisions. This paper examines whether social choice mechanisms can duplicate this success in the political arena. The famed Arrow result tells us centralized systems cannot achieve efficient, nondictatorial outcomes unless they rely on cardinal preferences. With decentralization, efficiency comes to require something more: the truthful revelation of preferences. Schemes that elicit honest preferences are derived here. By their very structure they are shown to lead to inefficient outcomes. This negative result leads to the question whether the validity of the initial analogy continues. Market-based standards of performance may be inappropriate for investigations of political phenomena.

By RICHARD ZECKHAUSER, Professor of Political Economy, Harvard University.

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- 947 A New Shape Measure for Evaluating Electoral District Patterns.** The concept of shape is considered in abstract terms drawing on approaches outside the electoral districting literature. The concept is broken down into a series of four divergences from "compactness" relating to "elongation," "indentation," "separation," and "puncturedness." Given this conceptual framework, the use of shape measures in electoral districting is reconsidered and a new shape measure is proposed. This assesses the indentation of a district shape and is based on the internal angles within the shape. It is suggested that this measure may be particularly relevant to the evaluation of proposed new districting patterns. The technique is illustrated using proposed new Congressional Districts for Iowa.

By PETER J. TAYLOR, Lecturer in Geography, University of Newcastle upon Tyne, England.

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- 951 Policy and Priority in the Budgetary Process.** Recent quantitative studies vastly understate the political conflicts and policy choices that are embedded in the budgetary process. The reason for this lies in the way these quantitative studies have organized budgetary data. Thus far the units of analysis have been federal agencies, the administrative categories of government. The "striking regularities" that have been reported reflect—quite accurately—the great stability of the administrative structure of government. However, these categories do not describe the intense competition between programs and policies that takes place within the framework. We argue, further, that the entire metaphor of an inert bureaucratic machine doing this year essentially what it did last year is erroneous. Rather, priority setting in the federal bureaucracy more resembles the market situation of nineteenth century capitalism where aggressive "policy entrepreneurs," unequal in talent and resources, struggle to build and sustain support for their programs. The competition between policies is both reflected in and promoted by the budgetary process. By shifting the units of analysis to programs and trans-

forming these data so that programs of different size are commensurate, we develop an index that reflects the relative growth and decay of programs as they compete for budgetary resources.

By PETER B. NATCHEZ, Assistant Professor of Politics, Brandeis University and IRVIN C. BUPP, Lecturer in the Kennedy School of Government, Harvard University.

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ARTICLES

- 1142 Authority Patterns: A Structural Basis for Political Inquiry.** The paper proposes a novel (even if not wholly unprecedented) solution to an old and fundamental problem: What should be the scope of political studies? Arguments have long been directed against the conventional equation of the scope of the field with the study of "state-organizations" and structures that directly impinge upon such organizations. The arguments are convincing. However, the principal proposed alternatives have important flaws of their own. These alternatives are the extension of the scope of the field to phenomena "functionally" similar to state-organizations and the inclusion in political study of all "asymmetrical" social relations—power, influence, or control relations. By means of the classificatory method of "progressive differentiation," an alternative that seems preferable is worked out: equating political study with the study of authority patterns in any and all social units. That conception of the subject matter of the field, it is argued, avoids all the difficulties raised by other conceptions and affords all of their advantages. Above all, it reconciles subjective interests with scientific (or disciplinary) imperatives and achieves a proper trade-off between the numerousness and homogeneity of phenomena covered by the field—a trade-off critical for the achievement of general, testable, informative empirical theory. A concluding section discusses the place in political study, thus conceived, of the study of international relations and of recent work in "political economy," which appears to focus on symmetrical, not asymmetrical, interactions.

By HARRY ECKSTEIN, IBM Professor of Foreign and International Studies, Princeton University.

- 1162 National and Local Forces in State Politics: The Implications of Multi-Level Policy Analysis.** Studies of American states and their policies are severely handicapped by the use of a single level of analysis. The operations of American politics and the assumptions of correlation methodology imply that only a multi-level approach can adequately comprehend state politics. All major aspects of politics, but particularly policies, are distorted by the single level approach. The extent of distortion suggests that relations among states and between states and the national government are the prime determinants of state politics and that the study of states ought to be organized around these relations. This approach accounts for the salient characteristics of state politics, indicating that states are not political systems but collections of tangentially related components of a national system. States which appear systematic derive their coherence from interactions with the national pattern, a process with important residual effects on the legitimacy of state governments.

By DOUGLAS D. ROSE, Assistant Professor of Political Science, Tulane University.

- 1174 Innovation in the States: A Diffusion Study.** This study focuses on a nonmonetary dimension of public policy—innovation by states in the fields of education, welfare, and civil rights. Innovation is considered equivalent to the adoption of a law by a state. From the literature on diffusion (or spread) of innovations, the explanation of user interaction is taken, and a simple model with an interaction term is constructed. The model performs fairly well when evaluated by several common criteria. The results do vary somewhat from one issue area to another; other types of supplementary analysis also indicate variation in diffusion patterns according to the issue involved. Political and economic differences among states are found to account for differences in time of adoption, and "innovativeness" is shown to be an issue- and time-specific factor.

By VIRGINIA GRAY, Assistant Professor of Political Science, University of Minnesota.

Comment. By JACK L. WALKER, Associate Professor of Political Science, University of Michigan.
Rejoinder. By VIRGINIA GRAY.

- 1194 Economic Growth and Intergenerational Change in Japan.** This article seeks to apply the hypotheses and findings presented by Ronald Inglehart in his "The Silent Revolution in Europe: Intergenerational Change in Post-Industrial Societies," to the Japanese scene. Inglehart argued that affluence had produced changes in value priorities in Western Europe: the older generation has acquisitive values, while the younger age groups have postbourgeois values. An analysis of Japanese data collected by the Institute of Mathematical Statistics in four national surveys in 1953, 1958, 1963, and 1968 suggests that value changes in Japan produced by rapid economic growth may be somewhat different. Three types of change were found, namely intergenerational, life-cycle, and adult. Compared to its elders, the younger generation is less acquisitive, more democratic, and somewhat more inclined to value freedom. The most important value change, however, appears to be intergenerational change from collectivity orientation toward individuation. It is suggested that individuation has led to a tendency toward privatization and less concern for the electoral process on the part of youth. Whether or not this is a temporary phenomenon is not clear.

By NOBUTAKA IKE, Professor of Political Science, Stanford University.

- 1204 Issue Area and Foreign-Policy Process: A Research Note in Search of a General Theory.** The purpose of this research note is to suggest a potential general paradigm for the study of foreign-

policy processes. It is explicitly synthetic in that it combines Arnold Wolfers's notion of a continuum, the extremities of which he labels the pole of power and pole of indifference, with Theodore Lowi's efforts to affirm the nexus between issue and policy process. Two questions prove crucial in the determination of issue area: Is or is not the domestic impact of the issue symmetrical? And are the political goods at stake exclusively tangible or not? With the answers to these questions it becomes possible to specify the issue area (distribution, regulation, "interaction-protection," redistribution) in which an event may be classified and to hypothesize the nature of the policy process (the identity of the major actors, the intensity of conflict) to be observed. Particular attention is paid to limited war as a redistributive issue area in order to make the case that redistribution, contrary to Lowi's view, is an important foreign policy process. Finally an effort is made to suggest how issue-based propositions could be utilized in the transnational comparison of foreign policy processes. It is suggested that differences in the policy process across issue areas within a given state may be as great as differences in process within a particular arena of power for two states as different in political system as the United States and the USSR.

By WILLIAM ZIMMERMAN, Associate Professor of Political Science, University of Michigan.

-
- 1213 Party and Incumbency in Postwar Senate Elections: Trends, Patterns, and Models.** Senators who seek re-election usually achieve it. Yet it is not clear whether they tend to win mainly because they are incumbents or because their party is strong in their states. Thus, the two principal questions motivating this study are the following: (1) What is the relative importance of party and of incumbency in influencing the outcomes of Senate elections? (2) How has their relative importance changed over the last quarter-century? To answer these questions, a theory is developed accounting for the outcomes of Senate elections in terms of the major potential sources of electoral support given to the candidates, namely, party loyalty, incumbency, "national tides," and idiosyncratic factors (e.g., issues, personality, local conditions). The theory is then represented in a formal model for which are generated multiple regression estimates of the respective roles of party and incumbency in all postwar Senate contests.

The major finding is that the relative importance of party and incumbency has changed dramatically over the last quarter-century. Party has undergone an overall decline in influence, while incumbency has experienced a roughly proportionate increase. At the same time, the importance of idiosyncratic factors has grown. The implications of these results for broader theories of American politics, including the argument that the United States has been experiencing a "critical realignment," are noted.

By WARREN LEE KOSTROSKI, Assistant Professor of Political Science, Wittenberg University.

-
- 1235 The Paradox of Vote Trading.** Although, conventionally, vote trading in legislatures has been condemned as socially undesirable by both scholars and lay citizens, a recently popular school of scholarship has argued that vote trading improves the traders' welfare in the direction of Pareto-optimal allocations. This essay is an attempt to reconcile the disagreement by showing formally that vote trading does improve the position of the traders but that at the same time trading may impose an external cost on nontraders. In sum, it turns out that sporadic and occasional trading is probably socially beneficial but that systematic trading may engender a paradox of vote trading. This paradox has the property that, while trading is immediately advantageous for the traders, still, when everybody trades, everybody is worse off. Furthermore, vote trading may not produce a stable equilibrium that is Pareto-optimal either for individual members or for coalitions of members.

By WILLIAM H. RIKER, Professor of Political Science, University of Rochester, and STEVEN J. BRAMS, Associate Professor of Political Science, New York University.

-
- 1248 Schema Theory: An Information Processing Model of Perception and Cognition.** The world is complex, and yet people are able to make some sense out of it. This paper offers an information-processing model to describe this aspect of perception and cognition. The model assumes that a person receives information which is less than perfect in terms of its completeness, its accuracy, and its reliability. The model provides a dynamic description of how a person evaluates this kind of information about a case, how he selects one of his pre-existing patterns (called schemata) with which to interpret the case, and how he uses the interpretation to modify and extend his beliefs about the case. It also describes how this process allows the person to make the internal adjustments which will serve as feedback for the interpretation of future information. A wide variety of evidence from experimental and social psychology is cited to support the decisions which went into constructing the separate parts of the schema theory, and further evidence is cited supporting the theory's system-level predictions. Since the schema theory allows for (but does not assume) the optimization of its parameters, it is also used as a framework for a normative analysis of the selection of schemata. Finally, a few illustrations from international relations and

especially foreign-policy formation show that this model of how people make sense out of a complex world can be directly relevant to the study of important political processes.

By ROBERT AXELROD, Assistant Professor of Political Science, University of California, Berkeley.

- 1267 Human Rights Without Discrimination.** This paper focuses on the requirement of the U. N. Charter that members shall promote human rights "without distinction as to race, sex, language or religion." It asks: (1) whether "without distinction" means "without any differentiation" or "without discrimination"; (2) whether the naming of race, sex, language, and religion means that "distinction" on other bases remains permissible, or whether what is ruled out is discrimination of *any kind* that affects human rights; and (3) whether the search for equality has not been internationalized, at least in principle, and what the implications of the internationalization may be. The inquiry can be classified as legal/philosophical.

The third question leads to an analysis of the "without distinction" clause of the Charter in terms of the principle of equality. Claims to nondiscrimination are negative claims to equality, and they have as their counterpart claims to affirmative action on behalf of equality—often for groups. The problem is that affirmative action promoting equality for groups entails differentiations among individuals that may be discriminatory. To resolve the dilemma, the test of reasonableness is endorsed—and the search for standards for judging it goes on—at both the domestic and international levels.

By VERNON VAN DYKE, Professor of Political Science, University of Iowa.

- 1275 Minority Electoral Politics in a North Indian State: Aggregate Data Analysis and the Muslim Community in Bihar, 1952–1972.** Frequent elections and a long tradition of census taking in India should combine to provide excellent scope for aggregate data analysis, but so far they have not, largely because the electoral constituencies and the census tracts do not match. A number of ways have been devised to surmount the problem, none of them very satisfactory. This paper offers a new solution in the form of isoplethic mapping, a method that avoids the shortcomings of other approaches and permits use of demographic and voting data at the level of the state legislative assembly constituency.

Substantively the paper traces patterns of voting for Muslim candidates to the Bihar Legislative Assembly and the relationship between Muslim population distribution and vote polled by different political parties over six elections. Instead of becoming more integrated over time within the general body politic, it appears that the Muslim minority group has become more politically cohesive and better able to elect Muslims to office where their numbers are strong. At the same time, Muslims have become less able to win elections where they are fewer in numbers. This tendency has not reached a state of political polarization between the Hindu and Muslim communities, however.

By HARRY W. BLAIR, Assistant Professor of Political Science, Bucknell University.

- 1288 Fear, Apathy, and Discrimination: A Test of Three Explanations of Political Participation.** Students of political participation have generally taken as given that nonparticipation in politics is a result of apathy, and that apathy is a function of low income, low education, and low status. This article suggests that there are two additional potential explanations of political participation rates besides that offered by the conventional wisdom. One of these acknowledges that political participation for some people in some circumstances involves considerable risk, so that nonparticipation can be explained more accurately in terms of *fear* than in terms of apathy. The other views political participation as a response to a sense of "relative deprivation" or discrimination. After each of these three "models" of political participation is translated into operational terms, it is tested by determining how well it accounts for the variations in black political participation rates in Mississippi during the first half-decade following the 1965 Voting Rights Act. The conclusion that emerges from these tests is that political scientists have erred seriously by overlooking the role of fear in political life. In situations like those faced by blacks in Mississippi, situations that are probably similar to those in parts of the "developing world," apathy compares poorly with fear as an explanation of political participation.

By LESTER M. SALAMON, Assistant Professor of Political Science and Assistant Professor of Policy Sciences and Public Affairs, Duke University, and STEPHEN VAN EVERA, Graduate Student in Political Science, University of California, Berkeley.

Comment. By SAM KERNEL, Instructor in Political Science, University of Minnesota.

Rejoinder. By LESTER M. SALAMON and STEPHEN VAN EVERA.

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By A. JAMES GREGOR, Professor of Political Science, University of California, Berkeley.

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INSTRUCTIONS TO CONTRIBUTORS

Address correspondence about contributions to the *Review* to Nelson W. Polsby, Department of Political Science, University of California, Berkeley, Berkeley, California 94720. Each manuscript should be accompanied by an abstract of up to 150 words briefly describing the article's contents. All manuscripts and abstracts should be submitted IN DUPLICATE. They should be double-spaced and may be in typed, mimeographed, hectographed, or other legible form. Footnotes should appear at the end of the manuscript, not at the bottom of the page, and should also be doubled-spaced. Manuscripts that do not follow this format will be returned to the authors for retyping.

Since manuscripts are sent out anonymously for editorial evaluation, the author's name and affiliations should appear only on a separate covering page. All footnotes identifying the author should also appear on a separate page.

Address books intended for review to Philip Siegelman, *American Political Science Review*, 210 Barrows Hall, University of California, Berkeley, Berkeley, California 94720. Information, including News and Notes, for the Association's newjournal, *PS*, should be sent to 1527 New Hampshire Avenue, N.W., Washington, D.C. 20036; advertising, reprint and circulation correspondence should be sent to the Executive Director at the Washington office. Domestic claims for non-receipt of issues must be made within six months of the month of publication; overseas claims, one year. Advertising information and rates are available from Nancy Edgerton, Adv. Manager, APSA, 1527 New Hampshire Avenue, N.W., Washington, D.C. 20036.

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Larry W. Bowman

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EDITORIAL COMMENT

Behind the Bamboo Curtain. One of our most valued colleagues sent us the following memorandum recently. Since it bears centrally upon the intellectual mission of the *Review*, we reprint it herewith:

On the off-chance that you have not already done a similar analysis yourself, I enclose an exquisitely rigorous tally of the area-related pieces which have

appeared in our journal over the past five years. There are a number of tidbits of information that could be teased out of these data, but I particularly call your attention to the final entry.

Over the last year or so I have had occasion to satisfy myself that one possible explanation for that entry is false: it is *not* the case that the quality or general interest of scholarship on China is so low as to disqualify it from appearance in our journal. My alternative explanation is that scholarship mirrors reality—that China scholars have lived in splendid isolation, only occasionally interested in those of us who live outside the Middle Kingdom, but also that we have not encouraged them to be outward-looking.

My message to you as our Leader is simple: go to Peking.

Or failing that, go to some of the gurus of Chinese scholarship and invite them to begin submitting stuff to us.

Bring us together.

Disregard Previous Fortune Cookie. This same correspondent then sent us another letter:

Here is the copy I promised you of those data on area-related articles in journals other than the *APSR*. These data are consistent with those from the *APSR*. Indeed, if you ignore *World Politics*,

Area-related articles in APSR, 1968-1972

Western Europe	11
Africa	7
U.S.S.R.	7
Latin America	6
South and Southeast Asia	4
Japan	3
Canada	2
Near East and North Africa	1
Eastern Europe	1
Underdeveloped areas in general	4
Communist countries in general	2
Sino-Soviet relations	1
China	0

Area	J.P.	C.P.S.	C.P.	W.P.	MWJPS	W.P.Q.	Pol.	Totals
Western Europe	14	13	35	12	6	19	8	107
Africa	3	4	7	5	1	4	0	24
USSR	5	1	3	7	1	5	2	24
Latin America	7	8	4	8	4	13	1	45
South and Southeast Asia	1	1	7	5	4	6	3	27
Japan	3	2	3	0	0	3	1	12
Canada	2	0	0	1	0	7	1	11
Near East and North Africa	2	4	4	4	1	3	0	18
Eastern Europe	3	2	3	1	1	1	0	11
Underdeveloped areas	0	19	10	11	1	0	3	44
Communist Countries	2	0	0	1	0	1	1	5
Sino-Soviet Relations	1	0	0	0	0	1	0	2
China	0	0	2	5	0	2	0	9
	43	54	78	60	19	65	20	339

Where possible volumes between 1968 and 1972 were consulted.

there have been only four articles on China in political science journals over the last five years.

One possible explanation is simply that less research is being done by political scientists on China. This is part of the explanation, but only part, as I think I can show. Let us use completed dissertations as one index of overall research activity, both in a direct sense (dissertations become journal articles) and in an indirect sense (dissertations are spawned by productive instructors). Assume a year's lag between dissertations and publication; hence look at dissertations produced 1967-1971. Using Steve Blank's data from the Fall, 1972 issue of *PS*, (and combining China and Japan because Steve's data do not break down the category "East Asia"), we get the following results:

	East Asia	Western Europe
Proportion of all area-related dissertations which deal with this area:	11%	17%
Proportion of all area-related articles which deal with this area:	6%	32%

The apparent conclusion: Research on Western Europe claims nearly twice its "fair share" of the general political science "publication space" devoted to area-related research, while research on East Asia claims barely half its "fair share," if by "fair share" we mean "proportionate to volume of (dissertation) research." As I said in my earlier letter, a variety of explanations are possible; I think the East Asian half of the problem, at least, deserves attention.

In general, we only occasionally solicit articles for the *Review*. Perhaps this would be a suitable occasion. We have the feeling that the specialized journals in the Asian field are doing such a good job that they have choked off sub-

missions to the *Review* as well as to most other general purpose journals in political science. With a little luck, however, this note might provoke a freshet of submissions here. If so, we pledge ourselves to give each and every submission serious consideration, and not to restrict ourselves to one from row A and one from row B.

Typoes. Although we do a lot of complaining about the complaints we get, every once in a while a complaint comes along that charms even while it chagrins. Here is one we received the other day:

Let this letter be my official concession of defeat. I fought the good fight but I lost. In my review of Dale Vinyard's book *The Presidency* (*APSR* 67, March 1973, pp. 234-236), illustrating Vinyard's error-prone ways, I pointed to his spelling of McGinniss. Vinyard insisted on omitting the second and final "s". Strangely, so did the secretaries, assorted editors, and printers of the *APSR* book review section. Painstakingly, I added the requisite "s" in my letters and final proof pages. You won. There is McGinnis, the last "s" glaring in its absence, the last word on page 234. Vinyard must be giggling in his cups as I cry in mine.

David L. Paletz

P.S. It is clever of you to have misspelled Vinyard as Vynard on p. 235 first full paragraph, first column.

What to do about typos? Our peerless proof-reader, Jeanne Dritz, ferrets out nearly all of them. Once in a while she misses. Sometimes, our printer inserts new errors while correcting the old, even after the page proof stage. The March 1973 issue, of which Professor Paletz complains, was a rats' nest of errors by every-

body, and there wasn't much we could do about it. Except complain.

Articles Accepted for Future Publication

- Paul R. Abramson, Michigan State University, "Generational Change in American Electoral Behavior"
- Peter H. Aranson, Georgia Institute of Technology, Melvin J. Hinich, and Peter C. Ordeshook, Carnegie-Mellon University, "Election Goals and Strategies: Equivalent and Non-Equivalent Candidate Objectives"
- William I. Bacchus, Commission on the Organization of the Government for the Conduct of Foreign Policy, "Diplomacy for the '70s: An Afterview and Appraisal"
- Paul Allen Beck, University of Pittsburgh, "Environment and Party: The Impact of Political and Demographic County Characteristics on Party Behavior"
- Robert A. Bernstein and William W. Anthony, Texas A&M University, "The ABM Issue in the Senate, 1968-1970: The Importance of Ideology"
- Gordon S. Black, University of Rochester, "Conflict in the Community: A Theory of the Effects of Community Size"
- Steven J. Brams, New York University and Morton D. Davis, City College of New York, "The 3/2s Rule in Presidential Campaigning"
- Christopher Bruell, Boston College, "Thucydides' View of Athenian Imperialism"
- Walter Dean Burnham, Massachusetts Institute of Technology, "Theory and Voting Research: Some Reflections on Converse's 'Change in the American Electorate'"
- Blair Campbell, University of California, Los Angeles, "Helvétius and the Roots of the 'Closed' Society"
- Edward G. Carmines, State University of New York, Buffalo, "The Mediating Influence of State Legislatures on the Interparty Competition-Welfare Expenditures Linkage"
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Authority Patterns: A Structural Basis for Political Inquiry*

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Every field of inquiry rests, at its most fundamental level, on an implicit or explicit conception of its "scope"—that is to say, a conception of the nature of its subject matter and a delimitation of that subject from others. To be sure, overlaps and linkages can usually be found among any set of academic fields, particularly among "sister disciplines," like the various social sciences. Nevertheless, prevalent conceptions of the scope of a field generally imply more than just an arbitrary subjective interest in particular phenomena. They also imply that a subject matter so differs from others that it justifies, or indeed requires, considerable autonomy, in the sense of a distinct set of disciplinary tools and products: special descriptive concepts, methods of inquiry, general theoretical perspectives and approaches and empirical generalizations.

Political scientists' conceptions of the scope of their field have, in the past, tended toward two extremes: on one hand, a very narrow conception centered on "state-organizations"; on the other, a very broad one encompassing all asymmetric human relationships. Both conceptions turn out to have important flaws, either as definitions of subjective interests or delineations of a subject matter in terms of disciplinary considerations. Awareness of these flaws accounts for one of the more peculiar facets of contemporary political studies: the remarkably numerous and various attempts by political scientists to define explicitly the nature of what they study—something normally taken for granted, and therefore left implicit, in academic fields. This discussion presents yet one more such attempt justified by lack of consensus on the matter and

failure (as it seems to me) to work out a satisfactory delimitation of the field's scope.

Politics as State-Organizations

Until quite recently, the predominant (for a long time, in fact, fully consensual) conception of political phenomena was, quite simply, that they had to do with "governments," or "state-organizations," as we know them in the modern West, together with organizations subsumed to them, like local governments, or directly impinging on them, like political parties. Most political scientists discerned little need to worry about the precise meaning of terms like government and the state. Hence the prevalent approach was rather like that predominant in contemporary studies of modernization: modernity is what exists in Western Europe and North America. Differences in meaning, mostly implicit, were many,² but mainly shades and nuances around a common and familiar core. State-organizations were thought of as those formally organized structures of societies that specialize in the exercise of "sovereignty," as that term has been understood since, roughly, the early seventeenth century: specialized organizations that make laws, implement them, and resolve conflicts arising under them, and have a uniquely "legitimate" right to do so.

One basis for that conception of the scope of politics was certainly the subjective interests of political scientists—what they wanted to know about. Another, made explicit only when the conception was attacked, was that sovereign states and their organizations are *sui generis* among social structures, thus constituting a subject matter best kept separate from others. This argument was based on various grounds: (1) the state's supposedly unique membership structure (the fact that it is not associational, i.e., not something one can join or opt out of at will or low cost); (2) its supposedly unique scope (ability to regulate subsidiary units without being regulated by a superior one); (3) its supposedly unique functions (above all, inte-

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¹ For a list of ten such definitions (a sample, not an inventory) see Harry Eckstein, "The Concept 'Political System,'" Paper presented at the 1963 Annual Meeting of the American Political Science Association.

² No fewer than 145 different definitions of the term "state" are claimed to have been identified by Charles H. Titus. See his "A Nomenclature in Political Science," *American Political Science Review*, 25 (February 1931), 45-61.

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The Structuring Principle: Political Socialization and Belief Systems

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Of what interest to political science are children's attitudes? Almost without exception, our socialization research has focused on childhood political learning. But the assumption that children's attitudes are important has never been convincingly demonstrated. Neither has this research been justified by fully articulated theoretical statements. The present article does not pretend to provide the final word on these complex matters. Rather, it proposes only to clarify one aspect of existing models, and subject this to an empirical test.

Two theoretical models are involved.¹ Each offers relevance to childhood learning by linking it with political outcomes. The first is an allocative politics model, which seeks a linkage with policy outputs. The other is a system persistence model, looking toward the stability and continued existence of political systems. We believe that each model incorporates the following assumptions: (a) The primacy principle: childhood learning is relatively enduring throughout life; (b) The structuring principle: basic orientations acquired during childhood structure the later learning of specific issue beliefs. It is this structuring principle which will be examined and tested in the present paper.

Assessment of childhood socialization's importance has been sidetracked by a widespread belief. This is the notion that the primacy principle must be tested in any such assessment. And, since that test would require longitudinal data which are presently unavailable, the entire question has been set aside. Yet, we shall argue, theoretical relevance rests not upon the primacy principle alone, but upon the structuring principle as well. Our point will be that the models' theoretical relevance is only as strong as their weakest links to the dependent variable.

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¹ For a discussion of these models, see David Easton and Jack Dennis, *Children in the Political System* (New York: McGraw Hill, 1969), Chapter 2.

If these break, the models fail. If they hold, the models are ready for further testing.

Because these models are poorly articulated in socialization literature, it is essential to clarify the range of research to which our inquiry does and does not apply. Moreover, the falsification rules we propose have not been self-evident to colleagues in the field. The burden is upon us, then, to make a convincing case that our test is indeed a reasonable one. We shall examine first the primacy and structuring principles, and then the allocative politics and system persistence models which incorporate them.

The Primacy and Structuring Principles

In explaining political outcomes, one naturally looks to *adult* attitudes, not to the attitudes of children. But if adult attitudes are the phenomena of interest, why have socialization investigators interviewed children? The reason is that childhood antecedents are regarded as important determinants of adult attitudes. Hence, childhood learning represents a legitimate focus for political science inquiry.² To a considerable extent, this position reflects the work of child development theorists who argue that what is learned earliest in life is learned best, and is least likely to be displaced by subsequent experiences.³ The belief that early political learning

² At the same time, it should be emphasized that scholars engaged in this research do not suppose that political man is but the political child writ large. Quite the contrary. All agree that political socialization continues throughout the life cycle; that not all childhood learning influences adult behavior; and that, in dynamic modern societies, political attitudes are rarely transmitted unchanged from one generation to another. The childhood focus stems instead from an interest in exploring how political attitudes *develop*. Fred Greenstein nicely summarizes this concern as an attempt "to identify who learns what, from whom, under what circumstances and with what effects." See *Children and Politics* (New Haven: Yale University Press, 1965), p. 12.

³ Once again, Greenstein has succinctly summarized this position: "In general, the more important a political orientation is in the behavior of adults, the earlier it will be found to emerge in the learning of the child" (p. 56). In fact, a widely accepted belief holds that many politi-

is relatively enduring has secured a firm foothold as a working assumption for socialization studies. It has been characterized as the *primacy principle*. Although everyone subscribes to this principle in varying degrees,⁴ no one has categorically claimed unadulterated persistence for orientations acquired in childhood—except, perhaps, in the case of party identification. Each of the better-known studies has simply offered a series of childhood orientations which *may* influence later adult attitudes and behavior. Arguments have been couched in terms of conjecture or suggestive hypotheses. These qualifications reflect recognition that evidence to support such categorical assertions is regrettably absent.

Similar qualifications accompany the *structuring principle*, which holds that orientations acquired during childhood structure the later learning of specific issue beliefs. Inasmuch as socialization research began in the United States, a rapidly changing society, it followed that specific political beliefs could not be the concern of these studies. What may be an important issue today often becomes an irrelevancy tomorrow. The times change, issues change, and attitudes toward them change as well. Moreover, it was soon discovered that young children simply do not possess many issue beliefs at all. The goal, therefore, was to identify more integumented orientations which would be less likely to shift with the temper of the times. But the search for such basic orientations did not imply an indifference to specific political attitudes.⁵ Rather, it was assumed that the learning of these specific attitudes toward policies, leaders, and events is structured by basic political orientations acquired in childhood. This structuring proceeds through selective per-

cal orientations have been acquired by the time children leave the eighth grade, and that little change occurs thereafter. The strongest statement of this position is that of James C. Davies: Political socialization "begins at about the age of three and is basically completed by the age of thirteen." See "The Family's Role in Political Socialization," *Annals of the American Academy of Political and Social Science*, 361 (September, 1965), 11.

⁴ Cf. Herbert Hyman, *Political Socialization* (Glencoe, Ill.: Free Press, 1959); Greenstein, *Children and Politics*; Robert D. Hess and Judith V. Torney, *The Development of Political Attitudes in Children* (Chicago: Aldine, 1967); Richard E. Dawson and Kenneth Prewitt, *Political Socialization* (Boston: Little, Brown, 1969); Easton and Dennis, *Children in the Political System*.

⁵ "This is not to argue that specific political attitudes are not important; indeed they are much more reliable guides to political action than beliefs about politics in general," (Sidney Verba, "Conclusion: Comparative Political Culture," in *Political Culture and Political Development*, ed. Lucian W. Pye and Sidney Verba [Princeton, N.J.: Princeton University Press, 1965], p. 518).

ception, cue-giving, and similar processes.

Since the structuring principle is the central notion to be tested in this paper, it should be illustrated by a concrete example from the literature. Thus, Hyman notes that American children acquire party identification at a very early age, and that this identification persists over time for the vast majority of the population. Party identification thereafter serves as a surrogate parental socialization agent. Parents cannot possibly socialize their children to deal with the specific political issues which will confront them during their adulthood: such issues simply cannot be foreseen or anticipated. Parents can and do, however, transmit strong loyalty to a political party. And, since parties will continue to take stands on emergent political issues, the loyal Democrat or Republican can fall back on these issue stances as surrogate parental cues. According to this view, then, the learning of new issue beliefs is *structured* by a more basic political orientation (party identification) which the individual internalized during childhood, and which has persisted over time.⁶

Up to this point, the primacy and structuring principles have been explicated as important components in theoretical justifications for socialization research. The next step is to examine the structuring principle's place in allocative politics and system persistence models. In so doing, we will provide the rationale for our test and will circumscribe the research to which the test applies. First, however, it is necessary to clarify several concepts which are central to the argument.

Concepts: Distinctions Between Political Orientations and Political Attitudes

A major difficulty with the structuring principle concerns the distinction between independent and dependent variables, i.e., between *basic political orientations* and *specific political attitudes*. The socialization literature abounds with such distinctions. Yet differences between these phenomena remain unclear. Easton and Dennis, for instance, define political orientations as perceptions, affect, and evaluations directed toward political objects.⁷ These orientations are widely believed to "underlie" more

⁶ Hyman, pp. 56–57. Greenstein, Easton and Dennis, Hess and Torney, and others, make a similar linkage argument with particular reference to the system persistence model. Each reports that American school children hold an exceptionally benign image of the President. (For a representative example, see, Greenstein, pp. 27–54.)

⁷ Easton and Dennis, p. 5.

specific political attitudes.⁸ In addition, they are assumed to be comparatively "central" or more resistant to change than are specific attitudes.⁹

Perhaps the best known operational distinction between the two concepts is associated with the research of public opinion specialists. Prothro and Grigg contrast *general statements* such as "I believe in free speech," with *specific applications* of these statements, e.g., "Freedom doesn't give anyone the right to teach foreign ideas in our schools."¹⁰ Herbert McClosky has elaborated this sort of distinction into a three-fold typology: *personality*, *attitude*, and *issue orientation*.¹¹ The terms are distinguished by descending level of generality in the attitudinal object: for example, from need for personal autonomy, to affect for freedom of speech, to dislike of speaker-ban rules. This approach can be carried further through social psychologists' attempts to disentangle the terms *value* and *attitude*. One common strategy has been to define value as a broader attitude. *Opinion* is then introduced as a third term, defined as a still more specific manifestation of an attitude.¹²

For present purposes, we suggest positing a

⁸ For example, see Greenstein, pp. 28-31.

⁹ Herbert McClosky, "Personality and Attitude Correlates of Foreign Policy Orientation," in *Domestic Sources of Foreign Policy*, ed. James N. Rosenau (New York: Free Press, 1967), pp. 51-109.

¹⁰ James W. Prothro and Charles M. Grigg, "Fundamental Principles of Democracy: Bases of Agreement and Disagreement," *Journal of Politics*, 22 (May, 1960), 276-94. See also Herbert McClosky, "Consensus and Ideology in American Politics," *American Political Science Review*, 58 (June, 1964), 361-82.

¹¹ McClosky, "Personality and Attitude Correlates of Foreign Policy Orientation," pp. 56, 92-93.

¹² See William J. McGuire, "The Nature of Attitudes and Attitude Change," in *The Handbook of Social Psychology*, ed. Gardner Lindzey and Elliott Aronson, 2nd ed. Vol. III (Reading, Massachusetts: Addison-Wesley, 1969), pp. 151-52.

continuum by level of generality in the attitudinal object. In political socialization models, items at the more general end of this continuum will be called "orientations." Items closer to the specific pole will be designated "issue beliefs." This, in fact, seems to capture the intuitive meaning accorded such distinctions in socialization research. Thus, examples of issue beliefs relevant to the models involve objects such as political violence, racial situations, paying taxes, and aggressive national behaviors.¹³ Examples of orientations are presented in Chart 1. These orientations were identified on the basis of frequency of usage in contemporary socialization research.

We have divided the orientations into participation-demand and persistence-support categories. Anyone familiar with the literature will recognize this as the distinction between learning connected with citizen roles and learning connected with subject roles. They will also realize that the classification is somewhat arbitrary. Several orientations found under the allocative politics category seem equally at home in the system persistence category, and vice versa.¹⁴ This ambiguity reflects, in part, the vagueness with which socialization models have been presented in the literature. It also indicates that some orientations may simply be relevant to both outputs and persistence.

¹³ Dawson and Prewitt, *Political Socialization*, pp. 14, 86.

¹⁴ Party identification, for instance, obviously relates to the types of demands citizens make upon the policy process. But party identification may also be viewed as a support for the political system. This point has been argued by Philip E. Converse and Georges Dupeux: "Politicization of the Electorate in France and the United States," in Angus Campbell, Philip E. Converse, Warren Miller, Donald Stokes, *Elections and the Political Order* (New York: Wiley, 1966), pp. 269-91.

Chart 1. Orientations Frequently Employed in Political Socialization Research Among Children

Allocative Politics Model Demand-Related Orientations	System Persistence Model Support-Related Orientations
*1. Political party identification	*1. Orientations toward authority
*2. Sense of political efficacy	*2. Benevolence of authorities
*3. Sense of political competence	*3. Responsiveness of authorities
4. Duty to participate in politics	*4. Competence of authorities
*5. Orientations toward: electoral behavior; interest articulation; political decision making	*5. Affect for roles such as President, Policeman, and institutions such as Supreme Court
6. Political interest	*6. Trust in government
7. Qualities of the good citizen	*7. Political legitimacy
8. Perceptions of social groups	8. Patriotism
*9. Liberalism—Conservatism	*9. Political cynicism

* Orientations to be examined in the following analysis

Theory: Allocative Politics and System Persistence Models

Some investigators, most notably Easton and Dennis, have consciously attempted to relate socialization data to broader models of the political process. More often, however, research has been far from explicit concerning its theoretical relevance. Still, it seems fair to argue that most orientations have been introduced, at least implicitly, under one of two models currently found in the subfield. These are models of a quite abstract order. Their intent is to aid us in thinking about the political process, and the structure of our discipline. They depict causal relationships among classes of variables, in order to evaluate the theoretical relevance of these components. We do not propose to explore these models in their entirety, or even to refine them any more than is necessary for our immediate task.¹⁵ This task is to locate the structuring principle within their schemata.

The allocative politics model is centrally concerned with explaining policy outputs: who gets what, when, how, and why in democratic politics. Classes of variables within its schema are accorded theoretical relevance by showing that they contribute to these outcomes. With regard to mass publics, this promotes a focus upon political participation variables, or *demand*s in the political process.¹⁶ The allocative politics model generally assumes that the political orientations of mass publics are significant determinants of *political demands*, and, thereby, policy outputs. Under this assumption, it becomes the task of socialization research to explain the genesis of these adult political orientations.

Clearly, the assumption needs to be examined with considerable care. How might these orientations affect political demands, and, hence, policy outputs? First of all, we should note that the last link, that between demands and policy outputs, is beyond the scope of our

inquiry.¹⁷ We begin by considering the prior relationship between orientations and demands. Two alternative linkages are suggested in the literature.

The first is *behavioral constraint*. This attempts to connect orientations with a subclass of demand variables which do not involve issue beliefs. For instance, a voter with a strong Democratic party identification may find his own issue beliefs and the party's policies, no longer in tandem. Nevertheless, when he enters the polling booth, he "automatically" pulls the Democratic lever. Similar arguments have been advanced in explaining other aspects of political participation, including the amount of that participation itself. Although behavioral constraint has never been satisfactorily articulated, it does seem a potentially important linkage. And, to the extent that the linkage is demonstrable, it circumscribes the range of allocative politics research to which our test does not apply. It is depicted as segment (c) in Figure 1 below.

We focus instead only upon a second, and alternative, linkage between orientations and demands. We shall argue that the structuring principle is one segment in this chain. Thus, the larger chain relates orientations to demand variables *by way of intervening issue beliefs*. The demand variables include selected aspects of voting behavior and political participation which are mediated by issue beliefs. We wish to emphasize that the issue-belief linkage seems theoretically more important than behavioral constraint. If issue beliefs have no role in political participation and policy formation, then public opinion data would simply be irrelevant to explaining policy outputs for democratic systems. This emphasis on issue beliefs circumscribes the portion of the allocative politics model we propose to examine. The structuring principle is incorporated here as the link between orientations and issue beliefs, segment (a) in Figure 1.

A straightforward statement of the full-blown allocative politics model would begin with childhood learning. Childhood political orientations would persist into the adult years,

¹⁵ Models of this sort are, in effect, paradigms which orient research and circumscribe the scope of a discipline, or subfields within a discipline. In the present case, this involves definitions of politics and other equally complex issues. Although these models could benefit from careful reconstruction, such a task is clearly beyond the capabilities of the present essay. We have attempted to take the models as they appear in socialization research, and refine them only to the point necessary for diagrammatic representation and discussion in an intelligible manner.

¹⁶ David Easton defines "demands" as follows: "... an expression of opinion that an authoritative allocation with regard to a particular subject-matter should or should not be made by those responsible for doing so." (*A Systems Analysis of Political Life* [New York: Wiley, 1965], p. 38).

¹⁷ This is not to say that the relationship between demands and outputs is unimportant; only that its examination is beyond the capabilities of our immediate concerns. Like the relationship between supports and system persistence, this involves the difficult problem of relating individual to system level data. It also necessitates further clarification of the variables involved. See, Fred Greenstein, "A Note on the Ambiguity of 'Political Socialization,' Definitions, Criticisms and Strategies of Inquiry," *Journal of Politics*, 32 (November, 1970), 969-78.

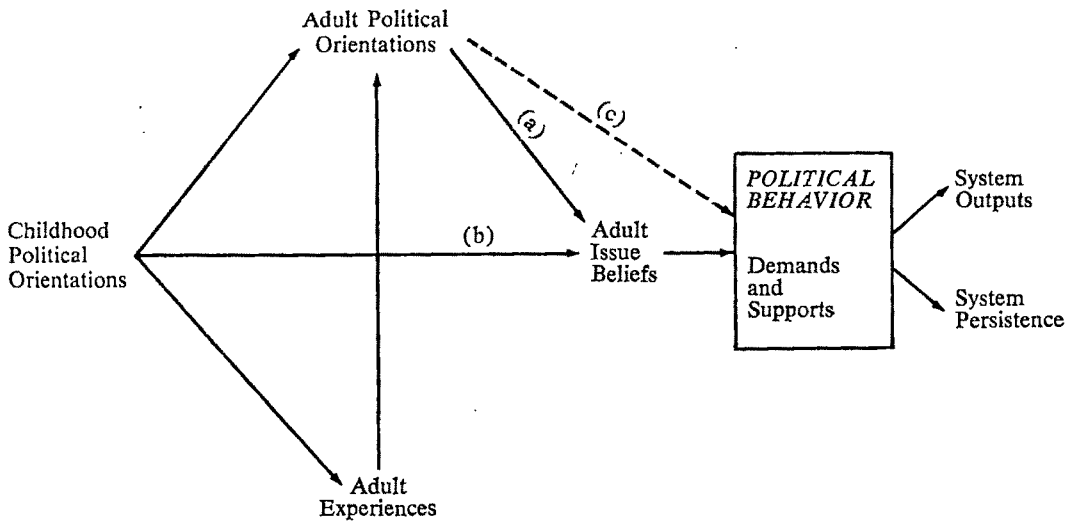


Figure 1. Generalized Political Socialization Model: The Structuring Principle

and thereby be directly related to specific issue beliefs. This simple model is especially familiar in treatments of political party identification: learned in childhood, the orientation persists into adulthood to structure the learning of issue preferences. But during the course of the life cycle, additional socialization experiences intervene so that the original orientations undergo some changes. Hence, it has been suggested that adult experiences and adult orientations be introduced as additional variables.¹⁸ This addition yields the more sophisticated model presented in Figure 1. For these reasons, the direct relationship between childhood political orientations and adult issue beliefs (segment b) is not taken very seriously. Before a citizen has even formulated his issue beliefs, his early childhood orientations, while relatively enduring, have eroded in the process of becoming adult orientations. The dominant flow of effects, then, proceeds through the relationship between adult orientations and issue beliefs (segment a). This is the structuring principle as incorporated within the allocative politics model.

The system persistence model addresses itself to quite different questions. Its focus is system stability and persistence: support rather than demand aspects of political processes.¹⁹ Questions of this nature lead socialization research

toward the genesis of mass attitudes which underlie support for the government, regime, and political community. We propose to argue that this model is, in one important respect, similar to the generalized political socialization model in Figure 1. It, too, incorporates the structuring principle. That is to say, the relationship between many support orientations and system stability is mediated by issue beliefs.²⁰

Easton and Dennis have differentiated support into two dimensions, specific and diffuse.²¹ Support must also be clearly differentiated into its attitudinal and behavioral manifestations. The attitudinal variables, such as trust in government, are depicted in our model as adult political orientations. By contrast, supportive acts are depicted under "Political Behavior." They include demonstrations in support of political institutions, or even serving as a member of a "silent majority" whose quiescence contributes to stability and persistence.

Specific support behaviors are defined as mercurial, *quid pro quo* acts. Like diffuse support, they are ultimately determined by favorable orientations toward institutions. But what

¹⁸ Dawson and Prewitt, pp. 204-05; Kenneth P. Langton, *Political Socialization* (New York: Oxford University Press, 1969), pp. 14-16.

¹⁹ While demands can also contribute to system stability and persistence, the model usually emphasizes support variables when it is discussed in the socialization literature.

²⁰ For example, citizens with strong affect for the presidency may support this institution, contributing to system stability by being uncritical of presidential actions in domestic and international affairs: Dawson and Prewitt, p. 59. More specifically, Americans are said to support their government by fighting its wars, paying its taxes, and obeying its laws: William C. Mitchell, *The American Polity* (New York: Free Press, 1962), p. 172.

²¹ Easton and Dennis, *Children in the Political System*, pp. 61-67. See also, David Easton, *A Framework for Political Analysis* (Englewood Cliffs, New Jersey: Prentice-Hall, 1965), pp. 124-26.

distinguishes specific support behavior is its contingency upon citizen approval of the institution's policies, i.e., upon intervening issue-belief variables. The theoretical relevance of specific support *orientations*, then, assumes their linkage with intervening policy preferences or issue beliefs. No doubt an orientation might seem to bypass issue beliefs, directly contributing to system persistence through behavior in support of institutions (segment c). But, even so, the orientation must always be accompanied by issue preferences favorable to the policies of these institutions (segment a). And these preferences contribute to system persistence. We say "always" because, *by definition*, where there is no issue-belief approval, there cannot be any specific support at all. This relationship seems adequately represented in Figure 1.

Diffuse support is more complex. It involves two alternative situations, only one of which incorporates the structuring principle. We shall first examine the situation that excludes the structuring principle. Attitudinally, diffuse support is defined as generalized feelings of trust and confidence in objects such as political institutions. This trust is akin to unconditional loyalty in the sense that it is given freely, regardless of perceived inconvenience. In short, the orientation produces behavioral support of institutions even when the citizen does not approve their policies. The citizen's issue beliefs are not, in this case, intervening variables between orientations and supportive behavior. The relationship is confined to segment (c) in our model. This delimits the range of system-persistence research to which our test does not apply.

But the holding of a diffuse support orientation does not necessarily imply indifference towards the institution's policies, or disagreement with them. Indeed, as a result of his diffuse

support orientation, the citizen is likely to agree with such policies more often than not. This brings us to the alternative diffuse support situation which does incorporate the structuring principle. When the citizen evinces agreement with the institution's policies, he is expressing an issue belief. Moreover, as with specific support, this issue belief contributes to system stability by expressing preferences supportive of institutional policies. It does so even if no further behavioral acts are involved. And, it does not seem unreasonable to suggest that diffuse support orientations explain, in part, these issue beliefs.

Chart 2 summarizes the range of research which falls within the purview of our inquiry, as well as research which remains beyond it. It would be desirable to be still more precise about the variables involved. But since they have not been sharply delineated in the socialization literature, such precision would require major efforts at theory construction that go beyond the limited concerns of our paper. We have restricted our review to stating the models as they are found in the literature and refining them only so far as necessary to locate the structuring principle within their schemata.

Research to which our inquiry applies is summarized in the upper two quadrants of Chart 2. The theoretical relevance of this research rests upon the structuring principle. All the political orientations considered below have, in part, been accorded importance by the structuring principle's link to demands and supports. The two lower quadrants, by contrast, identify research under the behavioral constraint aegis. Theoretical relevance in this research does not involve the structuring principle, and is therefore beyond the scope of our test. With reference to the allocative politics model, these orientations have been said to pro-

Chart 2. The Structuring Principle's Relationship to Theoretical Relevance in Political Socialization Models

	Allocative Politics Model (Demands)	System Persistence Model (Supports)
Issue Belief Links (Theoretical Relevance Dependent Upon The Structuring Principle)	<ol style="list-style-type: none"> 1. Voting behavior based upon issue preferences 2. Other forms of political participation based upon issue preferences 3. Issue beliefs which are perceived and acted upon by elites 	<ol style="list-style-type: none"> 1. Specific support 2. Diffuse support with policy agreement
Behavioral Constraint (Theoretical Relevance Not Dependent Upon The Structuring Principle)	<ol style="list-style-type: none"> 1. Voting behavior unrelated to issue preferences 2. Other forms of political participation unrelated to issue preferences 	<ol style="list-style-type: none"> 1. Diffuse support without policy agreement

mote actions such as "automatic" voting behavior, quite apart from any relationship with issue beliefs. From this viewpoint, orientations may be interpreted not as proximate determinants of issue beliefs, but as remote yet "central conditioners" of political action.²² Easton and Dennis come very close to limiting the importance of childhood political learning to these diffuse behavioral constraints.

If "behavioral constraint" were the only rationale for childhood socialization research, there would surely be no point to our study. But contemporary socialization models are far more complex than that. Most of this literature, under both the allocative politics and system persistence models, explicitly argues that childhood orientations persist into adult life and thereby "shape," "structure," and "determine" the later learning of issue beliefs.²³ Our research effort is designed to test these claims rather than the more elusive behavioral constraint argument. In the remainder of this paper, our references to allocative politics and system persistence models will concern only those aspects of theoretical relevance dependent upon the structuring principle.

Thus far, we have seen how the primacy and structuring principles provide theoretical justifications for investigating children's attitudes. We have further examined the manner in which the structuring principle has been incorporated in allocative politics and system persistence models. Now we are in a position to make a case for the reasonableness of our proposed test. The argument is as follows:

Children's orientations have been accorded importance in socialization research by virtue of the primacy and structuring principles. The primacy principle links them to adult political orientations. The structuring principle continues the chain by relating adult political orientations to issue beliefs. Issue beliefs are related to demands and supports, which, in turn, affect system outputs and persistence—the touchstones of theoretical relevance. This is a long

chain. It is one formulation of what Greenstein has described as the goal of relating childhood experience to adult behavior.²⁴ When we ask ourselves why children's orientations are an important topic, this is one notably widespread response found in socialization literature.

In order to demonstrate the political importance of children's orientations, each relationship in this chain must be established empirically. The links we have examined most closely are the primacy and structuring principles. We shall test the structuring principle below. This test will utilize data on *adult* orientations and issue beliefs. It may seem strange that we test the relevance of childhood orientations with data on adult orientations. Yet, it is of little immediate consequence whether or not these adult orientations have childhood antecedents, as asserted by the primacy principle. Similarly, it is of little immediate consequence whether demands and supports do indeed contribute to outputs and persistence. The point is that theoretical relevance for childhood data depends upon *every* link in the chain. We propose to test one such link: the structuring principle.

Data and Methodology

The structuring principle will be tested with data from two national cross-section samples: the Survey Research Center's (SRC) 1968 Election Study, and the University of North Carolina's Southeast Regional Survey (SERS). The SRC study interviewed a total of 1559 citizens of voting age both before and after Election Day, 1968. The SERS sample included 1504 adults interviewed in March, 1969.²⁵ Two data sets were employed for the following reasons. Where orientations and issue beliefs were identical in both studies, our intent was replication. In other cases, the two sets complemented one another, providing a wide range of items similar to those found in contemporary socialization research. We wish to emphasize that many items are either identical or highly similar to those used in socialization studies among children. Such studies seek to trace the genesis of adult orientations which are important in political behavior. They often adopt SRC type items as measures of these orientations. On the basis of the primacy and structuring assumptions, this procedure seems to make good theoretical sense.

²⁴ Fred I. Greenstein, *Children and Politics*, p. 163.

²⁵ The SERS sample is a national cross-section stratified by race, region, and size of place. This study was a University of North Carolina survey coordinated by Professors Robert G. Lehnen and Thad L. Beyle. It was supported by a Science Development Grant from the National Science Foundation.

²² An example from system persistence research will put this in sharper perspective. Easton and Dennis suggest that early affect for the President may be generalized into high trust toward governmental institutions. This is particularly important for adults who may be dissatisfied with the actions of incumbents such as Supreme Court Justices. At this point the early acquired trust in government can insulate the institution from citizen wrath. It prevents dissatisfaction with incumbents from "spilling over" into acts of hostility toward the institutions themselves. See David Easton and Robert D. Hess, "The Child's Political World," *Midwest Journal of Political Science*, 6 (August, 1962), 242-46.

²³ Easton and Dennis, p. 9; Hess and Torney, *Development of Political Attitudes in Children*, p. 7; Dawson and Prewitt, p. 43.

The appropriate test for orientation/issue-belief relationships is not as obvious as it may seem. Thus, it appears foolish to expect every orientation to predict every issue belief. There is little reason, for example, to expect measures of authority orientations, such as approval of policemen, to be associated with opinions about the oil depletion allowance. Converse suggests two types of links (sources of constraint) which might be relevant here: logical and psychological.²⁶ Logical constraints refer to logically necessary deductions from general principles to specific applications. A more common type of constraint is psychological. Here the sources of constraint do not follow logical rules, but respondents nevertheless perceive attitudes as logically bound together. For example: "We must incarcerate the Vietnamese population in order to liberate it."

Operationalizing these links is a difficult undertaking. Unless our purpose is to determine whether or not people are logical, it makes no sense to examine only logically related statements in the data. For the present test, we are interested in whether citizens display *any* orientation/issue-belief relationships, not whether they see them as we do. This applies equally to psychological constraints: We are interested in how *they* perceive the world, not whether their perception fits an ideal type of our own construction.

It is therefore necessary to proceed as follows. Rather than positing logical or psychological relationships *a priori*, we must begin with the model's dependent variable: issue beliefs. These are the key links to demands and supports in the political system. Fifteen issue beliefs were selected from the SRC 1968 data, and fourteen from the SERS 1969 data. They were chosen for their salience and importance in American politics during these years. Included are domestic policy items such as school prayers and the use of violence to suppress urban unrest, as well as foreign policy items such as alternatives for Vietnam. We reasoned that to produce a meaningful impact upon demands and supports, orientations found in political socialization research must be related to attitudes concerning these key policy issues of the day. Political orientations extracted for analysis include all those items in the SRC data (13) and in the SERS data (6) which were identical or highly similar to items from socialization projects. This selection includes those orientations generally regarded as *most central*: party iden-

tification, sense of political efficacy, trust in government, and legitimacy.²⁷

Findings

The structuring principle suggests that political orientations structure the learning of specific issue beliefs. In examining this statement, we proceed down a well-trodden path: relationships among elements of mass belief systems. Recent public opinion studies indicate that such elements may exhibit little constraint. Yet, these findings are by no means an adequate test of the structuring principle. For either they are restricted to very few orientations used in socialization research, or they focus upon relationships among issue beliefs alone. Thus, while suggestive, they provide only a backdrop for the present investigation.

Prothro and Grigg, in the first of these projects, found almost unanimous support for general statements expressing approval of abstract principles such as majority rule, minority rights, and democracy itself.²⁸ But this consensus disappeared on attitudes toward particular applications of the same principles, e.g., "Only the well informed should be allowed to vote." Similarly, McClosky discovered very high agreement on general statements approving free speech and opinion.²⁹ Again, this consensus contrasted with considerable disagreement over support for applications of free speech principles. Both findings can be interpreted as negative results for political orientation/issue-belief relationships—although the orientations include very few items found in socialization inquiries.

In the same vein, Converse reports quite low relationships among idea elements in the 1958 SRC election sample.³⁰ And while Luttbeg presents contrary results for two Oregon communities, his measure of association, as well as the items involved (local issues), make comparison with Converse's data quite difficult.³¹ In

²⁷ Several orientations in our analysis have been combined in scales by other studies (e.g., political efficacy). We did not employ such scales for the following reasons: 1) they were rarely used in the socialization studies whose theoretical relevance is under examination; and, 2) as summary measures, they would obscure information about different attitudinal objects (e.g., honesty vs. benevolence of public officials) which we wish to examine separately.

²⁸ Prothro and Grigg.

²⁹ McClosky, "Consensus and Ideology in American Politics."

³⁰ Converse, "The Nature of Belief Systems in Mass Publics," p. 228. See also Lloyd A. Free, and Hadley Cantril, *The Political Beliefs of Americans* (New Brunswick, N.J.: Rutgers U. Press, 1967), Chapter 3.

³¹ Norman R. Luttbeg, "The Structure of Beliefs Among Leaders and the Public," *Public Opinion Quarterly*, 32 (Fall, 1968), 401-404.

²⁶ Philip E. Converse, "The Nature of Belief Systems in Mass Publics," in *Ideology and Discontent*, ed. David E. Apter (New York: Free Press, 1964), pp. 109-13.

any event, both projects correlated issue beliefs with other issue beliefs, *not with political orientations*.³² Hence, their results do not bear directly on the model under consideration. One study which speaks more to the point, runs against our interpretation of the Prothro and Grigg, and McClosky findings. In a recent paper, McClosky reports that isolationism, an item akin to political orientations, is related to beliefs on foreign policy issues including immigration, foreign aid, and defense spending.³³ Clearly *some* political orientations are likely related to *some* issue beliefs. Still, the question that needs to be answered is: how are orientations used in political socialization research related to issue beliefs on key policy problems of the day? To this task we now turn.

Kendall's Tau_c is the measure of monotonic

³² Converse, (p. 229) did use one political orientation, political party identification, in his analysis. He found that party preference was "relatively unconnected to issue positions."

³³ McClosky, "Personality and Attitude Correlates of Foreign Policy Orientation," pp. 92-103.

association. This is a conservative measure of rank order correlation appropriate for the ordinal data under analysis. It should be noted that we are examining only direct, first-order relationships between orientations and issue beliefs. While it is possible that more complex associations are involved, we believe that contemporary socialization models have usually been restricted to these first-order relationships. Each orientation was correlated with each issue belief in the two data sets. The test was first executed using all categories (three or more) in the independent orientation variables. But the correlations were so low that a decision was taken to rerun the data using only high and low scores on these variables. Thus, the Tau_c test for each orientation/issue-belief pair is quite generous. It asks, what is the difference in issue belief position for those highest and those lowest on an orientation? If any relationship exists at all, it should show up between the high and low scorers. Results are presented in Tables 1 and 2.

Table 1. TAU-c Relationships Between Political Orientations and Issue Beliefs (SRC 1968 Data)

Political Orientations	Issue Beliefs															Row Mean
	Gov't Aid Health Care	Gov't Guarantee Good Living Standard	Gov't Promote School Integration	Approve Pace of Civil Rights Movement	School Prayers	Use of Violence to Suppress Urban Unrest	Evaluate Legal Protests	Evaluate Civil Disobedience (Laws)	Evaluate Civil Disobedience (Obstruction)	Approve Foreign Aid	Trade With Communist Countries	Vietnam Policy Alternatives	China Admission to UN	Overthrow Cuban Gov't	Vietnam Policy Scale	
Party Identification	.32*	.18*	.14#	.11*	.03	.17#	.04	.06	.07*	.01	.02	.03	.04	.01	.00	.08
Sense of Political Efficacy I	.01	.00#	.10*	.08	.01#	.04	.09*	.03	.00	.12*	.10*	.01	.03	.05	.04	.05
Sense of Political Efficacy II	.13*	.08#	.03	.04	.07#	.06#	.13*	.02	.05	.18*	.10#	.11*	.06	.00	.05	.07
Approval of Conservatives	.23*	.17#	.11	.18*	.11#	.19#	.12#	.10	.20*	.10#	.04	.18*	.13	.12	.13#	.14
Evaluation of Gov't Power	.27*	.25*	.27*	.19*	.03	.18*	.15*	.07*	.10*	.08#	.10#	.11*	.06	.02	.12#	.13
Sense of Gov't Complexity	.09#	.02#	.04#	.07#	.02	.09#	.22*	.12*	.14*	.04#	.08#	.01#	.18#	.14#	.05#	.09
Gov't Benevolence (Public Officials Care)	.07	.01	.10*	.06#	.02	.08#	.06	.01	.04	.15*	.04	.03#	.03	.01	.04#	.05
Approval of Policemen	.08	.10	.05	.14*	.11*	.17#	.20*	.15*	.20*	.03	.11#	.14*	.07	.06	.12#	.12
Gov't Responsiveness	.02	.05	.08	.07	.02	.07	.07	.01	.02	.19*	.02	.04#	.04	.01	.02#	.05
Political Cynicism I (Honesty of Authorities)	.08	.03	.11	.07	.05#	.14#	.02	.07	.02	.16*	.11	.12#	.03	.03	.12#	.08
Political Cynicism II (Competence of Authorities)	.00	.02	.09*	.06#	.04	.04	.06	.03	.02	.12*	.01	.00	.01	.03	.02	.04
Trust in Government	.07	.05	.17*	.09#	.03	.10	.04	.04	.00	.11*	.06	.01	.02	.01	.01	.05
Political Legitimacy	.04	.08	.19*	.17#	.00	.16#	.07	.01	.02	.19*	.11#	.01#	.02	.02	.10#	.08

Key: * = χ^2 significant at $p \leq .01$, no evidence of nonmonotonicity.

= χ^2 significant at $p \leq .01$, evidence of nonmonotonicity.

, All others not significant at $p \leq .01$.

Table 2. TAU-c Relationships Between Political Orientations and Issue Beliefs (SERS 1969 Data)

Political Orientations	Issue Beliefs														Row Mean
	Gov't Aid Health Care	Law Enforcement (Civil Liberties)	Approve Freedom of Religion	Approve Freedom of Speech	Gov't Promote School Integration	Progressive Taxes	Gov't Promote Good Living Standards	Use of Violence to Halt Riots	Gov't Promote Conservation	Evaluate Civil Disobedience	Gov't Restrict Car Manufacturers	Justification for Violence	Cooperation with Communist Countries	Approve Foreign Aid	
Party Identification	.22#	.06	.07	.04	.15	.04	.29#	.10	.02	.04#	.18*	.07	.00	.01	.09
Sense of Political Efficacy	.00	.11	.04	.12	.10	.01	.03	.11	.01	.03	.05	.01	.08	.22	.07
Power of Government	.15	.18#	.12#	.05	.02	.05	.15	.16	.05	.13	.16	.13#	.00	.04	.10
Political Legitimacy	.06#	.01	.03	.03	.12	.02	.06	.05	.00	.00	.07	.06	.02	.08	.04
Trust and Approval of City Officials	.05	.03	.07	.07	.17#	.01	.10	.02	.02	.01	.04	.10	.01	.10	.06
Political Cynicism (Competence of Authorities)	.02	.01	.02	.04	.02	.00	.02	.05	.01	.01	.02	.00	.02	.10	.02

Key: * = χ^2 significant at $p \leq .01$, no evidence of nonmonotonicity.

= χ^2 significant at $p \leq .01$, evidence of nonmonotonicity.

All others not significant at $p \leq .01$.

The Tau_c scores in Tables 1 and 2 are uniformly low. They afford only a marginal improvement over those from the first test, which used multiple categories in the independent variables. We conclude that *many of the most common orientations in socialization research are generally unrelated to attitudes toward the outstanding political issues of the day*. The only Tau_c exceeding .30 is that between POLITICAL PARTY IDENTIFICATION and GOVERNMENT AID HEALTH CARE (.32) in the SRC data. The correlation is still lower in the SERS sample.³⁴ This may indicate instability in the relationship; since the surveys were separated by only four months. If we adopt .20 as a marginal but perhaps acceptable relationship, then the pairs meeting this criterion include PARTY IDENTIFICATION—GOVERNMENT AID HEALTH CARE; PARTY IDENTIFICATION—GOVERNMENT PROMOTE GOOD LIVING STANDARD; and SENSE OF POLITICAL EFFICACY—

APPROVE FOREIGN AID in the SERS data, and the following pairs in the SRC data:

PARTY IDENTIFICATION—GOVERNMENT AID HEALTH CARE
 APPROVAL OF CONSERVATIVES—GOVERNMENT AID HEALTH CARE
 APPROVAL OF CONSERVATIVES—EVALUATE CIVIL DISOBEDIENCE (obstruction)
 EVALUATION OF GOVERNMENT POWER—GOVERNMENT AID HEALTH CARE
 EVALUATION OF GOVERNMENT POWER—GOVERNMENT GUARANTEE GOOD LIVING STANDARD
 EVALUATION OF GOVERNMENT POWER—GOVERNMENT PROMOTE SCHOOL INTEGRATION
 APPROVAL OF POLICEMEN—EVALUATE LEGAL PROTESTS
 APPROVAL OF POLICEMEN—EVALUATE CIVIL DISOBEDIENCE (obstruction)
 SENSE OF GOVERNMENT COMPLEXITY—EVALUATE LEGAL PROTESTS

It is noteworthy that most orientations in these pairs involve items which are predominantly affective or evaluative rather than cognitive. Furthermore, *post hoc*, many of these relationships appear perfectly "logical." But even a cursory examination of both tables reveals many equally "logical" relationships which failed to materialize. For instance, by similar reasoning it is nothing short of amazing that trust in government has no relationship whatever to Vietnam policy alternatives.³⁵ Those cit-

³⁴ The items are quite similar in both studies: (SRC) "Generally speaking do you usually think of yourself as a Republican, Democrat, Independent, or what?" (SERS) "Regardless of how you vote, when it comes to national politics, do you usually think of yourself as a Republican, Democrat, Independent, or what?"; (SRC) "Some people say the government in Washington ought to help people get doctors and hospital care at low cost; others say the government should not get into this. Have you been interested enough in this to favor one side over the other? (If yes) What is your position?" (SERS) "The Government ought to make sure that all people have good health care." (Agree—Disagree).

³⁵ The items were: "How much of the time do you think you can trust the government in Washington to do

izens who best learned to trust their government are apparently not the same ones who support the government's authorities in their most visible and perilous policy decisions (see Table 1). Similarly, legitimacy orientations are unrelated to opinions toward civil disobedience (Tables 1 and 2).³⁶ And political party identification has no impact on school prayer (Table 1) or progressive tax items (Table 2).³⁷ This list of counterexamples can easily be extended by perusing the tables. But such "for example" presentations are bound to be inconclusive, whether intended to support or refute the structuring assumption. What is more convincing is that only twelve relationships, and weak ones at that, appeared out of a possible 279. Obviously we cannot expect every orientation to be related to every issue belief. Still, the fact that the relationships are so few and so weak (even when comparing high versus low scorers), indicates to us that these orientations are marginal to demands and supports in the political system. This seems strong evidence against the structuring principle, and *ipso facto*, against theoretical relevance for the childhood socialization orientations involved.

How shall these findings be explained? Converse³⁸ hypothesizes that relationships between

what is right?" Those responding "Always" were contrasted with those responding "Some of the time" and "None of the time" (the middle category, "Most of the time," was dropped from the analysis) for their answers to the following questions: "Which of the following do you think we should now do in Vietnam? 1) pull out of Vietnam entirely, 2) keep our soldiers in Vietnam but try to end the fighting, 3) take a stronger stand even if it means invading North Vietnam."

³⁶ (SRC) "Would you say the government is pretty much run by a few big interests looking out for themselves or that it is run for the benefit of all the people?" "How about refusing to obey a law which one thinks is unjust, if the person feels so strongly about it that he is willing to go to jail rather than obey the law. Would you approve of a person doing that, disapprove, or would it depend on the circumstances?" (SERS) "The laws of this country treat all people fairly: Agree—Disagree."

³⁷ (SRC) "Some people think it is all right for the public schools to start each day with a prayer. Others feel that religion does not belong in the public schools but should be taken care of by the family and the church. Have you been interested enough in this to favor one side over the other? (If yes) Which do you think?" (SERS) "Taxes should be based on how much money people earn: Agree—Disagree."

³⁸ Philip E. Converse, "New Dimensions of Meaning for Cross-Section Sample Surveys in Politics," *International Social Science Journal*, 16 (1964), 19–34, esp. p. 27. It is for this reason that Robert Lane suggests we attempt to explain political issue beliefs by motivational variables (e.g., "Of what use to you is this opinion?") rather than by political orientations. *Political Thinking and Consciousness* (Chicago: Markham, 1969), p. 22. But this would be a largely complementary enterprise: a functional explanation (the role of the issue belief in

background variables and issue beliefs may be low simply because issue beliefs are quite unstable. He further suggests that, as we drop below the most educated ten per cent of the American populace, all sources of constraint decline rapidly. Constraint presupposes information about the issues. And level of information, in turn, is structured by the importance individuals attribute to these issues. From this viewpoint, it is interesting that a sophisticated belief system has been discovered for the race issue in an American subgroup: Detroit's black community. These respondents care deeply about racial matters. They therefore collect the information needed to develop a constrained belief system on this subject.³⁹ It may well be that the majority of American adults are insufficiently concerned with key political issues to seek much information on such topics. Perhaps it is even more unusual to find people who consciously attempt to link issue beliefs to their general principles or orientations. This suggests that many citizens have compartmentalized a symbolic set of political orientations having little relevance for their reactions to everyday political stimuli.

On the basis of this overview, let us re-examine Tables 1 and 2 for a closer look at two key theoretical notions from the socialization literature.

Political party identification is widely cited as the outstanding example of how political orientations serve as frames of reference for ordering issue beliefs. *The American Voter* demonstrated that in 1952 and 1956, party identification accounted for some variance in foreign and domestic policy views. Indeed, the structuring principle seems to have been introduced to socialization theory by projecting such party identification findings to cover basic political orientations in general. Thus, party identification is said to function as an organizing cognitive structure. It provides cues for evaluating newly emergent issues. Most issues involve matters which are complex and distant from the ordinary citizen's perspective. These ambiguities are resolved by adopting the party's po-

the self system) as opposed to a predictive or genetic explanation. For further discussion of linkage between orientations and issue beliefs, see Fred I. Greenstein, *Personality and Politics* (Chicago: Markham, 1969), pp. 124–25.

³⁹ Converse, "The Nature of Belief Systems in Mass Publics," p. 213; Joel D. Aberbach and Jack L. Walker, "The Meanings of Black Power: A Comparison of White and Black Interpretations of a Political Slogan," *American Political Science Review*, 64 (June, 1970), 380–82.

sition rather than struggling with each issue on its own merits.⁴⁰

This cue-giving attribute is conceptualized as *centrality*. In our data, orientations with the highest mean relationships to issue beliefs will be regarded as the most central. The relevant information is presented in row means for both tables. Here we see that party identification is not at all distinguished by unusually strong centrality. In fact, it is surpassed in the SRC data by approval of conservatives, evaluation of government power, sense of government complexity, and approval of policemen. The expectation of a relationship between party identification and issue beliefs assumes, of course, that parties have distinct issue positions—and that these positions are perceived by the individual. It would seem reasonable, then, to expect relationships only where these conditions are met. But regardless of such considerations, if party identification is related to only one out of fifteen leading issues of the day (two out of fourteen in the SERS data), then it is hardly possible to regard it as an organizing cognitive structure for such issues.

Other evidence supports this revised interpretation of party identification's centrality. Hess and Torney found that the item was independent of most political attitudes expressed by elementary school children. And this independence does not seem to wane during the adult years. In Converse's analysis of 1958 SRC election data, party identification was relatively unconnected to preferences on a selection of domestic and foreign issues. Similarly, rank order correlations reported by Converse and Dupeux yielded very low relationships in United States data, though somewhat stronger ones appeared for French voters who are party identifiers (less than half the total sample). Finally, Butler and Stokes argue that their British data provide remarkably little evidence that party identification serves to structure the voter's views on key political issues.⁴¹

It is an awkward point that the structuring principle's empirical basis appears to have been these putative cue-giving properties of party

identification. The original evidence was never fully convincing. For example, issue items in *The American Voter* were introduced through summary measures. Despite the demonstration of some relationships with party identification, the results were unclear regarding discrete links between party identification and separate issue beliefs. Yet these are the most theoretically appropriate links for assessing party identification's impact upon demands and supports in socialization models. In retrospect, the best evidence for party identification's "structuring" or "filtering" properties did not involve issue beliefs, but rather phenomena such as perception of candidates. Since the instability of issue beliefs is complemented by stability in party identification, it would indeed be paradoxical had the two turned out highly related.

Another widespread assertion deserving comment is the argument that the greater a political orientation's importance in adult behavior, the earlier it is learned during childhood. While "importance" has been associated with various denotations, it is often defined as impact upon demands and supports. Here we shall investigate the assertion by examining the portion of this impact mediated by issue beliefs. Following the allocative politics and system persistence models, this can be measured by an orientation's performance on the structuring principle, *i.e.*, by its relationship with beliefs concerning leading political issues of the day. Now, a major finding in socialization research is that benevolent images (affect and trust) of institutions and political leaders are learned by very young children; by contrast, political cynicism is known to be learned later, during adolescence or young adulthood. Thus, for adults, we would expect trust in government to be more important than political cynicism in the sense that it should be more strongly related to our issue beliefs.⁴² This is not borne out by the data, which show no substantial differences between the two types of items. In the SERS study (Table 2) trust and approval of city officials has a row mean of .06 compared with .02 for political cynicism.⁴³ And, in the SRC data

⁴⁰ Herbert H. Hyman, *Political Socialization*, pp. 46–47, 74–75; Angus Campbell, Philip E. Converse, Warren E. Miller, and Donald E. Stokes, *The American Voter* (New York: Wiley, 1960), pp. 128–36; 194 ff.

⁴¹ Hess and Torney, *Development of Political Attitudes in Children*, p. 211; Converse, "The Nature of Belief Systems in Mass Publics," pp. 228–29; Philip E. Converse and Georges Dupeux, "Politicization of the Electorate in France and the United States," in *Elections and the Political Order*, ed. Angus Campbell, et al. (New York: Wiley, 1966), p. 287; and David Butler and Donald Stokes, *Political Change in Britain* (London: St. Martins Press, 1969), pp. 193–94.

⁴² See Greenstein, *Children and Politics*, p. 155.

⁴³ Since city officials have little or no capacity to affect certain types of public policy, the following items were excluded from these calculations: approve freedom of religion, approve freedom of speech, government promote good living standards, civil disobedience, government restrict car manufacturers, cooperation with communist countries, and approve foreign aid. In addition, the political cynicism item refers to national authorities. Given findings on the generalization of authority attitudes, it does not seem too far fetched to use this item as an indicator of attitudes toward local authorities as well.

(Table 1), trust in government displays a row mean of .05 compared with .08 and .04 for the two political cynicism measures.

Our negative findings indicate to us that if the proposition that early learning determines adult importance has any force, it may be restricted to particular types of orientations. Once again, a sweeping generalization seems to have been extrapolated from party identification research. Party identification was found to be learned very early in life. It is also believed important in adulthood for "filtering" issue preferences. The finding and the belief are easily connected and generalized to other orientations. We have already argued that its "filtering" properties are exaggerated. But even were they substantial, the row means in Tables 1 and 2 indicate that party identification is not more strongly related to issue beliefs than are the political cynicism orientations which are learned later in life. The early learning adult importance proposition does not even apply to its party identification touchstone.

Because a large body of research regards nonpolitical personality factors as determinants of political attitudes and behavior, we determined to explore our findings further by analyzing selected personality factors and by performing a multiple regression test on the political orientations. It is assumed that childhood is a period of relatively little direct political learning. At the same time, childhood is characterized by politically relevant learning in the form of various personality factors.⁴⁴ These are expected to persist into adulthood and thereupon structure political attitudes and behavior.⁴⁵ During the 1930s and 1940s, however, numerous studies experienced very little success in attempting to relate personality factors to political beliefs.⁴⁶ The same personality characteristics, it was suggested, produced quite different issue preferences depending upon the issue preferences' functions in an individual's self system. We decided to perform another Tau_c analysis for the same issue beliefs, but this time using personality factors as independent variables. This test provides a baseline for further

interpretations of the political orientation Tau_c scores.

In Tables 3 and 4 we see that results are very similar to our earlier tests with political orientations: only nine relationships out of a possible 219 exceeded .20. A single relationship exceeded .30 in the SERS data (CONFORMISM—GOVERNMENT RESTRICT CAR MANUFACTURERS), and only the following two did so in the SRC data: APPROVE NEGROES—GOVERNMENT PROMOTE SCHOOL INTEGRATION, and APPROVE NEGROES—APPROVE PACE OF CIVIL RIGHTS MOVEMENT.⁴⁷ The last two issue items seem to be distinguished by references to concrete policy problems particularly salient for the orientations which predict them. Unfortunately, the data are inadequate to support further speculation along these lines. While it is clear that there are other equally "logical" relationships which did not materialize in the data, all the dependent variables involve attitudes toward key political issues of the day; and it is very difficult to see how the personality factors might "underlie" them in any causal sense. We wish to emphasize the similarity between these findings and those for political orientations employed in socialization research. Apparently such political orientations are no more related to issue beliefs than are the sort of personality factors for which negative results were discovered several decades ago.

One suggestion frequently found in socialization literature might go some distance toward explaining why our political orientation/issue-belief relationships are so weak. This is the argument that issue beliefs are determined by combinations of underlying orientations rather than by any single orientation. We attempted to examine this alternative through an exploratory multiple regression analysis between each issue belief and sets of five political orientations. Orientations used in the analysis are the five best predictors for each issue belief. They were entered in the order of their strength as single independent variables. The test is exploratory because data are ordinaly scaled and fail thereby to meet the interval scale assumptions of multiple regression analysis. Findings are summarized in Tables 5 and 6.

Results in Tables 5 and 6 show that the "complex substructure" explanation may not be

⁴⁴ It may appear that the concept "personality factors" as used in this context, does not reflect a distinct set of variables, much less any developed theory of personality and politics. Our choice of "personality factors," and their distinction vis-à-vis "political orientations," was dictated by past literature, availability of data, and the greater *prima facie* distance between these factors and issue beliefs than is the case with political orientations. This "distance" reflects the distinguishing characteristic of personality factors: independence of short term contextual variation.

⁴⁵ Easton and Dennis, pp. 77-79.

⁴⁶ Greenstein, *Personality and Politics*, p. 19.

⁴⁷ The personality items are: Conformist—"When talking with my friends, I should avoid topics that would lead to arguments." Approve Negroes—"There are many groups in America that try to get the government of the American people to see things more their way. We would like to get your feelings toward some of these groups: Negroes" (score on feeling thermometer).

Table 3. TAU-c Relationships Between Personality Factors and Issue Beliefs (SRC 1968 Data)

Personality Factors	Issues Beliefs															Row Mean
	Gov't Aid Health Care	Gov't Guarantee Good Living Standard	Gov't Promote School Integration	Approve Pace of Civil Rights Movement	School Prayers	Use of Violence to Suppress Urban Unrest	Evaluate Legal Protests	Evaluate Civil Disobedience (Laws)	Evaluate Civil Disobedience (Obstruction)	Approve Foreign Aid	Trade With Communist Countries	Vietnam Policy Alternatives	China Admission to UN	Overthrow Cuban Gov't	Vietnam Policy Scale	
Religious Fundamentalism	.02	.03	.01	.05	.18*	.01	.17*	.11*	.07*	.03	.16*	.08*	.21*	.04	.10#	.08
Approve Negroes	.09	.13*	.34*	.35*	.01	.25#	.16*	.09	.10	.19*	.18*	.09#	.11	.05	.15#	.15
Materialistic Optimism (Self)	.04	.00	.05	.01	.09#	.04	.13*	.00	.01	.10	.02	.04	.00	.05	.03	.04
Fatalism	.15*	.09*	.01	.01#	.07#	.03#	.02	.00	.05#	.10#	.01*	.13*	.07	.02	.09#	.06
Personal Efficacy	.11#	.08#	.02	.04	.04	.06#	.04	.04	.03	.00	.06	.03	.06	.01	.02	.04
Personal Security in Environment	.11#	.11*	.05	.00	.02	.08#	.02#	.08#	.09*	.05	.01	.09*	.05	.01	.11#	.06
Personal Competence	.12#	.13#	.05	.03	.07#	.07#	.04	.03	.03	.08#	.02	.11*	.07*	.03	.08*	.06
Personal Contentment	.15#	.18*	.03	.01	.08	.02	.08	.10#	.13#	.01	.04	.12	.03	.04	.09	.08
Trust in Others	.18*	.12#	.01	.02	.06#	.00#	.04	.01#	.08	.15*	.05	.08*	.05	.05	.02#	.06

Key: * = χ^2 significant at $p \leq .01$, no evidence of nonmonotonicity.

= χ^2 significant at $p \leq .01$, evidence of nonmonotonicity.

All others not significant at $p \leq .01$.

as sound as it seems by virtue of common sense alone. The highest R^2 (proportion of variance explained) obtained with these orientation combinations is .15 (see Table 5), and the results for most issue beliefs are considerably lower than that. On the basis of this provisional

test, our ability to predict issue beliefs does not seem substantially improved by using linear combinations of orientations. The original verdict still stands: Many orientations employed in socialization research appear unrelated to beliefs toward key political issues.

Table 4. TAU-c Relationships Between Personality Factors and Issue Beliefs (SERS 1969 Data)

Personality Factors	Issue Beliefs														Row Mean
	Gov't Aid Health Care	Law Enforcement (Civil Liberties)	Approve Freedom of Religion	Approve Freedom of Speech	Gov't Promote School Integration	Progressive Taxes	Gov't Promote Good Living Standards	Use of Violence to Halt Riots	Gov't Promote Conservation	Evaluate Civil Disobedience	Gov't Restrict Car Manufacturers	Justification for Violence	Cooperation with Communist Countries	Approve Foreign Aid	
Sense of Personal Efficacy	.15#	.05	.02	.07	.02	.08	.21*	.08#	.01	.06#	.16#	.02	.04	.04	.07
Responsiveness of Environment	.11	.00	.04	.04	.09#	.07	.08#	.02	.01	.12#	.14#	.04	.03	.02	.06
Fatalism	.14*	.01	.02	.06	.11*	.03	.16*	.00	.01	.14*	.10#	.01	.07	.05#	.07
Relativism	.04	.01	.06#	.01	.08	.01	.04	.03	.02	.10*	.06	.03	.02	.01	.04
Conformism	.16#	.03	.04	.04#	.06	.05	.28#	.06	.05	.11	.33#	.10	.01	.12#	.10
Personal Conservatism	.20#	.14	.05	.05	.12	.01	.21	.01	.02	.10	.12	.06	.00	.07	.08

Key: * = χ^2 significant at $p \leq .01$, no evidence of nonmonotonicity.

= χ^2 significant at $p \leq .01$, evidence of nonmonotonicity.

All others not significant at $p \leq .01$.

But theoretical relevance for much childhood socialization data depends on this relationship; otherwise the data lose their significance as anchors in a chain leading to demands and supports, and, ultimately, to system outputs and persistence. Our results therefore cast considerable doubt upon the importance of childhood data collected under allocative politics and system persistence models.⁴⁸

It may be a fundamental error to assume that average citizens possess such highly integrated belief systems in the first place. This reflects a presumption that issue beliefs must always be grounded (logically or otherwise) in "underlying" or "embedded" inner characteristics. But most policy issues are extraordinarily complex. They involve a changing kaleidoscope of situations and symbols. Even a philosopher would have difficulty deducing rigorous issue positions from a set of basic principles or orientations. And intellectual capacities are not the

⁴⁸ It should be noted that our results can be generalized only to a national population. It is, of course, still possible that some orientation/issue-belief relationships may be present for distinct subgroups within this population. Thus far, socialization research has usually been directed toward orientations learned by all members of a society. If research were focused upon orientations particularistic to subgroups, however, the structuring principle's validity would bear further investigation in these contexts.

Table 5. Multiple Regression Analysis: Political Orientations* and Issue Beliefs (SRC 1968 Data)

Issue Beliefs	Multiple R	R ²
Government aid health care	.35	.12
Government guarantee good living standard	.33	.11
Government promote school integration	.39	.15
Approve pace of civil rights movement	.32	.11
School prayers	.25	.06
Use of violence to suppress urban unrest	.30	.09
Evaluate legal protests	.28	.08
Evaluate civil disobedience (laws)	.18	.03
Evaluate civil disobedience (obstruction)	.18	.03
Approve foreign aid	.23	.09
Trade with communist countries	.16	.03
Vietnam policy alternatives	.26	.07
China admission to U.N.	.36	.13
Overthrow Cuban government	.13	.02
Vietnam policy scale	.16	.03

* The political orientations used in this analysis are the five best predictors for each issue belief (see Table 2). They were entered in the order of their strength as single independent variables.

Table 6. Multiple Regression Analysis: Political Orientations* and Issue Beliefs (SERS 1969 Data)

Issue Beliefs	Multiple R	R ²
Government aid health care	.31	.10
Law enforcement (civil liberties)	.20	.04
Approve freedom of religion	.10	.01
Approve freedom of speech	.19	.03
Government promote school integration	.22	.05
Progressive taxes	.15	.02
Government promote good living standards	.32	.10
Use of violence to halt riots	.16	.03
Government promote conservation	.12	.02
Evaluate civil disobedience	.17	.03
Government restrict car manufacturers	.32	.10
Justification for violence	.18	.03
Cooperation with communist countries	.19	.04
Approve foreign aid	.29	.08

* The political orientations used in this analysis are the five best predictors for each issue belief (see Table 3). They were entered in the order of their strength as single independent variables.

sole prerequisite either. From this viewpoint, what is more surprising than the lack of logical constraints in our data, is the absence of any constraints whatever. The construction of links or constraints requires considerable time and information on the part of the individual. It simply seems chimerical to expect the average citizen to muster the time and information required for serious application to the task.⁴⁹

Concluding Remarks

Political socialization research seeks to enrich our explanations of political outcomes. This it does by investigating attitudes which determine demands and supports in the political system. Obviously, these attitudes are those of adults; attitudes communicated to political elites. Why, then, we wondered, has socialization research focused almost entirely upon young children? One answer lies in the view that, (a) political orientations are learned in childhood and persist into the adult years; and, (b) they thereupon structure the learning of specific issue beliefs, the immediate determinates of demands and supports. These have remained untested assumptions; untested, in part, due to the absence of longitudinal data on childhood orientations. But the long chain linking childhood learning with political outcomes

⁴⁹ McClosky, "Consensus and Ideology in American Politics," p. 374.

depends upon the second assumption as much as upon the first. We have characterized this second assumption as the structuring principle. It has been subjected to systematic examination and test in the present paper. Greenstein writes that a long-run goal for socialization research is to relate childhood experience to adult behavior.⁵⁰ Our analysis has attempted a modest step in this direction.

The exploratory character of most socialization inquiries may lead readers to wonder whether we have constructed a "straw man" here. In particular, it is easy to imagine objections to our account of the structuring principle's role in political socialization models. Such objections are all the more likely because relationships between orientations and issue beliefs have never been stated with satisfactory precision in the literature. The chief difficulty involves the confounding of genetic and predictive explanation. Genetic explanation attempts to set out the sequence of events through which adult issue beliefs have been evolved. Childhood orientations are treated as the first major stages in this sequence. We have not been concerned with questioning such theoretical assertions or their methodology. Indeed, we wish to disentangle this enterprise from the task of predictive explanation in the literature.⁵¹

Predictive explanation specifies antecedent phenomena which are necessary and/or sufficient to generate adult issue beliefs. We have argued that much socialization literature identifies *adult* orientations as the phenomena which explain issue beliefs in this sense. And, in considering relationships between *adult* orientations and issue beliefs, we are no longer speaking of distant genetic antecedents, but rather of orientations which are said to "shape," "structure," and "determine" issue beliefs. The genetic and predictive enterprises are clearly complementary in socialization research. Yet, the distinction between them is often blurred by metaphorical statements characterizing *both* childhood and adult orientations as "frames of reference" or "political eyeglasses" through which political issues are seen. We believe that, in practice, when these metaphors describe adult orientations, they are interpreted in predictive terms.

Our findings can be summarized as follows:

⁵⁰ Greenstein, *Children and Politics*, p. 163.

⁵¹ See: Michael Scriven, "Explanation and Prediction in Evolutionary Theory," *Science*, 130 (August 28, 1959), 477-82, reprinted in *The Nature and Scope of Social Science*, ed. Leonard I. Krimerman (New York: Appleton-Century-Crofts, 1969), pp. 117-25; Robert Brown, *Explanation in Social Science* (Chicago: Aldine, 1963); and, William Dray, *Laws and Explanation in History* (London: Oxford University Press, 1957).

(1) Many of the most familiar socialization orientations are generally unrelated to attitudes toward outstanding political issues of the day. Even when orientations were subjected to a multivariate analysis their explanatory power did not improve. Such an absence of constraint in mass belief systems is certainly not a surprising finding. Similar results have been repeatedly reported in public opinion research, albeit most such inquiries examine relationships among issue beliefs alone. The point of our test has been to elaborate these public opinion findings and to interpret their implications for socialization models. (2) The structuring principle appears to have been generalized from the belief that party identification provides cues for evaluating political issues. But we found that party identification is not at all distinguished in this regard. Other political orientations are more strongly related to issue beliefs—though all in quite limited terms. (3) Evidence has also been presented contradicting the widespread assumption that the more important an orientation is in adult behavior, the earlier it is learned during childhood. (4) Finally, political orientations were found to be no more related to issue beliefs than are the sort of personality factors for which negative results have been familiar for some time.

Yet theoretical relevance in allocative politics and system persistence models appears closely bound up with the structuring principle's validity: otherwise childhood orientations lose their significance as the origin of a chain leading to demands and supports for the political system. Our findings cast considerable doubt upon the structuring principle, and, thereby, upon the theoretical relevance of much childhood research under allocative politics and system persistence models.

These models have never been fully articulated in the socialization literature. Indeed, with a few notable exceptions, a lack of careful attention to theory seems to underlie the difficulties unearthed in this paper. This is not intended as an unreservedly critical commentary. When next to nothing is known about an empirical phenomenon, exploratory data collection must be accorded the highest priority. It is only when such data have been assimilated that we can enjoy the luxury of theoretical niceties. For political socialization, we believe that the time has come when we are sufficiently familiar with the terrain to begin mapping it according to explicit theoretical concerns.

What, then, are the implications of our findings for directions in current research? First of all, these findings have no bearing on research which follows the behavioral constraint linkage

in allocative politics and system persistence models. Yet, with the exception of the relationship between party identification and voting choice, this relationship remains to be empirically established. Our difficulty in characterizing the linkage underlines the need for further clarification. Such clarification is a prerequisite for tests similar to the one applied to the structuring principle in this paper. Regarding data collected under the structuring principle's aegis, we suggest the following: If this principle is to be taken seriously as a research guide, and, if we are therefore to continue presenting SRC-type questions to young children, then the burden is now upon the principle's proponents to: (a) specify exactly what sorts of orientations are likely to be related to what sorts of issue beliefs, and why; and, (b) demonstrate empirically that these relationships actually exist. The point holds equally for behavioral constraint and structuring principle versions of the models: *An orientation's importance in adult behavior must be demonstrated before we can be sure that its childhood genesis is of interest to political science.*

Wherever our ultimate concern is to explain systemic demands and supports, we also reiterate the need, advanced by several researchers, to examine adult socialization experiences which may mediate, or even replace, the role of childhood orientations in learning issue beliefs. Although everyone acknowledges that socialization continues throughout the life cycle, there has, in fact, been very little empirical attention to adult political socialization. This does not at all entail a turning away from the current focus upon children. Some of their learning may yet be found to contribute to explanations of demands and supports. And even should this contribution prove limited, we believe that the rich data already collected provide a basis for formulating additional theoretical perspectives.

For instance, it is now clear that very young children are much more affected by political symbols and stimuli than had been previously thought. But these data have been projected primarily as contributions to explaining system inputs: demands and supports. Here we see reflected the discipline's predominant concern with input processes. At the same time, there is increasing acknowledgement that this has been overdone to the extent that output phenomena are neglected as independent variables. This criticism is as applicable in political socialization as in other subfields of the discipline. The question is, How can system outputs be related to political socialization? It is necessary to stress that output phenomena are not restricted

to governmental expenditures. Statutory and symbolic acts are also relevant. From this perspective, an output approach to political socialization might well investigate how the political system affects selected aspects of its citizens' psychological lives.⁵² How does it affect their experience of the political and social environment? Thanks to available data, we already know that this influence begins during early childhood. And, we are aware of some of the mechanisms involved. But instead of treating children's attitudes as independent variables, this alternative would utilize them as dependent, as phenomena to be explained. Such an approach, we feel, deserves our attention, not in place of the input focus, but in addition to it.

Methodological Appendix

Kendall's Tau_c was used as the measure of association between basic political orientations and specific issue beliefs. It was chosen as appropriate for the ordinal nature of the data, as well as for its characteristic of correcting to achieve unity when the number of rows and columns are unequal. But as a measure of monotonic association, Tau_c cannot distinguish between situations of no association on the one hand, and nonmonotonic, or crypto-curvilinear associations on the other. Indeed, no satisfactory analogue of curvilinearity tests exists for ordered data. Still, curvilinear functions are possible with ordered as well as continuous variables. By way of assessing the possibility that such relationships were present, but not reflected in the Tau coefficient, we undertook tests of significance and inspections of the cross-tabulations.

χ^2 was used to determine departure from expected cell frequencies. This statistic does not depend upon order properties of the variables in question. In addition, it provides us with a "discount" of apparent nonmonotonicity when this is based on relatively small expected and observed frequencies. We chose p equal to .01 as the level of significance. That is to say, cross-tabulations with a probability of the null-hypothesis being erroneously rejected less than or equal to 1 in 100 were considered evidence of some relationship. These tables were then examined further in an attempt to assess departure from monotonicity.

Examination of Table 1 shows that 101 of the 195 bivariate relationships (52 per cent) were found insignificant by our criterion. Of

⁵² Jack Dennis has noted that thus far this piece of the puzzle remains implicit in socialization research: "Major Problems of Political Socialization Research," *Midwest Journal of Political Science*, 12 (February, 1968), 109-110.

the 75 significant mis-fits, 48 (25 per cent of the 195 bivariate relationships) displayed evidence of some departure from monotonicity. Such departure was indicated by examining the column proportions in one of the two rows of our tables. In all cases, our political orientations (the independent variables) were two-valued. This means that categories on the dependent variable were used as one coordinate. And, proportions of one of the responses on the orientation variables were then used to plot a histogram. If a noticeable nonmonotonic pattern existed in that plot, and, if the departure from expected frequencies yielded a significant χ^2 , the variable was included among the instances of nonmonotonicity.

Similar analyses were performed on Tables 2, 3 and 4. Considering all these tables together, 314 of 498 bivariate relationships (63 per cent) were found insignificant at the .01 level. With the remaining 184 significant relationships, 106 (58 per cent) were found to be evidently nonmonotonic.

Although these findings might suggest some modification of our conclusions, the magnitude of such modification is impossible to ascertain precisely. It is for this reason that we decided to present these comments as a methodological appendix, rather than in the main body of the paper. Actually, we believe it unlikely that our conclusions warrant modification. There seems to be little patterning in the occurrence of ap-

parent nonmonotonicity and significant values of χ^2 . It would be difficult to argue that any orientation/issue-belief relationships are manifest, whether monotonic or nonmonotonic, at least at the first-order level.

We have not gone beyond the first-order level for two reasons. One is that our inquiry's purpose is to examine direct linkages between orientations and issue beliefs. Even where the socialization literature has not unequivocally asserted a direct linkage, this has been implicit in analyses concerned with first-order relationships. Then, too, the character of our first-order relationships, where nearly two-thirds are not significant, and better than half the remainder apparently nonmonotonic, makes the task of inductively discovering higher order relationships both epistemologically questionable and difficult.

If we intend to search further for relationships between orientations and issue beliefs, the first task is to improve our measurement instruments. It seems likely that some orientation/issue-belief relationships will be nonlinear. Yet, the ordinal nature of our attitudinal data compels us to apply monotonic models to the anticipated relationships. If interval measurement, or a convincing approximation, could be achieved, then we would be able to test for curvilinearity, and even induce function rules of higher order from our data.

Urbanization and Political Participation: The Case of Japan*

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The contribution of place of residence as a factor influencing mass political behavior has been ambiguous. Beginning with Tingsten's work on voting turnout, research has shown that urban residence can alternatively be a positive or negative correlate of political involvement and participation, or in some cases it may have no impact at all. Some of the apparent contradictions in research findings are obviously the result of noncomparabilities in units and levels of analysis. These problems notwithstanding, the meaning and impact of urban and rural residence still vary among political systems. Since urban growth is a basic fact of both observed modernization processes and life in the modern period itself, satisfactory specification of the linkage between place of residence and political behavior in different kinds of systems is obviously desirable. It should tell us something about the specific effects of urban-rural variations in community life, as well as adding to more general knowledge of the variables that affect political behavior.¹

Three distinguishable trends can be seen in the findings of various kinds of research in which urban-rural patterns in political partici-

pation or involvement are reported. According to one type of result, urban residence has been associated with higher levels of psychological involvement in politics, as in the United States and Germany.² Higher voting turnouts are also found in the urban parts of some countries, and somewhat analogously, turnout levels are higher in the urbanized countries of the contemporary world than in nations where the rural sector is relatively more important.³

There are also examples of an opposite tendency, particularly for voting turnout. In the prewar period, going out to vote was more common in the rural parts of Switzerland than it was in the cities.⁴ Comparable patterns of higher rural than urban turnouts are found for Germany, France, Britain and Japan in the postwar era.⁵ Finally, both political involve-

² Angus Campbell, Philip E. Converse, Warren E. Miller and Donald E. Stokes, *The American Voter* (New York: Wiley, 1960), p. 412; and Edward N. Muller, "Cross-National Dimensions of Political Competence," *The American Political Science Review*, 64 (September, 1970), 792-809, especially 797. Actually, Muller's findings on the United States differed with those of *The American Voter* but were based on different specific question items. The Campbell *et al.* finding, however, was supported by Muller's evidence on differences between rural areas and intermediate size cities.

Although evidence from other places is contradictory, as will be seen, earlier survey evidence on sectoral tendencies in voting turnout in the United States from the Survey Research Center parallels their findings on involvement. See Angus Campbell, Gerald Gurin, and Warren Miller, *The Voter Decides* (Evanston: Row, Peterson, 1954), pp. 128-130.

³ Herbert Tingsten, *Political Behavior: Studies in Election Statistics* (London: P. S. King, 1937), pp. 211-214, and Daniel Lerner, *The Passing of Traditional Society: Modernizing the Middle East* (Glencoe: The Free Press, 1958), p. 63. For additional information where nation states are the unit of analysis see Bruce M. Russett, Hayward R. Alker, Jr., Karl W. Deutsch, Harold D. Lasswell, *World Handbook of Political and Social Indicators* (New Haven: Yale University Press, 1964), pp. 279-80.

⁴ Tingsten, *Political Behavior*, p. 214.

⁵ For Britain see *Gallup Analysis of the Election 1966*, pamphlet (London: The Daily Telegraph, 1966), p. 27, and Jean and Monica Charlot, "Politisation et Depolitisation en Grande Bretagne," *Revue Française de Science Politique*, 11 (September, 1961), 609-41, especially 634. German trends are reported by Erwin Faul in *Wahlen und Wähler in Westdeutschland*, ed. Dolf Stern-

* Revised draft of paper presented at the annual meeting of the Association of Asian Studies, Washington, D.C., March 1971. Special appreciation is due colleagues Richard Hofstetter and John Kessel for their generous advice. The writer is also indebted to the staff of the Polimetrics Laboratory, Department of Political Science, The Ohio State University for help in data management.

¹ It is important to note from the very beginning that I am assuming that differences in *community* life or involvement between the urban and rural sectors, or between places of different magnitude of population, are of potential importance to political participation in some circumstances. What these circumstances may be, how they relate to the effects of urban-rural differences in other factors such as political competition or social structure—for example, the distribution of educational attainment—and what the effects may be for different kinds of attitudes and behaviors in one nation are the topics to be discussed.

The urban-rural disparities in turnout may also contribute to a particular configuration of party support. This has probably been the situation in postwar Japan, where it is frequently assumed that high rural turnouts favor Conservatives in specific relevant constituencies, a practice which is linked with maintenance of a dominant party system.

ment and participation in politics beyond voting itself are independent of the effects of place of residence in some important cases, at least where these attitudes or activities relate to national rather than local politics.⁶

Differences in measures of the dependent variable presumably enhance the appearance of contradiction in these findings. Psychological involvement in politics, and also the more active or demanding forms of participation in some cases, correlate differently with place of residence in France, Britain, Germany and Japan than does voting turnout within each of these systems. Nevertheless, there are also still some obvious differences between countries when the same kinds of behaviors are measured.

Differences in levels of analysis among different bodies of research can also be a complicating factor. Voting participation can obviously correlate positively with urbanization levels across countries where measurements are made at the aggregate system level, at the same time that analyses of internal sectoral patterns in some urbanized countries may show higher

turnouts in the rural districts than in the cities. This familiar problem further helps explain some of the misleading interpretations that have been given to urban residence as a determinant of political behavior.

Unfortunately, we have no comprehensive theory about the effects of living in the city or the countryside or in small communities. Modernization theory has been tested with survey data on the political behavior of persons from urban and rural residential situations.⁷ There was some legitimacy in doing this, even though modernization theory dealt with global processes of change that were believed to affect both urban and rural residents.⁸ The original ideas about the effects of massive social change did imply attitudinal and behavioral change at the individual level, and hence it might be assumed that people in social circumstances most affected by modernization would be influenced the greatest.

Accordingly, if we extrapolate predictions about the effects of place of residence from modernization theory, we would anticipate higher levels of political involvement and participation in urban districts. Following Lerner, this increased activity would be the result of higher educational levels and enhanced exposure to mass media more typically characteristic of the urban sector. Or treating the Deutsch interpretations in a similar fashion, we might expect urban residents to have greater awareness of the instrumental potential of political affairs.⁹

Milbrath's center-periphery concept is also applicable to analysis of urban-rural differ-

berger, Friedrich Erbe, Peter Molt, Erwin Faul, (Villingen: Ring, 1960), p. 156 and "Das Wahlverhalten verschiedener Bevölkerungsgruppen bei der Bundestagswahl 1965," *Wirtschaft und Statistik* 3 (1966), 165-72. French patterns are discussed in Alain Lancelot, *L'Abstentionnisme Electorale en France* (Paris: Armond Colin, 1968), pp. 195-97, Mark Kesselman, "French Local Politics: A Statistical Examination of Grass Roots Consensus," *American Political Science Review*, 60 (December, 1966), 963-73, and Sidney Tarrow, "The Urban-Rural Cleavage in Involvement: The Case of France," *American Political Science Review*, 65 (June, 1971), 341-57, especially 344-46. Japanese voting turnout is discussed in detail in Jun'ichi Kyogoku and Nobutaka Ike, "Urban-Rural Differences in Voting Behavior in Postwar Japan," *Economic Development and Cultural Change*, 9 (October, 1960), Part II, 167-85, especially 170-72.

Comparable tendencies are also found in some American states today, or at earlier points in time. The main sources are James A. Robinson and William Standing, "Some Correlates of Voter Participation: The Case of Indiana," *Journal of Politics*, 22 (February, 1960), 99-106, V. O. Key, Jr., *Southern Politics in State and Nation* (New York: Alfred Knopf, 1950), pp. 510-13, and Walter Dean Burnham, "The Changing Shape of the American Political Universe," *American Political Science Review*, 59 (March 1965), 7-28, especially 15-16.

⁶ Norman H. Nie, G. Bingham Powell, Jr., and Kenneth Prewitt, "Social Structure and Political Participation: Developmental Relationships, Part I," *American Political Science Review*, 63 (June, 1969), 361-78, especially 368. The authors conclude that differences in social status and organizational life are the main factors in explaining variations in national participation levels outside of voting.

See also Muller, "Cross-National Dimensions," p. 797.

⁷ Nie, Powell and Prewitt, "Social Structure and Political Participation" is the main example of research of this kind.

⁸ For the core writings in this area see Karl Deutsch, "Social Mobilization and Political Development," *American Political Science Review*, 55 (September, 1961), 493-515, and Daniel Lerner, *Passing of Traditional Society*, especially Chap. 2.

Deutsch's analysis treats the various components of the social modernization process as a cluster of inter-related forces and looks for their combined effects on political behavior. Lerner presents a sequential model wherein urbanization plays a special function in the early phases of modernization. The discussion that follows addresses itself primarily to the question of the utilizability of the participation elements of developmental models. I am not concerned here with the extensions of the discussion to cover the more elaborate models of democratic performance that have been developed from the Deutsch, Lerner, and other modernization analyses.

⁹ Actually, rural residents might develop an awareness of instrumental stakes through the effects of the commercialization of agriculture. See Deutsch, "Social Mobilization," p. 494.

ences.¹⁰ By virtue of their social circumstances, certain kinds of persons including urban dwellers are believed to be closer to critical decision-making centers and inter-personal political communications links, and this closeness is reflected in their higher rates of political involvement and participation.

Isolated hypotheses about the effects of urban-rural residence can also be found in the behavioral literature at various points. Urban-rural differences in population mobility have been seen as underlying causes of sectoral tendencies in turnout in some analyses in Europe. The lower turnouts found in large cities in several major European countries have thus been attributed by Tingsten and other writers to the less stable residential patterns of urban dwellers which reduces their stakes in election outcomes.¹¹

The physical aspects of urban and rural life are also important according to some studies of political behavior. Tingsten observes that voting turnout may be facilitated in some societies by the compactness of the urban environment.¹² Lancelot also cites the ease of voting in the parts of rural France where people live in compact villages, in contrast with the difficulties in going out to vote in mountainous districts or in less compact rural residential situations.¹³ Somewhat parallel interpretations have been found in the American literature, which places greater stress on the social isolation resulting from the physical isolation of rural life.

It can be seen from this brief review of the relevant theoretical positions that the existing alternative explanations are applicable to both urban and rural dominance in involvement and participation rates. These theoretical anomalies reflect in part the real probability—largely overlooked in comparative research—that place of residence has different meanings in different national and regional settings.¹⁴ It is evident,

for example, that life in the compact rural communities found in some places in Europe is very different from that found in the more isolated rural settings common in many parts of North America. The extent of industrialization in the rural periphery also obviously varies between countries as well as within nations. Compare the limited extent of industrial penetration in many rural communes in France, for example, with the much higher levels found in comparable units in many parts of Germany. Such differences can also produce variations in the intensity of organizational life among different residential situations in different countries.¹⁵

Clarifying the underlying differences in the implications of urban and rural residence in different places could pave the way to identifying basic cross-national uniformities in the roots of political performance. Such exploration is made all the more important by the recent discovery of cross-national uniformities in the effects of social status and organizational memberships on political participation. If the remaining disparities in sectoral participation and involvement between countries can be linked with cross-national differences in, say, the distributions of community or organizational involvement between the cities and the countryside, the present ambiguity could be reduced.

My objective in this article is to explain some tendencies in urban-rural political participation in Japan. In doing this, I will refer back to the different theoretical formulations we have just reviewed, but in a less formal and structured way than is sometimes the case in political science research. Rather than taking various hypotheses and testing them against the evidence and leaving matters there, I have endeavored in a few instances to suggest possible revisions in theory on the basis of my examination of the

fusion inherent in many approaches to the question of urban-rural differences, see Franz Urban Pappi's criticisms of the urban-rural continuum concept in *Wahlverhalten und politische Kultur* (Meisenheim am Glan: Anton Hain, 1970), pp. 90–101.

Lancelot's distinction between the effects of compact and dispersed settlement patterns in France is a case in point. Stein Rokkan and Henry Valen, in "The Mobilization of the Periphery: Data on Turnout, Party Membership and Candidate Recruitment in Norway," *Acta Sociologica*, 6 (1962), fasc. 1–2, pp. 111–158, also introduce refinements into an analysis of urban-rural participation patterns, and, among other things, pay special attention to the importance of intrafamilial role differences in the two sectors.

¹⁵For example, higher levels of group memberships are found in small communities in Germany, according to "Die Organisationszugehörigkeit in Westdeutschland," *DIVO Pressdienst*, August 1, 1964, p. 11.

¹⁰ Lester W. Milbrath, *Political Participation* (Chicago: Rand McNally, 1965), pp. 110–114 and 128–30. Robert Lane also uses the concept of centrality in *Political Life: Why People Get Involved in Politics* (Glencoe, Illinois: The Free Press, 1959), p. 196.

¹¹ See Tingsten, *Political Behavior*, p. 214. Actually, the interpretation is attributed to a study of turnout in the Zurich area of Switzerland.

¹² Tingsten, *Political Behavior*, p. 213.

¹³ Lancelot, *L'Abstentionnisme Electorale en France*, pp. 198 and 200.

¹⁴ Jurg Steiner's comments are relevant at this point: "The hypotheses must be complex, and in addition to the number of inhabitants other factors must be included, such as form of settlement, social structure and transportation situation." See *Bürger und Politik* (Meisenheim am Glan: Anton Hain, 1969), p. 150. For a brilliant consideration of the theoretical con-

Table 1. Election Turnout, by Place of Residence

	Urban	Rural	All
Local Elections:			
1955 Mayoralty ^a	84%	92%	86%
1955 Assembly ^a	85	92	—
1959 Mayoralty ^b	86	91	88
1959 Assembly ^b	86	93	—
1963 Mayoralty ^c	82	90	86
1963 Assembly ^c	82	92	—
Prefectural Elections:			
1955 Gubernatorial ^a	70	83	75
1955 Assembly ^a	72	83	77
1959 Gubernatorial ^b	76	84	78
1959 Assembly ^b	76	85	80
1963 Gubernatorial ^c	72	82	75
1963 Assembly ^c	74	84	77
House of Representatives Elections: ^d			
1952	66	80	74
1955	72	81	76
1958	74	81	77
1960	70	80	74
1963	67	79	71

^a Jichichō Senkyobu, *Chihō Senkyo Kekka Shirabe*, 1956, p. 13. National means are omitted for local assembly elections: city ward assembly elections, which are not strictly comparable with assembly elections elsewhere, are included in the official calculations, and their especially low turnout levels in turn considerably depress the national mean. The category "urban" in the above entries does not include major cities; turnout in the large cities is typically much lower than is the case in the provincial cities for which figures are shown here.

^b Jichichō Senkyokyo, *Chihō Senkyo Kekka Shirabe*, 1959, p. 13.

^c Jichishō Senkyokyo, *Chihō Senkyo Kekka Shirabe*, 1963, p. 8.

^d —, *Shūgiin Giin Sōsenkyo Saikō Saibansho Saibankan Kokumin Shinsa*, 1964, p. 4.

Japanese findings. I have also chosen eclectically from the ideas developed by other scholars when I saw that particular hypotheses were relevant to the Japanese evidence—in other words, when the evidence alternatively confirmed or rejected one of these propositions. The resulting interaction between theory and data is far less orderly than might be desired. In part this reflects my own reaction to a situation in which theory itself is far from well ordered. But this usage of theory also conforms to my main intention, which is simply to make an in-depth analysis of the Japanese evidence in

order to see if we can learn something new which might help unravel the general problem of the relationship between place of residence and political behavior.

Political Participation in Japan

Japan is one of the important examples of a country in which urban residence and political participation are negatively correlated. Voting turnout in the postwar era has been higher in rural than in urban districts at all election levels (Table 1).¹⁶ Similar patterns can be seen over

¹⁶ The precise meaning of urban and rural designations in Japan should be made clear from the outset. Japanese government publications and survey research groups typically use administrative designations rather than size of population to designate urban and rural locations. The relevant Japanese administrative divisions include a metropolitan prefecture (Tokyo), cities, towns and villages. Tokyo and other large metropolitan centers are also appropriately grouped under the category "large cities" in some official reports and in many survey analyses. The category "city" refers in turn in these cases to intermediate administrative units, whose populations may be largely rural in some cases, as the result of the effects of administrative mergers carried out in the 1950s. Towns and villages are nowadays quite systematically rural in composition. In other words, they contain high ratios of persons employed in agriculture, and population densities are much lower than in particularly the large urban areas.

In the following comments and figures, I have used

Table 2. House of Representatives Election Turnout and Urbanization^a

Year	Correlation (r) Turnout vs Level of Urbanization ^b
1925	-.419
1930	-.520
1935	-.535
1940	-.391
1950	-.718
1955	-.591
1960	-.643

^a The unit of analysis is prefectures, a unit sometimes compared with American states, but having different status in Japan's unitary system. With certain exceptions, elections and census information is from the same year period; however, 1928 election turnouts are matched with 1925 census figures, 1936 election outcomes are paired with 1935 population data and 1942 election participation is compared with 1940 population information.

The sources of the population information is Sōrifu Tōkeikyoku, *Nihon Tōkei Nenkan* 1967, pp. 12-16. Election turnout figures are from Kōmei Senkyo Remmei, *Shūgiin Giin Senkyo no Jisseki: Dai Ikkai-Dai Sanjukai*, 1968, pp. 39-51.

^b Turnout=prefectural mean turnout; urbanization=proportion of prefectural population living in cities.

time in the relationship between prefectural urbanization levels and turnout in House of Representatives elections (Table 2). Moreover, as levels of urbanization have increased, especially since World War II, the negative correlation has also become stronger.

Survey research findings show that other kinds of active participation in politics—discussion of public affairs with neighbors or in meetings, solicitation of other people's votes and articulation of self-interest—are also more common in the rural districts than in the cities in Japan (Table 3).¹⁷ The only kinds of participation in which urbanites match or exceed the levels of rural performance are in discussions of politics within the immediate family and at place of work.

Urban-rural tendencies in voting turnout in Japan have received more scholarly attention than other kinds of participation thus far. Noting that reported interest in politics is typically higher among persons interviewed in the urban centers, most scholars have seen the high rural turnouts as a kind of deviant case in participatory behavior.¹⁸ The rural performance at the polls has been explained on the basis of urban-rural differences in community structure which facilitate mobilization of the vote in the rural areas. Explicit pressures from local influentials to turn out the vote in support for a particular candidate, along with parallel efforts to attain the distinction of high voting rates, certainly are common in rural Japan, and their potential effects cannot be ignored. Nor can the mobilizing potential of rural life be forgotten in considering the roots of sectoral differences in other kinds of behavior.

"urban" where evidence is from samples drawn from metropolitan or city populations, and "rural" for findings on persons living in towns and villages. Information from intermediate sized cities will be omitted for simplicity's sake in most cases where data for metropolitan areas were available. I have indicated this in the table footnotes.

¹⁷ Rural levels of speech attendance also exceeded those in the cities, according to Kōmei Senkyo Remmei, *Sōsenkyo no Jittai*, 1961, p. 28. Joji Watanuki has reported similar trends using an index of participation that includes turnout, organizational participation, activity in solution of local problems, attendance at rallies and contacts with political elites. See his "Social Structure and Political Participation in Japan," The Laboratory for Political Research, Department of Political Science, The University of Iowa, Report Number 32, May 1970, pp. 4-5. Watanuki's work is the major exception to the comments that follow on emphases in the literature on urban-rural differences in Japan.

¹⁸ See, for example, the discussion in Kyogoku and Ike, "Urban-Rural Differences," pp. 171-72. These and other commentators on rural behavior have developed what is tantamount to a mobilizational or social influence model of participation and voting choice.

Nevertheless, it is still legitimate to look for other roots of these urban-rural differences. The effects of pressures to get out the vote in Japan have never been studied adequately by survey research techniques. Much of the research on political attitude structures to date has also focused on a comparatively limited range of orientations, and may have left out measurement of attitudes actually critical to participation. In order to fill this research gap, urban-rural differences in political culture will be examined here as a potential contributant to the observed participation trends. Using unpub-

Table 3. Political Participation, by Place of Residence

	Metro-politan	Urban	Rural	N
Discussion of national affairs: ^a				
At home	59%	61%	61%	
With neighbors	24	24	44	
At workplace	42	43	35	
At meetings	27	38	43	5000
Discussion of local affairs: ^a				
At home	56	61	64	
With neighbors	26	38	48	
At workplace	40	43	36	
At meetings	27	38	44	5000
Articulation of interest: ^b				
Often/sometimes	15		26	354
Solicitation of somebody else's vote:				
National election	5		16	
Local election	4		14	354

Note: The category of urban in this table and in Figure 4 refers to provincial cities, where both social and political characteristics are more like those in rural districts than in the large metropolitan centers.

All figures represent percentages of total relevant subsamples.

^a Kōmei Senkyo Remmei, *Kōmei Senkyo no Jittai*, 1958, p. 83. This is the only national study where frequencies for discussion outside the family are reported.

^b These data are from a reanalysis of my 1964 survey of political attitudes and voting choice behavior in Yokohama and two rural communities (previously reported in Bradley M. Richardson, "Political Attitudes and Behavior in Contemporary Japan," unpublished Ph.D. dissertation, University of California (Berkeley), 1966.) The findings on interest articulation are supported by the results reported in Miyagi ken Senkyo Kanri Iikai, *Kōmei Senkyo Jittai Chōsa*, 1962, appendix p. 8. Similar patterns in urban-rural vote solicitation were mentioned in Hiroshima ken Senkyo Kanri Iikai, *Senkyo Ishiki ni kan suru Yūkensha no Jittai*, 1965, p. 78.

lished findings from a large number of Japanese surveys, I will show that there are multiple patterns of differentiation between the urban and rural districts in attitudes about local and national political affairs. Because of their potential explanatory importance, the sources of these differences will be sought by looking at sectoral patterns in community life, social structure, and communications behavior, employing in most cases the Coleman multivariate model.¹⁹ Finally, the relationship between sectoral political culture tendencies and participation will be analyzed, and some suggestions for the theoretical relevance of the Japanese findings will be noted.

More than two hundred reports were utilized in the analysis reported here. The reports were the product of the research activities of the Kōmei Senkyo Remmei. This group, as is well known, is engaged in various kinds of educational activities in connection with campaigns to reduce election law violations. The many surveys conducted by the national headquarters and local affiliates of this group provide a substantial pool of replicative findings. Moreover, considerable creativity has been present in questionnaire construction in especially the case of surveys conducted in cooperation with local scholars. In the research reported here, representative findings from these studies are presented.²⁰

Sectoral Differences in Japanese Society

As in most contemporary industrial societies, Japan's modern communications networks and

¹⁹ The Coleman approach, known as effect parameter analysis, is discussed in James S. Coleman, *Introduction to Mathematical Sociology* (New York: The Free Press, 1964), chap. 6. It permits multivariate analysis of data that do not meet interval level assumptions. For an application in comparative analysis see Kenneth Thompson, "Cross National Voting Behavior Research: An Example of Computer Assisted Multivariate Analysis of Attribute Data," in *Comparative Politics Series*, ed. Harry Eckstein and Ted Robert Gurr (Beverly Hills: Sage Publications, 1970), Vol. I, publication number 01-003.

The Coleman analysis and more familiar contingency techniques were used with findings of several Prime Ministers' Office studies and my own 1964 data set. The Prime Minister's Office surveys, one of which was conducted under the auspices of the Kōmei Senkyo Remmei, were acquired from the Roper Public Opinion Center, Williamstown, Massachusetts.

My own survey of political attitudes was carried out in 1964 in Yokohama and in two rural areas, one in Kanagawa and one in Shimane. For purposes of economy the sample was stratified, but random sampling procedures were used in each district.

²⁰ For a discussion of the advantages and shortcomings of these reports in more detail, see Bradley M. Richardson, *The Political Culture of Japan* (Berkeley and Los Angeles: The University of California Press, 1973), chap. 1.

political-administrative structures deeply penetrate the rural sector and influence the nature of rural life. Yet despite these forces toward homogeneity, there are important differences between the large cities and the countryside in the character of community relationships.²¹ There are also sectoral variations in the distributions of certain kinds of communication behavior. For example, rural residents feel they depend more heavily on informal communications processes for political information, while urbanites report greater reliance upon the media. This observation is of special importance in the discussion that follows, since it is assumed that there may be some greater differentiation of informal and formal communications processes in at least the rural sector in Japan than has been anticipated by proponents of the theory of two stage communications flow.²²

Residential patterns in rural Japan are substantially more like those found in many places in Europe than those of the United States, with rural homesteads being typically concentrated in small, compact hamlets rather than being isolated from each other by fields. These rural communities have a very high density of both informal social interaction and group activities. Rural society thus typically encompasses substantially higher frequencies of contacts among neighbors and acquaintances than are found in the cities, along with far higher levels of organized group memberships and participation (Table 4). There is also less overall residential mobility in the rural areas, with far greater numbers of rural people reporting that they have lived in a particular community all of their lives than is the case among urban residents. Students of rural community life, moreover, have repeatedly called attention to the importance of feelings of solidarity and local loyalty.

²¹ The comments here on these matters draw from many sources, among them notably, R. P. Dore, *Land Reform in Japan* (London: Oxford University Press, 1959), chap. 14 and Ezra Vogel, *Japan's New Middle Class* (Berkeley and Los Angeles: The University of California Press, 1963), especially pp. 102-13. There are, in addition, substantial differences between the character of neighborhood life in different sections of the large cities, or there were a few years back. Also, provincial cities are in all appearances from the poll data much more like the rural sector in regard to the character of their community life than like the really large cities.

²² The nature of communications processes in the two sectors was discussed in some detail in Richardson, *The Political Culture of Japan*, chap. 6 (including notes 22, 24, 25 and 50). The really substantial variations in per-capita newspaper subscriptions between the cities and many rural districts was also noted.

For discussion of the two-stage theory, see, for example, Daniel Katz and Paul Lazarsfeld, *Personal Influence* (New York: The Free Press, 1964), chap. 14.

alties in these hamlets, whereas sentiments of these kinds are less commonly remarked upon in descriptions of urban settings.

In addition to these variations, there are also differences in the social and political character-

istics of the urban versus rural population. As is typical of advanced countries, there is a higher percentage of better educated people in Japan's cities than in the rural districts, both because more urban natives seek high school and college degrees and the better-educated sons and daughters of rural and small town families regularly immigrate to the cities. This educational advantage in turn helps contribute to greater exposure to the political content of the media in the cities.²³ Finally, there are, not surprisingly, more leftist party supporters in Japan's cities than in the rural areas.²⁴ Associated with these sectoral differences in community life and sociopolitical structure in Japan is a partial dichotomization of political socialization processes, to be considered in the next sections:

Political Involvement in City and Countryside

The expectations of modernization theory are certainly met at the sectoral level in Japan in regard to at least attitudes about involvement in the abstract world of national affairs. Just as was predicted by extrapolating hypotheses from the global theories of Lerner and others, urban residence is associated with higher levels of concern for what is going on in cosmopolitan political arenas. In this case, the specific attitudinal dimensions are appreciation of the relevance of national politics, interest in national affairs, and feeling that national politics is easy to understand (Table 5).

Underlying these differences in cosmopolitan involvement, moreover, are sectoral patterns in social structure, especially those differences in educational attainment already mentioned. When controls for educational levels are introduced, the original relationships between residence and feelings that national politics is relevant and easy to understand are substantially diminished.²⁵ Interest in national politics is also

Table 4. Social Characteristics of Urban and Rural Population

	Metro- politan	Urban	Rural	N
Education: ^a				
Lower/Middle School	52%	61%	75%	
High School/College	48	39	25	2425
Media Exposure: ^b				
Follow Newspapers	80	70	66	
Follow radio/television	93	87	85	5000
Party Support: ^a				
Leftist Parties	31	22	16	2425
Length of Residence: ^a				
Over 15 years	46	62	72	
15 years and less	54	38	28	2425
Association with Neighbors: ^c				
Associate "enthusias- tically"	28	37	50	
Associate without enthusiasm or don't associate	71	60	46	1794
Scope of Acquaintance- ships: ^c				
0-29 families	63	51	46	
30 or more families	15	23	36	1794
Group Memberships: ^d				
No groups	31		8	
1-3 groups	67		49	
4 or more groups	2		43	354

^a Calculated from Prime Minister's Office Survey 1404, May 1963. Figures for education are typical of those in Japanese national surveys.

Length of residence figures refer to specifically the "same ward, city or town." Length of residence data are also representative.

Leftist party support includes responses favoring the Japan Socialists, Democratic Socialists and Japan Communists.

^b Kōmei Senkyo Remmei, *Kōmei Senkyo no Jittai*, 1962, pp. 13-14. Percentages refer to frequencies of respondents who followed the media "often" and "sometimes."

^c Calculated from Prime Minister's Office Survey 2060, March 1966.

^d From Richardson, 1964 survey; see also Fukuoka ken Senkyo Kanri Iinkai, *Kōmei Senkyo Undō no Kōka Sokutei*, 1960, 9, for similar findings.

²³ For evidence on the very substantial impact on at least written media exposure of education, see, for example, Tōkyō to Senkyo Kanri Iinkai, *Kōmei Senkyo Yoron Chōsa no Gaiyō*, 1958, 12, 17, and 23. Only 20 per cent of primary school graduates and 27 per cent of those who completed middle school, for example, read about politics in the newspapers on a regular basis, in contrast with 78 per cent of college graduates. Differences in television and radio exposure to political content attributed to education were somewhat smaller, however.

²⁴ For the best discussion of the sources of leftist party support in different population groupings, including attention to the special trends within Japan's salaried middle class, see Joji Watanuki, "Patterns of Politics in Present Day Japan," in Seymour M. Lipset and Stein Rokkan, *Party Systems and Voter Alignments* (New York: The Free Press, 1967), chap. 9.

²⁵ An initial index of urban-rural differences of .098 for feelings about the relevance of national politics was reduced to .025 by controlling for the effects of urban-

Table 5. Political Involvement Attitudes, by Place of Residence

	Urban					Rural				
	Yes	No	DK	Total	N	Yes	No	DK	Total	N
National politics is relevant to our lives ^a	78%	15%	7%	100%	175	69%	15%	16%	100%	174
Politics is a topic of interest ^b	60	40		100	unk	50	50		100	unk
Elections are a topic of interest ^c	86	14		100	124	84	15	1	100	170
National election outcome is a matter of concern ^a	73	27		100	175	70	28	2	100	174
National politics is easy to understand ^a	28	69	3	100	175	18	72	10	100	174
Local politics is relevant to our lives ^a	78	12	11	101	175	78	10	12	100	174
Local politics is a topic of interest ^a	58	42		100	175	76	24		100	174
Local election outcome is a matter of concern ^a	64	34	2	100	175	68	30	2	100	174
Local politics is easy to understand ^a	30	64	6	100	175	43	49	8	100	174

Note: Results of significance tests are not presented here nor in subsequent contingency tables. They were not normally available in the original sources, and need not be calculated in most cases since representative tendencies in findings not dependent upon composition of single samples are reported. For discussions of this matter and the process of establishing representative patterns by aggregation of findings from other studies, see my *The Political Culture of Japan* (Berkeley and Los Angeles: The University of California Press, 1973), especially Chapter 1. Findings of significance tests will be presented in the tables that follow, however, when the Coleman effect parameter analysis depends on findings from only one survey, namely my own 1964 study.

^a Richardson 1964 post-election survey; "urban" is Yokohama.

^b *Nihon no Seijiteki Mukanshin* (Tōkyō, 1961), p. 41. "Urban" refers in this case to Japan's large cities. For similar results on interest patterns see also Kōmei Senkyo Remmei, *Seiji Ishiki to Senkyo Kōdō no Jittai: Fukuoka Kenmin no Seiji Ishiki*, 1966, 141, and Prime Minister's Office, Survey No. 1898, July 1965.

^c Kōmei Senkyo Remmei, p. 144.

affected to some degree by educational patterns, although to a lesser extent than the other

rural distributions of educational attainment. A sectoral difference rate of .098 in feelings that national politics was easy to understand was reduced to .043. (Underlining in the form of dashes indicates indexes significant at the .05 level of confidence. Solid lines indicate significance at the .01 level. No line indicates indexes that do not meet the .05 confidence criterion.)

The Coleman effect parameters or indexes are extremely simple to understand. In the bivariate case they represent simply the difference between the left and right cells of a normal two by two contingency table. In the case at hand, they are equivalent to the difference when the "yes" categories for rural respondents in Table 5 are subtracted from the "yes" responses for urban residents. The plus and minus signs are the result of the numerical coding system assigned to the different categories; in the analysis here a minus sign will indicate that higher frequencies were found in the rural sector. In the multivariate case, the indexes are based on more complicated calculations, but they can be compared to the mean of the relevant differences in the various tables generated by introduction of additional controls in contingency table analysis. In this case, the residual index for the effects of residence is the weighted average of differences attributable to urban-rural residence with the different educational categories held constant. Unlike contingency table analysis, however, multivariate analysis using the Coleman model involves simultaneous controls for two or more independent variables.

attitudes.²⁶ Furthermore, if education is seen as a surrogate for media exposure—an assumption warranted particularly by evidence on the relationship between education and exposure to the written media's accounts of politics—some of the linkages postulated at the individual level in the Lerner modernization model are indeed present in contrasting behavior within the different sectors in modernized Japan.

But something other than the cosmopolitanizing influences of education and media exposure seem to be associated with urban-rural differences in people's attitudes specifically about elections and local politics. Rural residents showed nearly as much interest in national elections—as contrasted with "politics"—as urban residents, and similar tendencies can be seen in concern about national election outcomes. Urban and rural differences are thus minimized when it comes to feelings about the more immediate and tangible aspects of politics. Both the opportunities for personal participation as well as the more tangible implications of electoral outcomes for both local and parti-

²⁶ Controls for education also substantially diminished urban-rural differences in interest in politics in a secondary analysis of the data from Prime Minister's Office Survey 1898 (1963).

san interests lend a greater saliency to elections than to the more abstract goings on of national politics at the Cabinet or Diet level.²⁷

The disproportionately greater involvement of rural residents in specifically local politics and elections, and their enhanced feelings of cognitive competence about local politics, are related to proximity considerations as well as to the special character of rural social structure. Acquisition of information about local politics and direct contact with persons who are involved in local public affairs are facilitated in the intimate rural social milieu. Multivariate analysis shows that sectoral differences in educational levels have little or no effect on the urban-rural tendencies in local political involvement. In contrast, sectoral trends in group membership levels, which constitute a surrogate measure for levels of community involvement in the rural sector in addition to their more general organizational involvement implications, do underlie some of the sectoral variation.²⁸ The competition between intensely loyal

groups of rival community partisans often observed in rural local elections also bears on the sectoral differences in local involvement, as does the comparatively high levels of instrumentalist sentiment found in the rural sector.²⁹

Two fairly distinct patterns in political socialization—one in the urban and one in the rural areas—can be identified from the findings I have reported. Tendencies in educational attainment and presumably also media exposure account for some of the sectoral differences in involvement on the more abstract levels of political life, while variations in group memberships, proximity effects and the salience of community loyalties and interests are the main socializing forces where attitudes toward elections and local affairs are concerned. Obviously, many persons in both sectors are exposed to cosmopolitanizing as well as localizing influences. But the differences in social structure on one hand and community life and organizational involvement on the other are of special importance in Japan and contribute to substantially differentiated attitude structures in the two sectors. The meaning of these urban-rural tendencies in involvement for political participation patterns has to be investigated. But it would seem that rural voters in Japan are more politically involved than has typically been assumed, and it is possible that some aspects of their involvement contribute to the high levels of participation found in the rural areas in comparison with the cities.³⁰

Evaluative Attitudes

Urban-rural patterns in Japanese voters' assessments of the performance of officials and the quality of public life, which I have called evaluative attitudes here, merit special attention. In response to a wide variety of questions about satisfaction with politics and politicians, estimates of the responsiveness of representa-

²⁷ These relationships between responses to remote and more proximate aspects of politics are also shown in information on people's knowledge about politics. Urban residents felt that they had known about the candidate they supported in gubernatorial elections prior to the election more commonly than rural people, according to my analysis of Prime Minister's Office Survey 1404, while the reverse was the case in regard to prior knowledge about mayoralty and assembly candidates. Moreover, controls for education substantially decreased the strength of the relationship between residence and knowledge at the gubernatorial level, but length of residence was the underlying contributory to development of prior knowledge about local figures. While this is certainly not surprising, the differences in communications and socialization processes for alternative levels of politics implied by these data are of critical importance. But simple proximity in the form of actual participation in electoral processes can be seen as important, too, permitting less educated persons in especially the rural sector to develop an interest in at least some aspects of national political affairs. Compare Lester Milbrath's proposition that "the more stimuli about politics a person receives, the greater likelihood he will participate in politics and the greater depth of his participation." See Milbrath, *Political Participation*, p. 39.

Elsewhere I have shown that variations in proximity to different levels of politics are the most plausible cause of differential rates of attitudinal change in the postwar period. Proximity and relevance considerations would thus occupy a central position in a theory of mass attitudinal change. See Richardson, *The Political Culture of Japan*, chap. 2.

²⁸ Controlling for group memberships reduced an original urban-rural difference of $-.181$ in interest in local politics to $-.106$, concern about local election outcomes was reduced from $-.044$ to $-.008$ and comprehension of local politics was reduced from $-.132$ to $-.056$. The relative importance of the higher group involvement levels found in the rural sector can be seen from these figures, as well as the residual importance of simply living in the compact, rural communities. In-

terestingly, length of residence, which is a variable of importance in some Japanese studies, did not affect the basic urban-rural differences, despite the fact that far more persons had lived all of their lives in the rural sector than was the case in the cities.

²⁹ A factor analysis of attitude structures in Japan reported elsewhere did show that feelings of wanting something (instrumentalism) loaded on the same dimension as interest in politics, and the loadings were higher where specifically local sentiments were concerned. See Richardson, *The Political Culture of Japan*, chap. 4, Table 4-5.

³⁰ It should be remembered at this point that most earlier studies of urban-rural differences in political behavior in Japan have typically relied on only one or two simple measures of attitudes toward national politics; this has resulted in a simplistic view of the nature of urban-rural political involvement.

Table 6. Evaluative Attitudes, by Urban and Rural Residence

	Urban					Rural				
	Posi- tive	Nega- tive	DK	Total	N	Posi- tive	Nega- tive	DK	Total	N
Satisfaction with politics ^a	12%	66%	22%	100%	102	18%	50%	32%	100%	68
Satisfaction with elected Councillors ^b	51	28	21	100	421	56	13	31	100	905
Quality of recent local assemblymen ^c	20	31	49	100	421	35	21	44	100	780
Politics improves by holding elections ^d	30	42	28	100	773	37	36	27	100	2205
National election went on honestly and fairly ^e	22	33	45	100	254	35	25	40	100	864
National politicians and officials are concerned about our needs ^f	51	35	14	100	175	55	20	25	100	174
Local politicians and officials are concerned about our needs ^f	57	27	14	100	175	75	10	15	100	174

Note: "Positive" responses were those which indicated satisfaction, positive appraisals of electoral processes and favorable perceptions of politicians' responsiveness. "Negative" answers included manifestations of dissatisfaction, pessimism about elections (including the feeling that "politics stays the same"), perceptions that "many" or "all" candidates violated election laws and perceptions that politicians were not responsive. "Urban" in national survey findings refers to large cities, and "rural" is counties or towns and villages; intermediate level population unit data from national surveys were omitted for the sake of simplicity.

^a Kumamoto ken Senkyo Kanri Iinkai, *Senkyo no Jittai*, 1960, p. 34.

^b Kōmei Senkyo Remmei, *Sangin Giin Tsūjō Senkyo no Jittai*, 1962, p. 88.

^c Kōmei Senkyo Remmei, *Dai Rokkai Tōitsu Chihō Senkyo to Yūkensha*, 1967, p. 133.

^d Kōmei Senkyo Remmei, *Kōmei Senkyo no Jittai*, 1958, p. 72.

^e Calculated from Prime Ministers Office 1898, July 1965.

^f From Richardson 1964 survey.

tives, and evaluations of the electoral process, urban residents have typically been either more pessimistic and cynical than their country counterparts, or else they showed contradictory tendencies of both optimism and pessimism (Table 6).³¹ Both the greater pessimism about political processes which prevails in the urban areas, as well as the ambivalence seen in some cases, add to our impression of the complexity of urban-rural differences in Japan. Greater pessimism could also be one of the explanations of the lower levels of participation found in the cities.³²

There are several possible explanations for these patterns in evaluative attitudes. In the first place, people in the cities can be assumed to be more exposed to the media's reports of political scandals and electoral law violations

than are persons who live in the country. They may also be assumed to be more aware of the political criticisms voiced in many newspaper editorials.

Modernization theory predicted that the expansion of media exposure accompanying urbanization would help integrate people and stimulate political participation through development of an awareness of what goes on in national political processes. Clearly there was no expectation that heightened media exposure could also lead to political alienation. Yet this is what seems to have happened in Japan. Data on media exposure which could make a direct test of this hypothesis possible are not available. But it is clear that high education, which typically means high exposure to media communications, is associated in some instances with greater pessimism about politics than is found at lower education levels; elsewhere, it is associated with more ambivalence than might have been anticipated from earlier writings on the political evaluations of well-educated persons (Table 7).

When controls were introduced for education as a surrogate for media exposure, the basic urban-rural differences in appraisals of the responsiveness of politicians—the one evaluative attitude for which I had data that could be analyzed further—remained largely unaffected.

³¹ Feelings of efficacy regarding national political life showed contrary patterns, as is shown in Richardson, *The Political Culture of Japan*, chap. 6. This is largely the result of educational differences, and in the face of the prevailing cynicism can be seen mainly as constituting a formal normative response to postwar emphases on democratization of Japanese society and politics.

³² It should be noted that Robert Ward has forecast the presence of tendencies in political negativism of the kind reported here in his "Japan: The Continuity of Modernization" in *Political Culture and Political Development*, ed. Lucian W. Pye and Sidney Verba (Princeton: Princeton University Press, 1965), pp. 27–82.

This was in part an artifact of the ambivalence registered among well-educated persons on this measure. Whatever the case, I was unable to pursue this interesting hypothesis further at this point, in spite of its plausibility in the Japanese frame of reference. (It would also be desirable to carry out comparative content analyses of media communications content in order to properly test this proposition.)

A second consideration of possible importance to sectoral differences in evaluative attitudes in the Japanese case is the relative distribution of party preferences in the cities and in the rural districts. Postwar partisan politics in Japan have been characterized by frequent manifestations of rigid oppositionalism and extreme frustration among the parties of the left. There is strong left-right ideological polarization and, as is well known, the left-wing parties have found themselves out of power during much of the postwar era.³³ The presence of both of these factors has led to an extreme negativism among leftist party activists, which in turn can be assumed to have contributed to negative evaluative orientations among leftist party supporters. Since leftist party followers account for a much larger ratio of the elector-

ate in the cities than they do in the country constituencies, some of the sectoral differences in evaluative attitudes may stem, in fact, from the socializing effects of party preference.

This hypothesis was tested by introducing controls for party support, since leftist party adherents were shown to be more negative in their attitudes than Conservative followers were in the various surveys reviewed.³⁴ Examination of urban-rural differences after controls had been introduced did support the basic hypothesis specifically for assessments of corruption levels in politics.³⁵ It is consequently obvious that patterns in the distributions of partisan preferences contribute to the basic urban-rural differences in evaluative feelings in some cases. Nevertheless, controls for party preferences using available data sets did not erase urban-rural differences in every case where attitudes were judged as having evaluative content, and there was no indication of systematic tendencies in regard to specific types of evaluations. The effects of place of residence or other related in-

³⁴ Leftist party supporters are not *always* more negative in orientation, according to the materials I have studied. But this is the case where national surveys were concerned, as in Kōmei Senkyo Remmei, *Kōmei Senkyo no Jittai*, 1962, p. 135; Kōmei Senkyo Remmei, *Kōmei Senkyo no Jittai*, 1958, p. 71; and Kōmei Senkyo Remmei, *Dairokkai Tōitsu Chihō Senkyo to Yūkensha*, 1967, p. 309.

³⁵ The analysis in question utilized data from Prime Minister's Office Survey 1398.

Table 7. Evaluative Attitudes, by Educational Attainment

	Low Educational Level					High Educational Level				
	Posi- tive	Nega- tive	DK	Total	N	Posi- tive	Nega- tive	DK	Total	N
Quality of recent local assemblymen ^a	32%	23%	45%	100%	1540	22%	38%	41%	101%	162
Politics improves by holding elections ^b	38	34	28	100		36	52	12	100	
Candidates obey the election laws ^c	25	24	52	101	560	42	35	23	100	146
National politicians and officials are concerned about our needs ^d	45	23	32	100	74	66	34		100	32
Local politicians and officials are concerned about our needs ^d	54	22	24	100	74	66	28	6	100	32

Note: "Positive" and "negative" have the same meanings as in Table 6. The DK category includes responses such as "can't say" and "unclear." Persons who have completed only primary school are included in the low educational category, and graduates of universities are represented in the high educational grouping. Although this could bias the findings in view of the fact that postwar educational requirements included compulsory completion of middle school, evidence of such bias could not be seen in the findings. Data for high school graduates (which in no way contradicted the picture shown here) were omitted for sake of simplicity.

^a Kōmei Senkyo Remmei, *Dai Rokkai Tōitsu Chihō Senkyo to Yūkensha*, 1967, p. 134. Primary and middle school graduates were grouped in this case. For comparable trends in general satisfaction with politics see Kumamoto ken Senkyo Kanri Iinkai, *Senkyo no Jittai*, 1960, p. 36.

^b Kōmei Senkyo Remmei, *Kōmei Senkyo no Jittai*, 1958, p. 71; the results are for men only, but those for women were similar.

^c Kōmei Senkyo Remmei, *Sangiin Giin Tsūjō Senkyo no Jittai*, 1962, p. 71.

^d Richardson 1964 survey.

fluences are consequently still important in many cases, and patterns in partisanship are not a systematic cause of urban pessimism as far as can be seen. These data must be taken in account along with the earlier evidence which indicated that sectoral educational differences were also not primarily responsible for the trends in popular evaluations of politics.

Both the qualitative literature on urban and rural society and evidence cited earlier in Table 3 call attention to the fact that there are substantial urban-rural differences in both the nature of community life and the levels of individuals' group involvement in Japan. These differences have significance for the analysis here, since contacts between ordinary people and politicians in Japan are often mediated by networks of personal relationship in which community influentials play critical roles. It is thus plausible to hypothesize that the higher frequencies of negative feelings about politics in the cities stem in part from the alienative effects of the comparatively lower levels of social contacts with especially politicians and influentials. Indeed, speculation along these lines seems particularly appropriate in the Japanese case, since dependence on personal contacts to seek access and favorable consideration seems to play an especially prominent role in Japanese behavior.

There was some evidence supporting this hypothesis in answers to open-ended questions in my own survey. Various people in the cities observed that they felt apathetic about politics because they "didn't know any politician" or "felt distant from public affairs."³⁶ But alienation from politics also occurred in rural situations where a Diet member lacked close local ties, as well as in cities where general social relationships were less dense than they were in rural communities.

If the hypothesis in this case were valid, a fairly strong positive correlation might be expected between levels of group membership and favorable appraisals of political performance. Group membership frequencies represent a direct indicator of some kinds of social

involvement and also comprise a surrogate measure for degree of integration into community activities in the rural areas. Were the anticipated kinds of effects present, controls for group memberships should also contribute to a reduction in the urban-rural differences in evaluative feelings.

Examination of these relationships in my own data on evaluations of politicians' responsiveness did in fact show the anticipated results for bivariate linkages. Group memberships showed a substantial positive correlation, as predicted, with feelings that politicians were concerned about people's needs, and it can be assumed that contacts with politicians mediated by group membership helped further these tendencies. Differences in organizational involvement, however, had little effect on the basic urban-rural patterns in a multivariate analysis of the findings about assessments of politicians' responsibility.

This analysis does not lend support to a social involvement interpretation of sectoral trends in evaluative attitudes, in which organizational memberships are seen as the basic and essential linkages with community influence structures. Nevertheless, the residual sectoral differences presumably attributable to differences in the quality of community life were still important here.³⁷ Simply living in the rural areas rather than being involved in group activities there, may be the critical determinant of positive evaluations.

But sectoral differences in normative perspective might also contribute to the observed trends in evaluations of political performance. It is possible to detect the presence of somewhat more permissive opinions among rural residents in contrast with those of people in the cities in Japan. For example, answers to questions about the degree to which violations of the electoral laws "can't be helped," in contrast with feelings that violations are clearly a bad practice, generally indicate the presence of more permissive perspectives in the rural areas (Table 8). Comparatively greater rural permissiveness is also implied in replies to queries about what might be done in the case a favored

³⁶ Interviews with household association leaders in Yokohama provided some additional evidence on this phenomenon. To be sure, some of the laments about the declining levels of community feeling from these local leaders could be discounted as merely unsubstantiated nostalgia. But the leaders also stated that increasing proportions of persons within particular neighborhoods were unknown to the leaders themselves and had no contact with local organizations. It is noteworthy that these evaluations were concentrated in Yokohama's *shitamachi* (downtown area), where neighborhoods are typically assumed to be more cohesive than in other urban settings.

³⁷ Interestingly an initial urban-rural index of $-.034$ in regard to evaluations of national politicians' responsiveness increased to $-.088$ when education was controlled; analogously, the index for assessments of local responsiveness increased from a basic $-.184$ to $-.242$ after controls for education were introduced.

Something was clearly different about rural residence in these instances, even though organizational involvement was not the contributing element. Perhaps communitywide feelings about responsiveness, reflecting stable linkages between local influentials and specific politicians, are the basis for the differences here.

candidate were found guilty of vote buying (Table 8). Urban-rural patterns in responses to questions about comparatively formal aspects of democratic normative systems also permit the inference that political idealism is somewhat higher in the urban sector, and these differences could contribute to urban negativism in evaluative attitudes.³⁸ In other words, urban residents are often more demanding in evaluating political performance than are rural residents.

In my own opinion, this fascinating urban-rural difference in normative perspective stems in part from the self-consciousness which urban residents feel toward being modern in outlook. This leads quite directly to espousal of democratic ideas. The compulsion to be modern also leads to condemnation of older social customs, such as gift-giving in exchange for favors, which in part underlie the practices in Japanese politics which result in charges of corruption. But the urban-rural differences in outlook on politicians' performance are also very likely affected by the overriding loyalties to particular politicians in some rural areas, as well as the

greater tolerance for older customs in the countryside.

Attitudes Toward Participation in Politics

Urban and rural residents in Japan differ in attitude toward both the obligation to go out to vote and the appropriateness of more active forms of political participation. The feeling that the vote is a social obligation is more pronounced among persons living in the rural sector than in the cities, as is indicated from the evidence from various sections of the country reported in Table 9.

These urban-rural distributions in feelings about the vote as a duty clearly reflect a variety of factors. In part they can be seen as an extension into political frames of reference of more general feelings of social obligation that are maintained at enhanced levels in the rural sector.³⁹ To the extent that this interpretation is valid, the analysis showed that these feelings are nevertheless independent of the effects of sectoral differences in organizational involvement. Being active in several groups did correlate positively with dutiful feelings about the vote, but controls for group membership levels did not erase much of the urban-rural distinctiveness in this attitudinal dimension.

It is also possible that the higher frequencies

³⁸ Evidence on the urban-rural distributions of feelings that accord with democratic postulates—such as "politics moves by our power" or "it's better to pay attention to what's going on in politics than just follow leaders"—supports this observation, and there is clearly some attitudinal ambivalence in the more normative dimensions of efficacy and appraisals of performance. A similar ambivalence can be observed among well educated persons. For relevant findings see Fukushima ken Senkyo Kanri Iinkai, *Chihō Senkyo no Jittai*, 1963, p. 176 and Kōmei Senkyo Remmei, *Seiji Ishiki to Senkyo Kōdō no Jittai: Yamanashi ken Toshi to Nōson*, 1966, p. 35.

³⁹ There is some indication that this popular expectation is found in reality in the reports from the Institute for Mathematic Statistics' national character study, although urban-rural differences were not enormous. See, as one example, Tōkei Sūri Kenkyūjo, *Kokuminsei no Kenkyū: Dai Niji Chōsa* (Tōkyō: Tōkei Sūri Kenkyūjo, 1959), pp. 55 and 58.

Table 8. Attitudes Toward Electoral Corruption, by Place of Residence

	Urban	Rural	N
Feelings about illegal election practices: ^a			
Can't be helped or 'natural'	22%	32%	
It's a bad thing	75	65	
Unclear	3	3	
	100%	100%	1734
Action in event supported candidate had bribed voters: ^b			
Will not vote for him under any circumstances	71	58	
Would vote for him if no other suitable candidate	11	19	
Will vote for him as he's still an outstanding person	2	6	
Doesn't bother me as bribery is common		3	
Don't know	16	14	
	100%	100%	985

^a Kōmei Senkyo Remmei, *Kōmei Senkyo no Jittai*, 1958, p. 78.

^b From secondary analysis of Prime Minister's Office 1898.

Table 9. Attitudes Toward the Vote and Participation, by Place of Residence

	Urban					Rural				
	Yes	No	DK	Total	N	Yes	No	DK	Total	N
It's everybody's duty to go out and vote ^a	30%	43%	27%	100%	174	62%	26%	12%	100%	174
It's bad to abstain from voting except under special circumstances ^b	48	41	11	100	unk	64	30	6	100	unk
It's better to vote for the best candidate rather than abstain, when a person you prefer isn't running ^c	54	26	20	100	172	68	27	5	100	79
Disagree with idea that it's all right to abstain when there is no appropriate candidate ^d	43	49	8	100	333	60	29	11	100	167
Would stress group activity to get people's opinions reflected in national affairs ^e	17	77	6	100	249	24	53	23	100	411
Would stress groups or movements to have opinions reflected in local politics ^f	35	61	4	100	163	49	46	5	100	312
Would take an active role in the resolution of local problems ^g	32	67	1	100	187	43	52	5	100	44

^a Richardson 1964 survey.

^b Gumma ken Senkyo Kanri Iinkai, *Kenmin no Seiji, Senkyo Ishiki*, 1963, pp. 63-64.

^c Gifu ken Senkyo Kanri Iinkai, *Gifu ken ni okeru Senkyo to Kenmin no Seiji Ishiki*, 1963, p. 69. The urban sector of the sample was drawn from the population of Gifu City.

^d Fukuoka ken Senkyo Kanri Iinkai, *Kiken no Jittai*, 1960, p. 51. The urban respondents lived in Fukuoka City.

^e Fukushima ken Senkyo Kanri Iinkai, *Fukushima ken ni okeru Seiji Ishiki ni kan suru Yoron Chōsa Kekka*, 1962, p. 172.

^f ———, *Chihō Senkyo no Jittai*, 1963, p. 168.

^g Miyagi ken Senkyo Kanri Iinkai, *Kōmei Senkyo Jittai Chōsa*, 1962, appendix, p. 7. The urban sector was Sendai City.

of dutiful feelings in the rural sector represent a continuation into the present of attitudinal patterns that began developing before the war. Voting turnouts have always been higher in Japan's rural sector for many reasons, and it is reasonable to assume that norms of dutifulness could be found there at earlier points in time. But multiple reinforcements also favor rural dominance in this attitudinal area today. High turnouts are valued as an end in many rural communities, and competition between local areas for the distinction of having the highest turnout is not uncommon. Loyalties to community are also converted into feelings that one must go out to vote when community supported candidates are running. Finally, some evidence indicates that rural voters are simply less responsive to the democratic concept of the vote as a "right" or a "chance to express one's interests."⁴⁰ This does not mean that rural voters are deficient in comparison with urban dwellers in regard to being aware of their own interests; the contrary is in fact the case,

⁴⁰ See the extensive findings of these matters reported in my *The Political Culture of Japan*, chap. 4.

as I will show shortly. It simply reflects the fact that rural respondents think self-consciously of obligation where the vote is concerned more than persons in the city.

Rural residents are also inclined more than urbanites to feel that group action is an appropriate vehicle for articulation of interests, and to express greater willingness to take an active part in local decision-making processes. (Table 9). It was not possible to pursue directly here the nature of the relationship between attributes of rural residence and these preferences for more active and collectivist kinds of political participation. But it is entirely plausible that the higher density of group life in the rural sector makes *collectivist* forms of political activity seem more natural there than is the case in the city. Similar linkages can be postulated for sectoral differences in willingness to take *active* political roles. The existence of higher frequencies of social interaction and group memberships in the rural sector probably reinforce the notion that active participation at the local level is possible. Undoubtedly, the norm favoring active participation in various kinds of

community collective efforts also plays some role in making rural respondents feel the appropriateness of taking part in local decisions.

Instrumentalism in Contemporary Japan

Perhaps the most surprising aspect of urban-rural differences in political orientations in the Japanese case lies in the finding that rural residents are substantially more aware than urbanites of the instrumental potentialities of politics (Table 10). Replies to questions about explicit awareness of having "interests that one would like the Diet Member or Local Assemblyman to see about," as well as answers to numerous questions about voting motivations indicate that persons living in the rural areas are much more aware of wanting things done for them than are urban residents.⁴¹ While it would probably be a good idea to systematically compare these attitudes with opinions on salient national issues—this was not permitted by the data used—it is nonetheless important to see that narrowly defined interests are much more salient to rural than to urban voters. These tendencies in concern for representation could prove to be important factors in the comparatively higher levels of both voting and other forms of political participation found in the rural districts.

⁴¹ This pragmatism is also shown in evidence I have discussed elsewhere regarding the content of both discussions of politics and specific requests for one's vote; in all instances, the open-ended questions I used to probe these matters resulted in spontaneous references to local problems more than to broad national issues. See my "Political Attitudes and Behavior in Contemporary Japan," unpublished Ph.D. dissertation, The University of California, Berkeley, 1966, p. 90.

There are clearly some precedents in the history of American rural life for anticipating high levels of involvement in concerns for narrow interest representation in the rural sector of countries; nevertheless, modernization and center-periphery hypotheses have both failed to forecast the systematic development of sentiments of this kind outside of the areas most influenced by urbanization.⁴² Developments of this type undoubtedly depend upon integrational links being forged between the rural districts and national political and economic processes; in Japan these have been built up in periods when representational considerations were probably less relevant than at present, but the character of social structure in rural Japan has clearly provided a favorable setting for enhancing instrumental sensitivities. The community differences of interest and the rivalries common in the rural sector are clearly one means of developing an instrumental awareness

⁴² Deutsch's writings are an exception in part, as observed earlier. But Deutsch's model postulates that the important linkage will be the commercialization of agriculture, with consequent development of concerns relating to general agrarian problems, while the evidence from Japan indicates that specifically local and parochial interests have precedence over other concerns. For evidence suggesting that these themes in rural outlooks have precedents in prewar days, see John K. Fairbank, Edwin O. Reischauer and Albert M. Craig, *East Asia: The Modern Transformation* (Boston: Houghton Mifflin, 1965), p. 520.

The presence of growing sentiments of an instrumental kind in the rural sector has been reported earlier by Joji Watanuki in his "Political Attitudes of the Japanese People," *The Sociological Review Monograph No. 10* (University of Keele, September 1966), pp. 166–168.

Table 10. Instrumental Attitudes, by Place of Residence

	Urban					Rural				
	Yes	No	DK	Total	N	Yes	No	DK	Total	N
Want our Diet member to do something for our local or occupational problems ^a	28%	44%	28%	100%	175	60%	26%	14%	100%	174
Want our Assemblyman to do something for our local or occupational problems ^a	36	36	28	100	175	67	24	9	100	174
Considered local or national interest in making voting choice ^b	70 (64)	22 (20)	8 (7)	100	428	74 (97)	21 (28)	5 (6)	100	1026

^a Richardson 1964 survey.

^b Kōmei Senkyo Remmei, *Sōsenkyo no Jittai*, 1961, p. 42. The figures in parentheses in the lower row are actually more meaningful here, since multiple answers were permitted and each response category was calculated as a separate percentage independent of the frequencies of answers in other categories. Thus, when one adds up the percentages of all of the persons in the urban and rural districts respectively who indicated that they thought about their interests in response to different separate parts of the question, it can clearly be seen that rural residents substantially exceeded urbanites in regard to at least the number of items which concerned them.

among rural citizens. Moreover, there are influentials in many rural communities whose mere presence and contact with other members of the community provide a leadership potential that is probably less common in most urban settings. The scope of local social involvement and the opportunities for informal communication in both neighboring and group relationships also permit more sharing of information and opinions on instrumental questions than is afforded by the typical metropolitan neighborhood.⁴³

The consequences of differences in social context were visible in strong relationships between multiple group memberships and instrumentalism, and controlling for group memberships reduced some of the differences in the simple effects of residence.⁴⁴ Even with this control, however, there were still effects attributable merely to living in a rural area, and it is clear that simply residing in the rural districts contributes in part to the observed tendencies in pragmatic feelings. Whatever the mix of these influences, rural voters are clearly more intensely concerned than their urban counterparts about narrowly defined interests, and this concern has potential importance for contemporary urban-rural differences in participation levels.

Sectoral Political Culture Differences in Japan and Political Participation

Some distinctive patterns in attributes of political culture have been identified in Japan's urban and rural sectors. Urbanites are more involved in national affairs, but they are also generally more negative in their evaluations of politics, less aware of self-interest dimensions, and less inclined to feel they would participate in politics either on the basis of the perceived value of activist contributions to political life or

on the basis of obligation. Rural residents had more involvement in local affairs, as well as comparatively greater political optimism and awareness of instrumental wants. They also indicated a greater willingness to assume activist roles in at least local politics, but were likewise more likely to see the vote as an obligation rather than a privilege. These sectoral tendencies were in turn linked—sometimes clearly, sometimes only suggestively—with different socializing influences in the two sectors.

Some effects of the urban-rural distinctiveness in political cultural orientations on political participation were also examined. In particular, it was anticipated that some links between sectoral political attitude structures and the higher participation rates observed in the rural districts might be discovered. Since the primary focus was the effects of the various attitudes on several types of behavior yielding only single dichotomous measures of performance, Coleman effect parameter analysis was again employed. The specific kinds of participation examined in this analysis were: voting turnout, attention to the media and speeches during election campaigns, discussion of politics, articulation of needs by talks with politicians or officials, and solicitation of other persons' votes for a particular candidate or party. In addition to the expectation that different attitude-behavior linkages could exist for different kinds of behavior, it was also felt that the linkages might be different for more cosmopolitan processes than they were for more local political processes.⁴⁵ In the analysis, therefore, information on attitudes and behavior was treated separately for the national and local political arenas.

The analysis was conducted at two levels. First, the effects of each single attitude on the different kinds of behavior were observed using simple bivariate indicators of effect (Table 11). From these statistical manipulations it could be seen that, in general, involvement in politics, including awareness of the instrumental potential of political processes, contributed more to participation in politics of different kinds than did favorable evaluations of political performance. It could also be seen that involvement and optimism about politics were comparatively more important as motivations for active participation (including campaign attention) than for

⁴³ There are other interpretations for this trend in sectoral feelings of instrumentalism in addition to those advanced already. Feelings of deprivation may motivate acute instrumental sensitivities in many areas of the country, perhaps especially the less affluent ones. It is also possible to speculate on the effects of the higher levels of property ownership that are found in the rural areas, although the only information available in this area is limited to statistics on home ownership. See Sōrifu Tōkeikyoku, *Nihon Tōkei Nenkan 1967*, pp. 434–35.

⁴⁴ The basic index for the effects of residence was —.304. When controls for group membership levels were introduced this was reduced to —.244.

The role of organizational involvement here is somewhat analogous to that reported for France by Sydney Tarrow in "The Urban-Rural Cleavage . . .," 353–355. But the consequence is still locally oriented instrumentalism in Japan in many instances, and this would appear to be different from the more general focus of rural pragmatism in the French example.

⁴⁵ I have followed the precedent set by Sidney Verba, Norman H. Nie, and Jae-on Kim, who differentiate between different forms of participation in "The Modes of Democratic Participation: A Cross-National Comparison," in Eckstein and Gurr, *Comparative Politics Series* (Beverly Hills: Sage Publications, 1971), Vol. II, publication number 01–013.

simply going out to vote.⁴⁶ In contrast, going out to vote depended more on feelings that the vote was an obligation, the perception that people in the neighborhood were going out to vote—a measure introduced in the analysis of turnout to try to tap feelings of social conformism—and to some degree interest in politics and reports of wanting something done for one's interests.

Once it was established how important such attitudes are for various kinds of participation, the effects of sectoral differences in attitude structures on participation were observed by controlling for the effects of each attitude and examining the residual impact of place of residence. Finally, in a general multivariate analysis, the remaining independent effects of each attitude and place of residence were indicated

⁴⁶ Possession of the various attitudinal perspectives was also a little less instrumental in motivating people to go out and ask others for their vote, a tendency that accords with the familiar mobilizational interpretation of participation in Japan.

after simultaneous controls for all confounding influences from other variables were eliminated. The results of both of these analyses are shown in Table 12.

According to these analyses, the attitudinal patterns in general contributed very little to reducing the effects of place of residence specifically on participation in national elections and politics. This finding suggests that the influence model referred to in earlier studies of sectoral voting trends in Japan does have some validity in explaining turnout in national elections and perhaps other kinds of participation in national politics as well. However, feelings of wants from national politics did contribute to the urban-rural participation patterns to some degree in regard to national voting turnout, attention to national campaigns and solicitation of others' vote in national elections.

Quite different patterns in the results were shown for linkages between attitudes and behavior at the local level. With the exception of solicitation of others' votes, *all* of the urban-ru-

Table 11. Political Culture and Political Participation, Bivariate Effect Indices

National Election and Politics								
	Interest	Concern	Responsiveness	Relevance	Comprehension	Wants	Civic Duty	Neighbor's Turnout
Turnout 1963	<u>.127</u>	.053	.067	— .013		.074	<u>.265</u>	<u>.133</u>
Campaign Attention	<u>.352</u>	<u>.265</u>	<u>.165</u>	<u>.203</u>	<u>.126</u>	<u>.227</u>		
Discussion of Politics	<u>.264</u>	<u>.250</u>	<u>.172</u>	<u>.200</u>	<u>.222</u>	<u>.161</u>		
Articulation of Wants	<u>.229</u>	<u>.217</u>	.079	<u>.113</u>	.059	<u>.163</u>		
Solicitation of Vote	<u>.133</u>	<u>.102</u>	.012	— .022	.010	<u>.111</u>		
Local Election and Politics								
Turnout 1963	<u>.112</u>	<u>.183</u>	.064	.060		<u>.160</u>	<u>.210</u>	<u>.258</u>
Campaign Attention	<u>.404</u>	<u>.275</u>	<u>.308</u>	<u>.246</u>	<u>.229</u>	<u>.285</u>		
Discussion of Politics	<u>.227</u>	<u>.195</u>	<u>.160</u>	<u>.169</u>	<u>.150</u>	<u>.151</u>		
Articulation of Wants	<u>.161</u>	<u>.143</u>	<u>.110</u>	<u>.225</u>	<u>.148</u>	<u>.163</u>		
Solicitation of Vote	<u>.068</u>	.033	— .016	<u>.081</u>	<u>.081</u>	<u>.082</u>		

Note: Figures are Coleman effect parameters or indices indicating the impact on the dependent behaviors of maintaining the various relevant attitudinal perspectives. Turnout and solicitation of the vote were simple dichotomous items; discussion of politics, articulation of wants and campaign attention were dichotomized from scalar measures. All attitudinal categories were also dichotomized, or in the case of wants from politics, based on dichotomous items.

As in earlier citations the results of tests for significance are indicated by underlining; no underlining means not significant at the .05 level, a broken line indicates significant at the .05 level and a solid line represents significance at the .01 level.

The decision to treat individual attitudes as independent variables in this and subsequent analyses, rather than aggregate them into scales where feasible, reflects simply a desire to trace through the effects of different single components of involvement and evaluative structures on behavior. Underlying formulation of this objective is the simple fact that attitudes were similarly treated in the unpublished secondary materials used as a source in much of the analysis, and important variations in sectoral tendencies between individual attitudinal dimensions could be seen in these findings.

Table 12. Political Culture, Place of Residence, and Political Participation, Multivariate Effect Indices

National Elections and Politics I									
	Resi- dence	Interest	Con- cern	Respon- siveness	Rele- vance	Compre- hension	Wants	Civic Duty	Neighbor's Vote
Turnout 1963	-.144	-.139	-.145	-.141	-.141	-.148	-.128	-.151	-.117
Campaign Attention	-.149	-.135	-.156	-.144	-.172	-.165	-.092		
Discussion	.011	.022	.006	.018	.009	-.010	.014		
Articulation	-.104	-.116	-.121	-.105	-.123	-.114	-.095		
Solicitation	-.103	-.099	-.105	-.103	-.107	-.106	-.076		
Local Elections and Politics I									
Turnout 1963	-.128	-.112	-.125	-.119	-.127	-.120	-.081	-.123	-.033
Campaign Attention	-.236	-.172	-.222	-.188	-.236	-.209	-.161		
Discussion	-.167	-.136	-.161	-.145	-.167	-.151	-.129		
Articulation	-.104	-.085	-.111	-.120	-.092	-.092	-.095		
Solicitation	-.097	-.088	-.097	-.105	-.097	-.088	-.080		
National Elections and Politics II									
Turnout 1963	-.128	.084	-.010	.030	-.068		.024	.142	.077
Campaign Attention	-.127	.218	.180	.061	.072	.015	.048		
Discussion	.010	.137	.182	.086	.053	.153	.098		
Articulation	-.156	.174	.214	.010	.041	.023	.083		
Solicitation	-.095	.091	.090	-.022	-.014	.002	.073		
Local Elections and Politics II									
Turnout 1963	-.016	-.042	.112	-.005	.006		.042	.152	.188
Campaign Attention	-.127	.230	.078	.107	.064	.039	.074		
Discussion	-.114	.111	.096	.003	.116	.002	.033		
Articulation	-.108	.016	.083	-.005	.202	.094	.078		
Solicitation	-.091	.000	.020	-.112	.081	.053	.052		

Note: The explanation for Table 11 also applies here. Numeral I indicates the results of calculations where the effects of place of residence were examined with each of the other categories controlled; the indices thus indicate the residual effects of place of residence. Under II, the results of a multivariate analysis of the effects of residence and the political culture attributes are shown. Comprehension was eliminated from these calculations involving turnout in order to add other relevant variables since the computer program used permitted handling of only eight independent variables.

ral differences in participation were at least partly related to the sectoral disparities in political cultural patterns I have reported earlier. Of particular importance for local participation in the rural areas were voters' concern for representation of wants, interest in politics and feelings that local politicians were responsive. While these linkages do not completely negate the possibility that community endorsements and exhortations to vote for a particular candidate might stimulate turnout and possibly other kinds of local participation, the sectoral attitude structures are still very important in the local context.

Finally, it can be clearly seen from the findings when simultaneous controls are introduced

that many of the attitudes still have residual impacts on behavior, at the same time that the statistical controls do not reduce the effects of specifically rural residence (shown by the negative correlations). What this means is that in addition to persons who participate in politics because they are motivated by feelings of interest or positive appraisals of public responsiveness, some rural residents go out to vote or participate on the basis of other influences besides the attitudes identified here. Fragmentary evidence from other Japanese studies has also shown this to be the case: interest in politics, for example, correlates highly with turnout in the city, but not at all in the country in some instances, simply because some politically apa-

thetic persons in the rural areas still go out to vote.⁴⁷ Obviously, this observation supplements those already advanced, and is more relevant for turnout than for other forms of behavior, and more relevant for national than for local contexts.

We have seen so far that there are some fairly complex patterns of urban-rural political culture differences in Japan, that these differences can be related to various aspects of community life and sociopolitical structure which vary between the cities and the country, and that the sectoral political culture differences correlate in certain instances with tendencies in political participation. At this point, I would like to summarize some of the analysis in order to consider the implications of these findings for empirical theory. Obviously, the discussion at earlier points was sufficiently complex that only certain salient conclusions can be discussed here. This is particularly true for my comments on the origins of sectoral political culture differences (even though it is also clear that some of the special interpretations of Japanese political culture might suggest profitable avenues for research in other places).

In the first place, while psychological involvement in at least national politics in Japan accords with the predictions about individual behavior extrapolated from social modernization theories of political development, political participation does not. Participation in national politics at the sectoral level in Japan was found to be ultimately more responsive to the mobilization potential of rural community life as far as can be seen from the present analysis. Turnout in a national election was especially dependent on this kind of influence or on the activities of candidates and local influentials to mobilize the vote.⁴⁸ Participation in national politics at the sectoral level is thus ultimately sensitive in an important degree to influences not predicted in the theory. No one would deny that the integration of rural Japan through some of the processes emphasized in modernization the-

ory has some indirect relevance for contemporary participation rates. But the major points are that the critical attitude-behavior linkages postulated from the theory are absent at the individual level and that less "modern" persons participate more than other categories in a significant number of cases.

Furthermore, exploratory parts of the analysis suggested that Lerner's critical media communication processes might have alienative as well as integrative effects, particularly for sectoral evaluations of political performance. These and other aspects of urban-rural differences are believed to have contributed to the greater pessimism found in the cities in Japan, although the various hypothesis tests using available data were not conclusive. While analysis of the evidence suggested that these sentiments were also less salient for identifiable participation levels than might have been anticipated, the potential importance of widespread pessimism and cynicism about politics in the modern sector of countries should not be ignored.

Modernization theory addresses itself mainly to postulates about individuals' responses to social processes which favor development of an awareness of cosmopolitan political affairs. Consequently, it doesn't seem appropriate to compare sectoral attitudes about local politics in Japan with the Lerner and Deutsch models. The center-periphery conceptualizations seem much more relevant at this point, once certain modifications are made in their formulations. In effect, it seems obvious that the multiple political and social centers at the local level in contemporary Japan constitute the reference point for identifying both social locations and dependent attitudes about local politics. Having said this, one can then appropriately observe that differences in community life and social involvement between the urban and rural districts are reflected in sectoral differences in the proximity of ordinary persons to these local decision making centers. These differences quite clearly favor the rural districts.

This reconceptualization of the center-periphery notion is certainly not a startling observation, but it represents a possibility largely ignored in the earlier discussions of these concepts. Moreover, it can have ultimate meaning for analysis of attitudes toward more cosmopolitan political arenas in places where the myriad of local political systems are closely linked with national processes. In Japan, linkage of this kind does exist in many cases through the intimate contacts maintained between local influentials and elected officials and national politi-

⁴⁷ An example is Aichi ken Senkyo Kanri Iinkai, *Senkyo Ishiki to Tohyō Kōdō Henyō* (Nagoya: 1963), p. 37.

⁴⁸ It is important to remember that the analysis and interpretation focuses on the differences between urban and rural residents not accounted for by the relevant attitudinal patterns. The reader should not forget the fact that many rural as well as urban voters were in fact psychologically involved. Still, more persons who turned out in national elections in the rural districts were not involved than was the case in the city.

Watanuki has presented a similar interpretation of rural participation in his "Social Structure and Political Participation in Japan." The findings were based on a path analysis of data from the Verba group cross-national study.

cians. While the effects of these kinds of relationships could not be analyzed directly, these links with national affairs may be an especially critical aspect of the integrational process by which rural voters are aware of at least the instrumental relevance of national political life. While "abstract" appreciations of national political life were highly associated with urbanization and education, rural pragmatism was the product of processes in which local political "centers" were sources of information and cues about narrow but concrete interests, representation for which was sought from national as well as local politicians. These feelings about local interest representation were the sole attitudinal component of sectoral political cultural differences relevant for the urban-rural tendencies in participation in national politics, thus underscoring the importance of the relationships between local centers and national processes in the Japanese case.

The findings on Japan have some relevance for the explanations advanced for voting participation and urban and rural living patterns in European settings. The greater transiency of urban residents in Japan certainly contributes to the observed sectoral differences in community and organizational involvement. This urban mobility still seems characteristic even of persons whose lifespans have excluded wartime experiences—people in the age cohorts potentially affected by the war could have moved solely as the result of the bombing of the cities. This finding clearly has consequences for political behavior, according to various Japanese studies; while the evidence is still fragmentary, longer residence is uniformly associated with higher involvement and participation levels, especially where local politics is concerned.⁴⁹

A second kind of mobility also found more often in the cities takes the form of commuting out to work, especially among men, who are

consequently more mobile than women. This relatively higher mobility has been shown in several urban studies to contribute to lower levels of involvement and participation in specifically local politics for men.⁵⁰ Since there is no direct evidence, however, on the relationship between commuting and behavior that would permit analysis of its impact on urban-rural attitudes and participation tendencies, this line of interpretation must remain untapped for the time being. Nevertheless, future research on the effects of commuting could contribute to further refinements of hypotheses about urban-rural differences.

The compact nature of rural communities and the physical proximity inherent in rural residential patterns in Japan also clearly parallel the characteristics reported for some European rural settings. Nevertheless, this compactness seems to have its primary impact on the character of local social structure rather than on the physical ease of voting. Indeed, the main import of the findings presented here is to call attention to the significance of sectoral community structure variations in the Japanese case. That these variations are in part manifested in urban-rural disparities in organizational involvement is also significant, given the emphasis in recent cross-national research on organizational influences on participation. What is salient in the Japanese case is that forms of social organization emerging from Japan's rural traditions contribute as much to attitudinal structures and behavior as do educational and communicational patterns in a modern society. In addition to the special and direct mobilizational implications of rural residence on political participation at the national level, rural community life contributes to attitudinal outlooks of major importance to the sectoral differences in local participation. Rural residence in Japan also contributes to an awareness of self-interest that underlies in part the patterns of sectoral participation at the national level. Obviously, comparable effects may be found elsewhere, and some of the apparent anomalies in the impact of place of residence could reflect the presence of such mediating influences in other societies.

⁴⁹ One of the best examples of this kind of relationship is found in Ōsaka fu Senkyo Kanri Iinkai, *Kōmei Senkyo Undō no Kōka Sokutei* (Ōsaka: 1961), 32, 44. Longer residence led in this case to heightened exposure to campaign communications and to higher voting levels in a House of Representatives election. However, the linkages between attitudes responsive to the effects of mobility and various kinds of political behavior were not examined. The implications of the study were also limited by the mainly urban composition of the sampled population. It should also be noted that length of residence in my own study did not prove to have a strong relation to the urban-rural differences in political culture, but this may have been due to the simplicity of the dichotomous measure which tapped only lifelong residence and its absence.

⁵⁰ This tendency is shown by figures reported in my "Political Behavior and Attitudes in Contemporary Japan," p. 39, and by various Japanese sources. For a qualitative assessment of women's comparatively greater involvement in local community life in some urban districts see Ezra Vogel, *Japan's New Middle Class*, pp. 102-13.

Aspects of Coalition Payoffs in European Parliamentary Democracies*

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One important aim of many political parties in parliamentary democracies is to obtain control of the governing apparatus of the state. Put another way, some parties seek to become governing parties.¹ If a party participates in government, not only do the psychological rewards of wielding power accrue to the party elite and its backbenchers, but also the party is in a position to use the power of the state to reward its friends and punish its enemies.

Normally, a party gains control of the government in one of two ways. Either an election returns a single party with a parliamentary majority, in which case that party usually governs alone, or it produces the result that no single party receives a majority, in which event a coalition of political parties commonly forms a government. At some point in the process of government formation, it becomes necessary to share out the rewards of office—to divide the pie among the winners, so to speak. How is this done—that is, who gets what reward as a result of winning a place in government? This question will occupy our attention in the remainder of the paper.

We have chosen to examine the problem using data drawn from thirteen European parliamentary democracies: Austria, Belgium, Denmark, Finland, German Federal Republic, Iceland, Ireland, Israel, Italy, Luxembourg, the Netherlands, Norway, and Sweden. Further, all of the cases selected concern only the division of rewards (or the payoff) in formal government coalition situations. The decision to con-

centrate only on coalition governments was taken largely because of considerations of data availability. In governing coalitions, the perquisites of government are usually divided among an unambiguous set of bargaining partners—the political parties represented in the coalition. In single party governments, while the payoff will often be divided among formal and informal factional groupings, these are somewhat less easy to identify.

The Nature of Payoffs in Cabinet Coalitions

If we are to formulate and test propositions relating to the distribution of payoffs in governing coalitions, we need some fairly precise understanding of what payoffs are. Essentially, payoffs are resources which are distributed among coalition partners and used by them to advance their individual goals. Viewed this way, governing coalitions are tactical alliances of political parties, each one of which is seeking to achieve its own set of goals by using the resources it obtains as a result of coalition membership.

What are the payoffs which accrue to parties which are included in governing coalitions? On the most elementary level, the payoff contained in any governing coalition is, abstractly, the government. The payoff, then, to individual coalition partners is membership in the government. While not ordinarily sufficient by itself, inclusion in the government may be quite an important payoff to parties seeking to achieve particular goals (e.g., parties seeking to legitimize their status as constitutional, or "pro-system," parties).

Most parties, however, surely seek and expect, in exchange for their participation in government, more tangible payments than merely the status benefits which attach to governing parties. Since most political parties have policy goals, such expectations will reasonably include payoffs in the form of policy prerogatives. The potential availability of such tangible benefits to coalition partners is evident since the government embodies the decision-making apparatus of the state.

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¹ It is worth pointing out that not all parties expect and/or seek to be included in a government. This is because their strategic position in the party system either makes inclusion unlikely (e.g., the British Liberals), and/or their ideological position precludes participation (e.g., the Irish Sinn Féin).

The value of policy payoffs to parties is obvious. Essentially, parties which exercise a measure of control over the policy output of government are in a position to influence the distribution of rewards (or values) within society in ways which they believe will advance their individual goals. The extent of a party's influence over such distributions will vary, of course, depending on the strength of its position as a coalition partner. But the crucial point remains that governing parties are able to call upon the resources of the state to advance their individual goals.

The possible policy payoffs available to partners in governing coalitions range in kind from the very diffuse to the quite specific. Perhaps the most diffuse of all policy payoffs might be the access accorded party leaders to all the various policy-making sites of government. Even if these leaders bear no formal responsibility for (or overt control over) policy made in specific issue areas, the opportunity for them to exercise personal influence on policy made in these areas is facilitated greatly by government partnership.

Another, more specific, way in which parties may exercise influence over the general flow of public policy is through a grant by their partners of what are known as "policy promises" (or, alternatively, policy vetoes) over certain legislative matters (e.g., a selective tax exemption or the construction of some public facility). In this case, the party, while not actually being given institutional control of the policy made in a given issue area, is allowed to control the content of specific policies in that area. Unlike mere access to policy-making sites, policy promises carry with them a contractual expectation of successful control, and a party may legitimately interpret failure of delivery as a breach of faith.

The most important policy payoff, however, is the distribution of public offices among the various governing parties. Here, each party is awarded one or more of the government ministries (and/or sub-ministerial posts) in exchange for bringing its parliamentary support to the government, thereby establishing its formal responsibility for and primary influence over individual policy areas. Government ministries are the most tangible manifestations of policy payoffs to governing parties in that they give parties an institutional base from which to attempt to influence the entire flow of public policy. Insofar as parties controlling ministries are responsible for specific policy outputs (policy making in specific issue areas), they are in a position to shape the content of governmental

legislative proposals to suit their own policy goals. In addition, control over policy in specific areas also gives parties bargaining capital to trade with parties controlling other issue areas, so that mutual accommodation will often profit both partners, and each will then expand its scope of policy influence. In short, the control of a given set of ministries confers upon a party a particular set of institutional resources which it can manipulate in an effort to advance its policy goals.

If by now we have a more or less clear idea of the kinds of payoffs which may accrue to governing parties, we have as yet no clear notion of the way in which they are distributed. How are payoffs to governing parties shared out? Are there any observable regularities influencing the distributions of the payoff, or, rather, are payoffs haphazardly distributed among the parties? If certain patterns of distribution do emerge, what variables might account for them? What patterns might we expect in payoff distributions to governing parties?

Payoff Distribution in Cabinet Coalitions

All empirical research is dependent upon the generation of hypotheses to give meaning to the quantities of data which are collected about any given subject. In this respect the study of political coalitions is in some ways facilitated, for the existence of a sophisticated body of theory concerning coalition formation provides a rich source from which hypotheses may be extracted and tested.² But unfortunately, these theories of coalition formation are vague and incomplete in their treatment of coalition payoffs. Nevertheless, the field is not altogether barren, and we may start our analysis with reference to one or two of the conclusions of these theories.

Generally speaking, coalition theorists conceive coalitions as being a joint use of resources by some defined set of actors (or players) to achieve a common goal. Using this definition, they attempt to build models of the coalition formation process which depend upon the assumptions of rational behavior as their princi-

² See, for example, William H. Riker, *The Theory of Political Coalitions* (New Haven: Yale University Press, 1962), Michael A. Leiserson, "Factions and Coalitions in One-Party Japan," *American Political Science Review*, 62 (September, 1968), 770-787, and William A. Gamson, "A Theory of Coalition Formation," *American Sociological Review*, 26 (June, 1961), 373-382. For methodological discussion and empirical illustrations of coalition formation, see also Sven Groennings, E. W. Kelley, and M. A. Leiserson, eds., *The Study of Coalition Behavior: Theoretical Perspectives and Cases From Four Continents* (New York: Holt, Rinehart and Winston, 1970).

pal motivation component. As such, coalition formation is viewed as a payoff maximization game in which all players are assumed to be actively seeking inclusion in some winning coalition which will yield them their greatest payoff. The "solution," or outcome of the formation process is predicted to be that winning coalition (among the many possible winning coalitions) which yields to *all* individual participants their greatest payoff. Further, once players discover this optimal coalition, they will be constrained by the situation from demanding an excessive payoff from their partners, since such behavior would likely result in their exclusion from the coalition in favor of some other less greedy player.

While conceiving the problem in this way has led to some important propositions about the processes of coalition formation, very few of these are directly concerned with specifying the exact nature of payoff distributions. Since the assumption of payoff maximization behavior is central to these theories, the lack of derived propositions about payoff distributions constitutes a serious weakness in their explication.

Of the three theories of coalition formation surveyed (those of Riker, Leiserson, and Gamson), only Gamson's makes an unambiguous prediction about the distribution of the payoff. Riker maintains that payoff distributions are in some way a function of the "weight" of the various partners in a winning coalition. He declines, however, to specify the exact meaning of "weight," and he does not indicate how such a concept might be utilized in the prediction of payoff distributions.³

Leiserson deals with payoff distributions in two ways. First, he states that payoffs to partners in a winning coalition will be directly proportional to their "weights." While he does not specifically elaborate the concept of weight, his own application indicates that weight is defined by the seat/vote resources of the players expressed as proportions (the specific reference is identified factional groupings in the Japanese Liberal Democratic Party) plus certain "intangible factors," which include the extent of financial backing and subjective assessments of support from nonaligned Dietmen.⁴

Second, Leiserson attempts to deal with the question of which payoffs (in this case government ministries and high party posts) accrue to

which partners (LDP factions). This assignment is accomplished by making a complex series of calculations which involve consideration of an ordinal set of payoffs, certain maintenance considerations involving strategy preoccupations whereby partners are constantly reassessing their positions vis-à-vis the upcoming or next coalition formation, and, of course, the weight of individual players including the "intangible factors."⁵ The main problem with Leiserson's treatment is that the criteria established for the measurement of his major variables are based for the most part on the subjective interpretations of the analyst. This in turn poses grave problems of comparability across coalition situations and makes replication of his technique virtually impossible. In short, Leiserson's contribution to the explication of the process of payoff distributions is minimal, since his treatment does not adequately meet the requirements of generalized theory.

The most straightforward proposition concerning coalition payoffs derives from Gamson's theory of coalition formation. Gamson seeks to predict discrete coalition formation outcomes by specifying certain conditions which must be present for a *bona fide* coalition situation to exist (an essential game) and by postulating certain behavioral assumptions about actors in these situations.

The major conditions which operate in coalition situations according to Gamson are as follows:

- (1) That there is some initial distribution of resources among the actors which aid them in bargaining.
- (2) That there is some payoff for the successful formation of a winning coalition and that this payoff is the same regardless of which of the possible winning coalitions actually forms.
- (3) That all actors have ranked preferences among the other actors as prospective coalition partners based on criteria other than their respective resource holdings (nonutilitarian strategy preferences).
- (4) That there be an "effective" decision point at which a winning coalition is able to control decisions (a point at which a losing coalition becomes winning).⁶

Given these conditions, Gamson proceeds to make one major assumption relating to the level of information present in the game:

All actors have the same information about the initial distribution of resources and the nature of the payoff.⁷

³ See Riker, especially Chapters 5 and 6 where he discusses the nature of side payments and indicates that smaller players are likely to be more advantaged in the distribution process than "weightier" ones.

⁴ Leiserson, p. 774.

⁵ Leiserson, pp. 777-782.

⁶ Gamson, pp. 375-376.

⁷ Gamson, p. 376.

Having reached this point, Gamson states what he calls "the general hypothesis" of his theory. This is:

*Any participant will expect others to demand from a coalition a share of the payoff proportional to the amount of resources which they contribute to a coalition.*⁸

This hypothesis, of course, also serves Gamson as an additional behavioral assumption, for without it he is unable to predict coalition formation outcomes. With it, however, Gamson is able to predict that "the cheapest winning coalition" will form. The reason for this is explained by Gamson thus:

When a player must choose among alternative coalition strategies where the total payoff to a winning coalition is constant, he will maximize his payoff by maximizing his share. The theory states that he will do this by maximizing the ratio of his resources to the total resources of the coalition. Since his resources will be the same regardless of which coalition he joins, the lower the total resources, the greater will be his share. Thus, where the total payoff is held constant, he will favor the "cheapest winning coalition."

We may observe immediately that Gamson's "general hypothesis" (read assumption) not only allows us to predict coalition formation outcomes, but it also allows us to predict payoff distributions in an unambiguous, generalizable manner. Gamson is plainly stating that *payoffs to partners in winning coalitions will be proportional to their resource contributions*. This, if verified, is a theoretically meaningful proposition about coalition payoffs, and it will serve as the point of departure in our analysis of payoffs in European cabinet coalitions.

While Gamson has offered us a suspected relationship, he has not formulated it in such a way as to admit verification. What is a payoff? What does Gamson mean by the amount of resources parties contribute to the coalition? The first task which confronts us, then, is operationalizing both of these variables.

We have already dealt in a general way with the concept of payoff. Now we need to know how we can measure the amount of payoff received by each coalition partner. It must be obvious that there is no entirely satisfactory way to do this. If, as we have suggested, payoffs in governing coalitions vary in their content from the specific to the diffuse, we have little hope of being able to specify the value which attaches, for example, to something as conceptually murky as the ease of access to decision-making sites. Indeed, the only payoff to parties which is

at all well documented is the actual distribution of ministries and subcabinet posts.

Leiserson, in his work on factionalism in the Japanese Liberal Democratic Party, has recognized this difficulty of specifying the content of payoffs, but has at the same time minimized its importance. He has argued that, while there may be several kinds of payoffs available to coalition partners, all of them are "... roughly equivalent to, or are reflected in the acquisition of Cabinet and high party posts."¹⁰ This view is justified by Leiserson's belief that parties will not accept diffuse payoffs without tangible evidence of good faith from their bargaining partners. The expectation, then, is that negotiating coalition partners will expect and insist on their fair share of ministries *in addition to whatever other payoffs are distributed*. This strategy will ensure that each party in the coalition retains continuing control over its tangible payoffs even in the event that its more intangible payoffs are for some reason not forthcoming. Because we accept this argument, we shall, for the purpose of analysis, take ministerial shares to partners as constituting our indicator of the level of payoff to partners.

This leaves us to deal with the problem of specifying the nature of party resources. What contributions do parties make which merit their inclusion in governing coalitions? The concept of party resources, like that of payoff, is complex, because parties are capable of bringing a wide variety of resources to a government. For example, some parties may list among their parliamentary leadership certain eminent and popular political figures who may be valuable to a government wishing to enhance its standing among the population at large. This kind of resource might be seen as valuable to some governments, particularly in times of crisis. The presence of such legitimacy-conferring personnel, then, may have an important bearing on the chances for a party's inclusion in government on terms which might be seen as favorable. In our universe perhaps the best example of this effect is the presence of the Rafi Party in recent Israeli cabinets. Here, the commanding figure of Moshe Dayan undoubtedly accounts in large part for his party's present status.

Another resource which parties contribute is their cooperation with rather than opposition to the government. While all parties contribute this resource when they join a government, some are in a better position to profit from it than others. This is because it will be important

⁸ Gamson, p. 376.

⁹ Gamson, p. 376.

¹⁰ Leiserson, p. 778. Note: Leiserson's reference to the receipt of "high party posts" is only germane to factional bargaining and should not be considered appropriate in our context.

to the government to neutralize the opposition of some parties, while it can function perfectly well with the opposition of others. Again the Israeli case furnishes us with our best example. Here, the religious implications of a Jewish state have made the religious parties potentially a very effective opposition force. That is, a religious opposition in Israel carries with it the potentiality of opening a serious religious/secular division in society which would probably produce bitter and protracted governmental and legislative crises, and perhaps ultimately threaten the constitutional foundation of the state. The Israeli response to this situation is evident in the inclusion of the National Religious Party (and its predecessor, the United Religious Party) in every cabinet since the founding of the Israeli state. In effect, then, what the dominant governmental party, Mapai, has done is to buy off religious opposition by co-opting it into the government.

But the most obvious, and probably the most important, set of resources a party brings to the government is its share of parliamentary seats, which may then be translated into votes on government policy. If the government is to survive as a viable decision-making unit, it must maintain a stable law-making majority in parliament. This majority is normally (though not necessarily) achieved through the combined voting strength of the coalition partners. Hence, if each partner is generally satisfied with the content of public policy, the government will continue to function through the instrument of its parliamentary majority. If, on the other hand, one or more of the partners becomes dissatisfied with the government's performance, it may decide to withhold its votes, thereby jeopardizing the continued existence of the government.

It may be argued that the various resources contributed to the government are "roughly equivalent to, or reflected in" the voting strength of the governing parties. This is exactly analogous to the case of ministries and payoffs. In the final analysis, a government can withstand every kind of adversity except the loss of its parliamentary majority. For example, if a coalition crisis is precipitated by the withdrawal of a partner which had contributed a large measure of, say, legitimacy (Moshe Dayan in our earlier example), one of several outcomes are possible. Three of the most obvious are:

- (1) the government falls due to the loss of its law-making majority (withdrawal of the party's necessary parliamentary votes),
- (2) the government falls because other partners also withdraw from the coalition feeling that

government continuation without the party in question is intolerable.

- (3) the government continues because even without the party in question, the government maintains its necessary majority.

In two of these cases, the government falls not because of the loss of legitimacy but because it ceases to have a law-making majority. Of course, the government may well have lost its law-making majority because it ceased to be legitimate, but that only means that legitimacy, in this case, is reflected in the voting strength of the party. In the third case, the government continues because, even though it loses the party's contribution to legitimacy (and its votes), this loss is not so severe as to bring down the government. In view of this "life-and-death" property of voting strength (the ability of parties, through the use of their votes, to insure either the continuation or the fall of a government), we have decided to take it as our measure of party resources, or contribution to the coalition.

An Empirical Test of Gamson's Proportionality Proposition

At this point we are ready to reformulate Gamson's proportionality proposition in operational terms:

The percentage share of ministries received by a party participating in a governing coalition and the percentage share of that party's coalition seats will be proportional on a one-to-one basis.¹¹

The manner in which this proposition predicts distribution outcomes can be shown easily with the aid of the following table.

Table 1

Party	Seats	Possible Winning Coalitions	Proportional Payoff		
			A	B	C
A	45	A, B	.56	.44	.00
B	35	A, C	.75	.00	.25
C	20	B, C	.00	.64	.36
		A, B, C	.45	.35	.20

Here, the predicted percentage payoff to each partner is found by dividing its seat holding by the total number of seats represented in the particular coalition.

¹¹ Notice that we have altered Gamson's proposition to postulate that the proportionality must be one-to-one. Naturally, a relationship may be proportional on any basis, 1:1, 2:1, 3:1, etc. It was obviously Gamson's intention that the relationship be 1:1 since any other interpretation is contrary to the logic of his theory.

Although logically we are now ready to conduct an empirical test of an important proposition about ministerial distributions, we shall do well first to consider the implications of the suspected relationship. As it is presently formulated, the proportionality proposition will, when tested, provide us with information which is strictly quantitative: a party will receive a given number of ministries for the contribution of a given number of seats. But while this quantitative dimension is certainly important in our understanding the distribution process, it is clearly not the only dimension in which distributions are taking place. In addition to bargaining over how many ministries each party is to receive, coalition partners are also interested in which specific ministries they are. If, for example, the proportionality equation demands that a party receive two ministries for its contribution to the coalition, it will surely make a difference to that party whether the ministries it receives are, say, the Prime Ministership and the Ministry of Finance, or if they are Sport and Tourism. In addition to a quantitative dimension operating in the bargaining context, then, we may reasonably expect that a qualitative one operates as well.

If bargaining between coalition partners takes place in two dimensions as we have suggested, what might the consequences be? The answer to this question depends largely on the degree to which the dimensions are interrelated. It is quite plausible, for example, that if the Prime Ministership is worth considerably more to a party than any other ministry, that party might be willing to compensate its coalition partners for receiving this prize by accepting less than its proportional share in the distribution. If this behavior is common, we are placed in the difficult position of having to decide whether: (1) the party was underpaid in the quantitative dimension because it was able to receive more than its due in the qualitative dimension, or (2) that no norm of perfect proportionality governs the distribution process. In this latter case, the party in question may in some sense really have received its "fair share," based not on proportional distributions but on such other factors as blackmail and bargaining skill.

One indication of the possible relationship of the two dimensions may be gained by testing the proportionality proposition. Suppose that over the entire universe, we find a symmetrical distribution such that the percentage of seats in the coalition held by parties and the percentage of ministries they receive genuinely approximate 1:1 proportionality; then we will be justified in concluding that the particular ministries

a party receives has a negligible effect, if any, on the number it receives. This would not necessarily mean that ministries are not valued differently by the parties. Surely they are. Rather, what perfect proportionality would tell us is that, even though parties consider some ministries more important than others, they are unwilling to pay for the receipt of ministries they want by accepting less than their fair share of the distribution taken as a whole. On the other hand, any remaining variations unexplained by the proportionality proposition will suggest that some of these variations were in fact due to bargaining over specific ministries in the manner postulated above.

Let us now proceed to test the proportionality proposition. The data are drawn from the thirteen parliamentary democracies already listed, and consist of the proportion of seats contributed by each of 63 parties to each of the 114 coalitions in which they participated between 1945 and 1969, together with the proportion of ministries they received in return.¹² The proposition, it should be noted, suggests two features of the data which are predicted to occur simultaneously. These are that there should be a high positive relationship between the two variables (if one party contributes more seats than another, it should also receive more ministries), and that this relationship should be proportional on a one-to-one basis. Because the postulated relationship is linear, both predictions can be tested by means of a single-stage least-squares regression analysis, with its associated Pearson's product-moment correlation coefficient.¹³ These analyses will establish how closely the individual cases, taken together, conform to a best-fitting line that could be drawn through the plotted coordinates of the variables, and will identify the location and slope of this line. The expectation to which the prediction gives rise is that all of the coordinates will fall on a forty-five degree line inter-

¹² Regarding each party in each coalition-forming situation as one "case" gives us 358 such cases. However, 32 of these cases in fact represent ministerial distributions made to individuals who served in a non-party capacity. The proportionality proposition is not relevant to these cases, and they were therefore excluded. Of the remaining 326 cases, two represent parties that received no ministries at all in return for the seats they contributed. These were parties which lent support to a coalition during a period of crisis, while refusing the responsibilities of office. Since it was not the bargaining process that led them to receive no ministries, these are cases where the proportionality proposition is again not relevant; these were again excluded, leaving us with a final universe of 324 cases.

¹³ These techniques are so well known as to require a minimum of documentation. The computing formulae used were taken from William L. Hays, *Statistics* (New York: Rinehart and Winston, 1963), 490-538.

secting the axes at their origin. This means (a) that the correlation coefficient (Pearson's r) should approach its best possible value of 1.0 (indicating that none of the plotted points deviate at all from the best-fitting line), (b) that the slope of this line should be close to 1.0 (indicating that a movement of one unit along one axis corresponds to a movement of one unit

along the other), and (c) that the vertical displacement of this line in relation to the origin (the intercept point) should be close to 0.0 (indicating that there is no systematic bias in the relationship in that no seats yield no ministries). In short, we expect a regression equation of $Y = 0 + 1.0 X$ which (in the statistical sense) explains all the variance in Y .

ALL CASES EXCEPT NON-PARTY

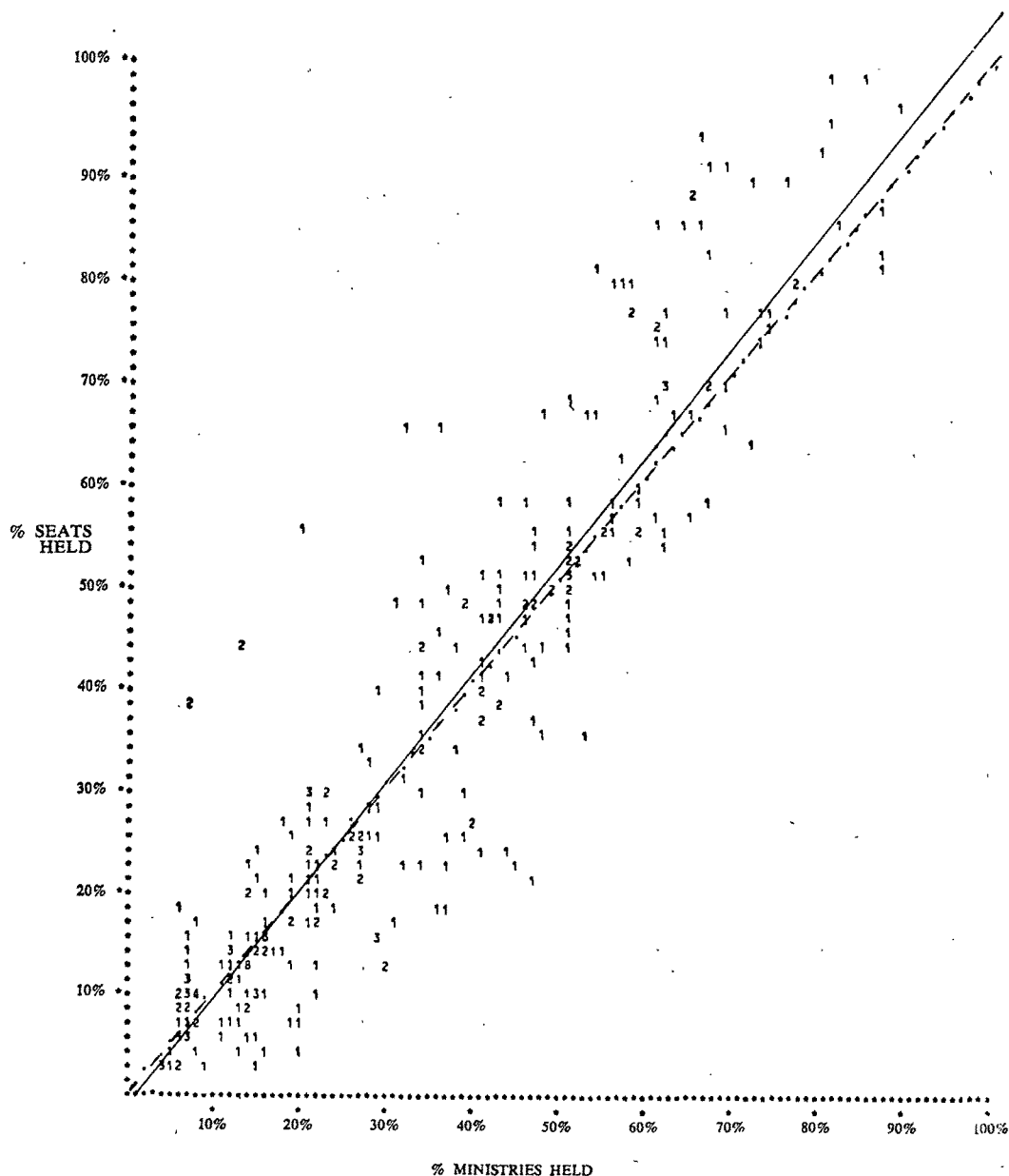


Figure 1. Seat Shares Plotted Against Ministerial Shares.*

* Note: Each of the numerals pictured represents the number of points occupying the space upon which it stands; 324 cases included in this plot.

We shall begin by reporting the strength of the relationship found, since if the variables had not been related the question of proportionality would have retained little theoretical interest. In fact the correlation coefficient over the whole universe of 324 cases examined attains a value of 0.926. This means that, of all the variance in the percentage of ministries received,¹⁴ 85.5 per cent was explained by the variation in the percentage seats contributed. Concerning the question of proportionality, we find that the best-fitting line calculated for these cases has an equation of

$$Y = -0.01 + 1.07 X$$

(Equation 1)

which is very close indeed to the relationship predicted by the proportionality proposition ($Y = 0.0 + 1.0 X$). The observed relationship is presented visually in Figure 1, which is an ordinary scattergram upon which has been superimposed a continuous line, representing the least-squares regression line, and a broken line drawn at forty-five degrees to each axis, indicating the coordinates that would have exactly corresponded to proportionality on a one-to-one basis. The extent to which these two lines approximate each other, and the extent to which the observations cluster about them can be readily appreciated.¹⁵

From a research point of view, this result is quite surprising, if not staggering. Seldom in the social sciences does one find a simple bivariate relationship which is both theoretically meaningful and also able to explain such a high proportion of variance. Only 14.5 per cent of the variance in the dependent variable remains to be explained by all other factors whatsoever. It can therefore be affirmed, with few reservations, that the number of ministries received by partners in a governing coalition is indeed explained, almost on a one-to-one basis, by their contribution of parliamentary seats to that coalition. This relationship is so strong that it would seem to preclude any qualitative exchanges of the kind postulated earlier. Parties may indeed value one ministry more than another, but it is evidently not customary for them to exchange one valued ministry for several others.

¹⁴ The values taken on by both variables in fact vary all the way from 1 per cent to 99 per cent.

¹⁵ Readers who are left unimpressed by correlation coefficients and regression equations are invited to apply to Figure 1 what has been named the "interocular trauma test": a relationship is significant if it hits you between the eyes. See Edward Tufte, "Improving Data Analysis in Political Science," *World Politics*, 21 (July, 1969), 641-654.

Explanations of Deviations from Proportionality: The Relative Weakness Effect

Multivariate analysis of data in social science often takes the form of maximizing the variance in some dependent variable that can be explained by some combination of independent variables. But in a situation such as ours, where all but 14 per cent of the variance has been explained by a simple bivariate relationship, a search for additional variables to add statistical explanatory power does not appear particularly fruitful.¹⁶ Hence, in the remainder of this paper we shall not pursue this research strategy, but rather concentrate our efforts on attempting to determine why this relationship differs from proportionality on a one-to-one basis.¹⁷

Perhaps the most interesting characteristic of our observed relationship is a marked tendency for small coalition partners to do better in the exchange process than do large ones. That is, the deviation from one-to-one proportionality has the effect of causing small parties to receive additional or "bonus" ministries to be distributed above their proportional shares, while large parties tend to surrender such ministries. This we shall call the "relative weakness effect."¹⁸ (Scan the scatterplot for visual confirmation of this tendency.)

¹⁶ For example, an additional independent variable which might be considered potentially fruitful in explaining additional variance is a partner's relative size or position in the coalition. Regardless of its absolute size, the largest coalition partner might be able to exercise exceptional influence over distributions, relative to his other partners. When relative size was treated as an interval-level variable and correlated against ministerial distribution, controlling for absolute size, it yielded a correlation coefficient of -0.25. Yet, when brought in as an additional term in the basic regression equation already presented, the total of the variance explained rose by only 1 per cent.

¹⁷ In another context, Hubert Blalock has argued that correlation coefficients are affected by factors that may be considered "accidental" to specific populations, so that in stating general laws or comparing populations (both of which we do in this paper) attention should be concentrated rather upon the more stable regression coefficients. H. M. Blalock, Jr., "Causal Inference, Closed Populations, and Measures of Association," *American Political Science Review*, 61 (March, 1967), 130-136.

¹⁸ The relationship is not quite reflexive. The displacement coefficient of -0.01 tells us that the smallest parties in general only contribute about 1 per cent fewer seats than they receive ministries, while the slope coefficient of 1.07 tells us that the very largest parties can expect on average to contribute about 6 per cent more seats (seven per cent more than the smallest parties) than they receive ministries. (See Equation 1.) Because there are many more small parties than there are large ones, however, the difference averages out, and about as many parties gain somewhat from the bias (155) as lose (166).

The extent of the relative weakness effect can be demonstrated more effectively if we create a new variable (called the index of overpayment) by subtracting the proportion of seats contributed by a party from the proportion of ministries received by that party. This index will be negative when the party contributes more seats than it receives ministries, and positive when it receives more ministries than it contributes seats. The more positive it is for any party, the higher the proportion of bonus ministries among that party's portfolios. The fact that it is small parties on the whole that receive bonus ministries and large parties that surrender them is demonstrated if this index is correlated against party size. Such an analysis yields a coefficient (Pearson's r) of -0.54 , showing that the smaller the party the greater the overpayment.

How can we account for the success of small parties in extracting bonus ministries from their larger partners? In fact, the literature on coalition theory suggests that small partners will be disproportionately advantaged in terms of their strategic position in coalition situations.¹⁹ Two explanations consistent with these theories will be advanced. First, a small party may be more likely to be a "pivotal partner" in the sense that its addition to a coalition will be sufficient to cause the coalition to win. Since this addition is at least psychologically more crucial to the success of the coalition than the addition of any other partner, that party will be in a position to extract a high price for its entry. Second, small parties may be more desirable coalition partners than larger parties because they are not as likely to be able to inhibit the activity of their larger partners, at least not to the extent that might be possible for other sized parties.

The first explanation will not be tested here because our data are not able to substantiate it in any way, and because it is at least logically possible, if not probable, that pivotal parties may be of any size instead of the implication that they will be predominantly small. We leave the possibilities of this explanation to further research. The second explanation is more interesting and can be inferred from our data. The logic of this second argument leads to the pre-

diction that largest parties will characteristically be avoided as coalition partners. We shall have to reject this implication of the second explanation also, since it appears to have no basis in observed reality.²⁰ This evidence, however, does not prejudice the notion that small parties are more desirable than larger ones as coalition partners.

The line of argument we wish to pursue states that coalition formation outcomes characteristically produce a set of partners in which one (or occasionally more than one) of the partners assumes a position of overall coalition leadership (it is responsible for the overall direction of the government), while others assume less prominent positions in general policy direction. Since coalition leaders are expected to control a large number of ministerial portfolios (in general a large plurality and often far in excess of an absolute majority), and since their position ensures them effective control over the flow of public policy, we might expect that the surrender of one or two ministries (bonus ministries) at the margin of coalition control would not seriously detract from this position. Further, we expect that coalition leaders will be willing to surrender these ministries in order to obtain the necessary support to form a viable government. In short, coalition leaders probably can afford or tolerate the cost of surrendering bonus ministries to small partners.

For the small parties, the situation is quite different. Since they cannot expect to play a dominant role in policy direction, it is reasonable to expect that small parties will seek to expand their area of influence at all costs. What we are arguing here, essentially, is that the gain of one ministry by a small partner is *not* equal to the surrender of that ministry by a coalition leader (all other things being equal). The reason for this is obvious. If a party holds one portfolio and receives another as a bonus, its ministerial share has increased 100 per cent. If, on the other hand, a coalition leader holds nineteen portfolios and surrenders one to the small party, its holding is diminished by only 5 per cent. Consequently, it is reasonable to expect that small parties will be eager to expand their payoff share to compensate for their relatively weak position in general policy control. By the same token, coalition leaders should be willing to surrender a portfolio or two in order to obtain the support of a small coalition partner which is no threat to their leadership position. What has occurred, we are arguing, is that

¹⁹ It is impossible in a footnote to demonstrate the manner in which the major coalition theorists arrive at this conclusion. We may say, however, that in general, large parties appear to suffer under certain limitations which are imposed on them *because* of their size. See, for example, Theodore A. Caplow, *Two Against One: Coalitions in Triads* (Engelwood Cliffs, N.J.: Prentice-Hall, 1969), and Anatol Rappoport, *N-Person Game Theory: Concepts and Applications*, (Ann Arbor, Mich.: University of Michigan Press, 1970).

²⁰ In fact, more than 85 per cent of the coalitions in our universe included the largest party represented in parliament.

the coalition situation has created a bargaining advantage for small coalition partners which allows them to demand and receive bonus ministries so long as they remain unable to challenge effectively the overall policy control of coalition leaders.²¹

Limitations on the Relative Weakness Effect

While our data have shown that the relative weakness effect operates generally in the context of ministerial distributions (small parties do in fact tend to receive bonus ministries), it is not obvious that it operates uniformly in all coalition situations. In other words, are there particular sets of circumstances where the demands of small parties are less likely to be successful?

If, as we have argued, the bargaining context affords smaller parties an advantage in their dealings with coalition leaders, it might reasonably be hypothesized that this advantage will be variable according to the size of (number of parties in) the coalition. Thus we would expect that small parties will be most successful in extracting bonus ministries when there are relatively few parties represented in the coalition (two or three) and less successful as the number of coalition partners increases (four or more).

The reason for this expectation follows directly from the argument already advanced about the receipt of bonus ministries in the general case. If coalition leaders are disposed to reward their smaller partners with bonus ministries as long as such behavior does not significantly jeopardize their leadership positions, they are most likely to do so when the size of the coalition is small. This hypothesis can be supported by two lines of argument and tested with our data.

The first argument for the validity of the hypothesis may be expressed by the following syllogism:

- (1) The fewer the small parties in a governing coalition, the fewer the bonus ministries that will have to be distributed.
- (2) The fewer the bonus ministries distributed, the more likely it is that coalition leaders will maintain effective control over the flow of public policy.
- (3) Therefore, the fewer the small parties in the coalition, the more likely is the coalition leader to maintain control over the flow of public policy.

²¹ We do not wish to advance this explanation as definitive since it is inferred from our data rather than being tested by them. Instead, we offer it as a tentative explanation which awaits further testing.

This argument needs some explication. Since we already have established that the overwhelming number of bonus ministries are in fact distributed to small parties, the first premise has been demonstrated to be the case. The second premise depends upon the assumption that coalition leaders maintain control over the policy process through the control of ministries. Since the size of each party's ministerial holding is finite, each ministry surrendered to other parties by coalition leaders propels them toward some critical point where they will lose effective control over the policy-making process. Therefore, the implication of the conclusion to the syllogism is that coalition leaders will be progressively reluctant to surrender bonus ministries as their rate of distribution increases. Consequently, coalition leaders will be more likely to distribute bonus ministries to small parties when only a few distributions are necessary than would be the case in large coalitions where the distribution of more bonus ministries would likely be necessary.

The second line of argument states that the greater the number of parties in coalition (regardless of the sizes of individual participants), the less likely are coalition leaders to agree to distribute bonus ministries. The reasoning behind this hypothesis may also be presented in the form of a syllogism.

- (1) The more parties comprising a governing coalition, the less likely coalition leaders are to maintain effective control over the flow of public policy.
- (2) The less this control by coalition leaders, the less likely they are to surrender their instruments of control (ministries).
- (3) Therefore, the more parties comprising a governing coalition, the less the likelihood of coalition leaders agreeing to distribute bonus ministries.

This argument also requires explication. The first premise seeks to establish a linkage between the number of coalition partners and the maintenance of control over policy by the coalition leader. Implicit in this relationship is the assumption that intracoalition agreement (and hence greater opportunities for mutual adjustment) is relatively more difficult to achieve with a large rather than small number of actors.²² Where mutual adjustment is difficult, re-

²² Leiserson makes much the same point when he adds to his theory of coalition formation a "bargaining proposition" which states that in the universe of minimal winning coalitions, smaller rather than larger ones will form *regardless of which is closer to the decision point*, since bargaining difficulties among large numbers of partners are dysfunctional to the viability of the coalition.

quiring a great deal of bargaining and compromise of policy positions, the level of control over the policy process by coalition leaders should diminish.

The second premise seeks to establish a linkage between the level of control over the flow of public policy and the ministerial share of partners. Implicit here is the assumption that control of policy is exercised through the ministerial holding of parties in governing coalitions. Since we have assumed that coalition leaders are highly motivated to control the flow of public policy (i.e., to direct the policy of the government), it follows that, in situations characterized by intracoalition disagreement, coalition leaders will be more reluctant to surrender their instruments of overall policy control than would be the case in situations exhibiting a higher degree of intracoalition agreement. If both these premises are tenable, it then follows that coalition leaders are less likely to agree to distribute bonus ministries when coalitions are large (operationally defined as comprising four or more parties) than when they are small (two or three parties).

In sum, we expect to observe that the relative weakness effect (small parties are disproportionately advantaged in payoff distributions) will operate in small coalition situations. As the size of the coalition increases, however, this advantage to small parties should diminish demonstrably. In such coalitions, there should be no relationship between size and the index of overpayment, and the observed regression slope relating the number of ministries to the number of seats contributed should decline toward one-to-one proportionality.

Table 2 summarizes the relationship between coalition size and ministries received for the five different coalition sizes actually found in our universe (coalitions of five and six parties have been taken together because there was only one coalition of six parties). Although

there are differences between the coefficients, the pattern is quite clear. Where there are two or three parties in a coalition, the slope coefficients are markedly higher than in the universe as a whole (which yielded a coefficient of 1.07), and the displacement coefficients are markedly lower than in the universe as a whole (-0.01). On the other hand, where there are more than three parties these characteristics are reversed; the slope coefficients decline to just below 1.0, and the displacement coefficient becomes positive.

Because of the rather low correlation found in the two-party situation, and the small number of cases comprising the five and six party situations, it is probably safest to aggregate together all small coalitions on the one hand, and all large coalitions on the other. When this is done, the regression equation describing the relationship between seats (Y) and ministries (X) in the case of small coalitions is:

$$Y = -0.05 + 1.16X \quad (\text{Equation 2}),$$

which explains 83.8 per cent of the variance in Y ($r = 0.915$), while for large coalitions it is:

$$Y = +0.02 + 0.97X \quad (\text{Equation 3}),$$

which explains 83.9 per cent of the variance in Y ($r = 0.916$). This means that any small party in a small coalition can expect to contribute some seven per cent fewer seats in exchange for one ministry than a small party in a large coalition. If we consider a hypothetical government with as few as twenty ministries, a small party in a small coalition can expect to receive one ministry more than would have been its due on the basis of strict proportionality (a bonus ministry). On the other hand, a very large party in a small coalition might expect to contribute some ten per cent more in terms of seats than it would receive in terms of

Table 2. Relationship Between Ministries and Seats for Coalitions of Various Sizes

Number of parties in coalition	Number of coalitions this size	Number of parties (N)	Variance explained	Correlation (Pearson's r)	Displacement (a)	Slope (b)
Two	54	114	77.6%	0.881	-0.05	1.12
Three	24	72	88.3%	0.940	-0.07	1.26
Four	26	104	81.0%	0.900	+0.02	0.98
Five & six	7	34*	96.5%	0.982	+0.02	0.92

* Two parties receiving no ministerial payoff excluded.

Table 3. Average Differences From One-to-One Proportionality Sustained Over All Parties

Size of coalition	Parties gaining in the exchange of ministries for seats		Parties losing in the exchange of ministries for seats	
	N	Mean overpayment	N	Mean underpayment
Small (2-3 partners)	90	+0.07	91	-0.10
Large (4 or more partners)	76	+0.03	64	-0.06

ministries. In our hypothetical government this would amount to a loss of two ministries.²³

In large coalitions the relative weakness effect (the underpayment of large parties and overpayment of small parties) seems not to operate at all. The small deviation from one-to-one proportionality in the equation for such coalitions is in the opposite direction from that in the universe as a whole, but this deviation is almost certainly an artifact of the least-squares estimating procedure adopted, which underestimates the slope coefficient to some extent when a correlation is not perfect. When the index of overpayment in terms of ministries for seats (calculated by subtracting the latter from the former, as explained earlier) is correlated against size of party, the coefficient (Pearson r), which reaches a value of -0.66 in the case of small coalitions, vanishes entirely in the case of large coalitions. Small parties in coalitions of four or more members are no more likely to receive bonus ministries than are large parties in these coalitions.

It follows from all this that if we were trying to predict the probable payoff in terms of ministries that would be received by a particular party's joining a governing coalition, we really need to know only two facts: (1) the size of the party and (2) the size of the coalition it was intending to join. From the first of these facts we could estimate the number of ministries the party might expect to receive if proportionality on a one-to-one basis were to obtain, and from the second (in conjunction with the first) we could estimate the extent to which

the party was likely to do better or worse than this in the distribution. By knowing the exact number of seats a party holds, payoffs in terms of ministries can be estimated by applying the appropriate regression equation from Table 2.

Of the two factors (size of party and size of coalition), the most important is without doubt the size of party. The regression lines only differ from one another to any marked extent at their extreme ends, and few parties are either large enough or small enough to feel the full effects of this bias. Nevertheless, it can be shown that the average extent to which different parties gain and lose is quite large. Table 3 shows the average extent to which underpaid parties suffer, and the average extent to which overpaid parties gain, in the exchange of ministries for seats.

It can be seen that parties in large coalitions differ from one another to the extent of about one ministry in ten and, because we have discovered no trace of the relative weakness effect in such coalitions, these differences may be regarded as disturbances unexplained by the effect (in this analysis we treat them as random disturbances, although of course their causes may be sought in future research). In small coalitions, parties differ from one another to the extent of about one ministry in six, and the difference between this situation and that pertaining in large coalitions may be regarded as indicating the stakes of the bargaining process, which are not small.

The thrust of the two syllogisms we presented earlier was to regard gains and losses of this kind as being made in the bargaining process, rather than resulting from differences in the resources that parties contribute to the coalitions they join; but it is now evident that the resulting differences in the payoffs to different parties are almost entirely beyond the control of the parties themselves. They seem to arise from features of the party system rather than from the effects of ideology, personnel, or variations in bargaining power. Indeed, we would perhaps do well to regard those parties that gain as being the beneficiaries of fortune rather than of skill. There is room, within the variance

²³ In three-party coalitions in our universe, there happens to be a much greater polarization in party sizes than in two-party coalitions, and, as can be seen from Table II, the loss to large parties amounts to an astronomical 20 per cent: fully a fifth of the ministries to which they might otherwise have been entitled, or five ministries in our hypothetical example. The figures are arrived at by subtracting the displacement coefficient from the slope coefficient for large parties, and taking the displacement coefficient itself for small parties. This procedure yields hypothetical values for parties which hold 100 per cent and 0 per cent of seats, respectively, and provides fairly accurate estimates for the largest and smallest parties in our universe.

left unexplained by our findings, for some parties to affect their fate; but not many, and not by much.²⁴

The Distribution of "Bonus" Ministries

One last question remains to be confronted concerning payoff distributions in the quantitative dimensions. If, under certain circumstances larger parties surrender portfolios from their proportional shares to smaller partners as bo-

²⁴For those who insist that party ideology is an important, if not the crucial, variable associated with coalition behavior, these results will be discouraging. Clearly, ideology can have little effect upon the quantitative distribution of government ministries. However, since it may be of more than passing interest for some scholars to know the extent to which particular ideological types of parties fare in ministerial distributions, the following table summarizes the experience of those parties which were not more or less equitably treated. The table depicts those parties which formed a sufficiently large concentration within an advantaged or disadvantaged situation as to affect noticeably the position of their ideological type taken as a whole. The numerals before the party labels indicate the number of parties concerned, and the percentage these constitute within their ideological type. Numerals following the labels indicate the average index of overpayment (proportional excess of ministries received over seats contributed) to the ideological type taken as a whole (not just for the individual parties depicted).

Ideological Types Gaining and Losing in the Exchange of Ministries for Seats

Size of Party	Size of Coalition	
	Large	Small
Large	—	7 (30%) Conservative (-0.04)
		27 (41%) Religious (-0.04)
		6 (21%) Agrarian (-0.05)
Small	—	23 (34%) Radical/Lib (+0.05)

Socialist and Communist parties do not figure in the table. Their distribution into different size categories was so even as to preclude any significant gain or loss to their ideological type taken as a whole. The Conservative, Religious, and Agrarian parties all had a disproportionate number of large parties in small coalitions, and this, it has been demonstrated, accompanies heavy ministerial loss. It should be clear that the only ideological type to profit in the distribution is the Radical/Liberal. This type is the only one which contains a large concentration of parties in the strategic position that characterizes small parties in small coalitions (i.e., they benefit most from the relative weakness effect). Their gains are substantial. Radical/Liberal parties, on average, provide five per cent fewer seats than they receive ministries. This represents about one ministry in twenty as a bonus in return for nothing more than their presence as coalition partners. Moreover, it is important to remember that these figures are averages and that a number of parties do even better (or worse) than they might suggest.

nus payments, is there some typical set of such ministries, or does the payoff share of overpaid parties vary randomly as between ministerial types? That is, are there *specific* types of bonus ministry which accrue to overpaid parties such that bonus ministries may be differentiated from other portfolios included in their shares?

Parties receiving bonus ministries are those gaining in the exchange of ministries for seats: they receive proportionally more of the former than they contribute of the latter. By subtracting the proportional share of ministries received from the proportional share of seats contributed, for each party in each coalition, we may generate the same index of overpayment that we have used before. This index is positive when parties receive more ministries than they contribute seats and negative when they receive fewer ministries than they contribute seats.

In order to discover which ministerial types tend to be associated with parties overpaid and underpaid in the exchange of ministries for seats, we have correlated the proportional share of each ministerial type received by each party with the index of overpayment for that party.²⁵ The resulting coefficients will be positive when the ministerial type concerned tends to go to parties overpaid in the exchange of ministries for seats, and negative when that type tends to go to parties underpaid in the exchange.

When we look at the universe as a whole, the results of this analysis are not impressive. But our search for differences between individual ministerial types can be refined by looking only at those coalitions in which we know there to be a relationship between number of ministries of all kinds received and the index of overpayment. These are the small coalitions, which, as we have established, are the only ones in which bonus ministries are distributed on a systematic basis to small parties. In this subset of our universe the correlations are still not high.²⁶ Most

²⁵Since larger parties are more likely to receive ministries of all types than are small parties, it is necessary to standardize the data by dividing the number of ministries of each type received by the total number of ministries of all types received by that party. This calculation yields the proportion share of each ministerial type that each party receives.

²⁶Essentially this is because (as explained earlier) we are seeking to gain the maximum descriptive power from the analysis rather than to maximize the variance explained. Descriptive power is gained from simple bivariate relationships, while most variance is explained in complex multivariate relationships. We have chosen to take account of the fact that larger parties are more likely to receive a ministry of any type by dividing M (the number of any particular ministerial type received) by N (the number of all ministries of any type received). We would have done better, from

Table 4. Relationship Between Ministerial Types and the Index of Overpayment in Small Coalitions

Ministerial type*	Total distributed	Correlation (Pearson's r) with index of overpayment
<i>Group 1</i>		
Prime Ministership	8	-0.313
<i>Group 2</i>		
Finance	24	-0.086
Transport	54	-0.077
Agriculture	22	-0.068
Social Affairs	57	-0.066
Defense	8	-0.038
Justice	34	-0.031
Foreign Affairs	29	+0.012
Internal Affairs	46	+0.047
Specialized Portfolios	12	+0.079
Education	23	+0.082
<i>Group 3</i>		
International Trade	22	+0.134
Economic Affairs	92	+0.152

* Excludes Religious Affairs (N=15) and Inter-governmental Affairs (N=5)

ministries appear just about as likely to go to underpaid parties as to overpaid parties. In comparing different ministerial types with each other, however, quite small differences may be interesting.²⁷ The various coefficients are presented in Table 4.

The ministerial types listed are divided into three groups. The first group contains the one ministerial type which tends *not* to go to parties overpaid in the exchange of ministries for seats (parties receiving bonus ministries). The second group contains those ministerial types which go to such parties about as often as not.

the point of view of explaining variance, to take out N as a separate term in a multiple regression equation of the form

$$M = a + b_1N + b_2I + b_3NI + e,$$

where M and N are as described above, I is the index of overpayment, and NI is the interaction term for the joint effect of size and overpayment. Such an analysis in fact explains between 3 per cent and 43 per cent of the variance in the distribution of different ministerial types; with a low multiple correlation of 0.175 for specialized portfolios, and a high multiple correlation of 0.660 for prime ministerships. The relationship we are interested in, however, then becomes some function of b_2 and b_3 which is relatively hard to describe.

²⁷ It should be borne in mind that because we are dealing with a universe rather than with a sample, there is no question of whether our coefficients are statistically significant. Even the smallest r describes a real relationship.

The final group contains the only ministerial types which are at all likely to be found among the portfolios distributed to parties overpaid in the exchange. Within group two, those ministerial types accompanied by positive coefficients are somewhat more likely to be distributed to overpaid parties than those ministerial types accompanied by negative coefficients.²⁸

The major finding to be derived from Table 4 is that most ministerial types are about as likely as not to go to parties overpaid in the bargaining process. Only International Trade and Economic Affairs are noticeably more often distributed to overpaid parties than are other ministerial types. At the same time, one ministerial type—the Prime Ministership—is most unlikely to be found among the portfolios distributed to overpaid parties.

In order to gain some idea of the actual extent to which overpaid parties were more likely to receive those ministries in Group 3 than they were to receive ministries in Groups 1 and 2, the mean proportion of each ministerial type received by small parties in small coalitions was calculated. This proportion was then standardized according to the relative number of ministries of that type distributed in the whole universe (obviously, any party is more likely to find a ministry of Economic Affairs among its portfolios, when 92 of these ministries were distributed). A party's mean probability of receiving any one of the ministries in each of the three groups is presented in Table 5, in which the parties are divided between those which were overpaid in the distribution and those which were underpaid.

²⁸ It should be borne in mind that ministries have been assigned to types on the grounds of their area of concern, not their value in any ordinal sense. When several portfolios have the same area of concern, all were assigned to the same category, though only one of them would normally be honored with the title of the category concerned. This is particularly important in the case of the Foreign Affairs ministerial type, which often contains a number of portfolios other than that actually called the Ministry of Foreign Affairs. If these additional portfolios are excluded from the analysis, this ministerial type correlates negatively with the index of overpayment, though not strongly enough to take it out of group two.

Table 5. Mean Proportion Shares of Standardized Ministries Distributed to Second and Third Parties in Coalitions of Less Than Four

Ministerial types	Overpaid (N=75)	Underpaid (N=34)
Group 1	1.28%	0.96% (N=2)
Group 2	5.64%	7.08%
Group 3	8.82%	6.28%

From this table it can be seen that small overpaid parties in small coalitions were more than four times as likely to find one of the ministries in Group 2 among their portfolios than to find the Prime Ministership, and about seven times as likely to find one of the ministries in Group 3. By contrast, underpaid parties were no more likely to find a Group 3 ministry among their portfolios than one from Group 2.

But while small overpaid parties may be more likely to find one of the ministries in Group 3 among their portfolios than any other individual ministerial type, nevertheless, since this group is small, all parties will in aggregate receive most ministries from Group 2. This fact is reflected in the low correlations reported in Table 4.

Even had the correlations in that table been higher, however, we would not, strictly speaking, have been justified in drawing final conclusions about whether or not any particular ministerial type tended to be distributed as a specific bonus ministry. (If several ministries are distributed to one overpaid party, which of these ministries is responsible for the overpayment of that party?) If, however, any ministerial type always formed part of the set of ministries distributed to overpaid parties, it would be *quite likely* that this ministerial type was distributed as a specific bonus. Thus we would have been prepared to infer that any ministerial type correlating strongly with the index of overpayment was a ministerial type that could be specifically identified with the notion of bonus ministries. Failing any such high correlations, we cannot tell whether parties distinguish sharply between their proportional share of ministerial payoffs and the addition to that share which constitutes a specific bonus ministry. So we tentatively conclude that, while some ministerial types are more likely to be distributed to overpaid parties than others, all ministries going to such parties partake equally of the attributes of bonus ministries.

But we have found that some ministerial types partake of these attributes more often than others. This raises the question whether parties overpaid in the quantitative dimension (receive proportionately more ministries than they contribute seats) are compensatingly underpaid in the qualitative dimension. This would be the case if those ministries most likely to be distributed as bonus ministries were also those of least value to the recipients. Since we do not know how parties rank different ministries (or, indeed, whether different parties have similar preference orderings) we can do no more than speculate upon this point. It is note-

worthy, however, that if one splits the list of ministries presented in Table IV in half, dividing it in the middle of Group Two at the point between Foreign Affairs and Internal Affairs, those ministerial types below the line (correlating more than 0.04 with the index of overpayment) might generally be regarded as being less valuable to the recipients than those above the line (correlating less than 0.04 with the index of overpayment).

Thus the qualitative differences between the values of different ministries are likely to mitigate some of the inequities in the numerical payoff accruing to coalition patterns that we have found to exist in small coalitions. Confirmation of this hypothesis, however, must await further research.

Summary Propositions

In this paper we have been preoccupied with the exploration of one overriding problem: How can we account for payoff distributions in governing coalitions? The findings of our research verify a number of propositions describing the structure of coalition payoffs which may now be stated more explicitly.

P₁ The percent ministries received by parties in governing coalitions is, over all cases examined, directly proportional to the percent seats they contribute to the coalition.

This proposition is the general finding of the research and is the major hypothesis (or assumption) issuing from Gamson's theory of coalition formation. What it states is that, in general, we find the proportionality proposition operative in the ministerial distributions of governing coalitions. Our findings also disclose, however, that there are a number of limiting factors operating in governing coalitions which, in some cases, produce results other than direct proportionality.

P₂ The size of the party, and the size of the coalition it joins, will determine the extent to which the percent ministries it receives are directly proportional on a one-to-one basis with the percent seats it contributes.

Proposition Two lists the major variables which determine the exact nature of the numerical distribution of ministries in governing coalitions. Here, variation between the parties on these two variables will determine the extent to which they receive their directly proportional share of ministries.

P_{2a} Large parties in small coalitions are underpaid (receive less than their proportional share) in the distribution of government ministries.

P_{2b} Small parties in small coalitions are overpaid

(receive more than their proportional share) in the distribution of government ministries.

- P_{2c} Medium-sized parties in small coalitions (less than four partners) tend to receive a share of government ministries directly proportional to their seat shares.
- P_{2d} All parties in large coalitions tend to receive a share of government ministries directly proportional to their seat shares.

It should be noted that the effect of overpayment or underpayment is most pronounced at the extremities of variation in party size. This means that many of the deviations from direct proportionality which are found occurring *between* these extremes of variation are more likely to be computational—they do not necessarily result in the actual distribution of a bonus ministry.

A third proposition, relating to the nature of bargaining over the distribution of ministries in governing coalitions, may be inferred from our research.

- P₃ Parties in governing coalitions restrict their bargaining over the distribution of ministries to the question of which ministries each partner is to receive.

It is important to note that this proposition is not demonstrated by the data. Rather, it is a logical inference which follows from the premise that the basic nature of payoff distributions in governing coalitions is two-dimensional, and the fact that the number of ministries distributed to parties is proportional to their seat holding.

Since, in general, proportional distributions in the quantitative dimension indicate that exchanges across both dimensions do not occur frequently, it seems reasonable to conclude that bargaining over the number of portfolios each partner is to receive does not occur. Rather, it is more plausible to think of such distributions as institutional features, or "givens," of the bargaining context. In Schelling's terminology, proportional distributions in the quantitative dimension would constitute "focal point solutions"—outcomes mutually agreeable because they are obvious or natural.²⁰

But while we may reasonably expect that cross-dimensional bargaining is rare when the proportionality relationship operates, it was pointed out that in some cases parties are overpaid in the quantitative dimension and in some they are underpaid (Propositions 2_{a,b}). When such imbalances occur we may expect some kind of cross-dimensional bargaining to occur.

- P₄ Depending on whether or not parties in governing coalitions are quantitatively overpaid (underpaid), they will tend to receive a disproportionate share of particular ministries in the distribution.

This proposition, while generally consistent with our findings, is not itself sufficient to demonstrate the existence of cross-dimensional bargaining. What it does tell us is that a particular set of ministries goes disproportionately to those parties which are most likely to receive bonus ministries, and that at least one type of ministry tends to go to parties which surrender ministries. What it does not tell us is the preference ordering each party holds for the set of ministries distributed. If we knew this ordering, we would be able to make a judgment about the extent to which most preferred ministries accompanied the surrender of bonus ministries (indicating overpayment on the qualitative dimension for these parties) and the extent to which least desired ministries accompanied the receipt of bonus ministries (indicating underpayment on the qualitative dimension). While we have no objective method of ranking the ministries according to the degree of party preference, our ranking of the ministries supports intuitive judgments about the values of particular portfolios. In general, parties which receive bonus ministries tend also to receive a disproportionate quantity of "less valued" ministries, and those surrendering bonus ministries, a disproportionate quantity of "more valued" ministries. This would seem to indicate that, in effect, the lower value of the surrendered ministries serves to mitigate the degree of inequality (negative deviation from proportionality) suffered by coalition leaders when they surrender bonus ministries. Too much should not be made of this conclusion, however, since, strictly speaking, our data are insufficient to demonstrate it.

Unresolved Issues

While we may now know a great deal more about payoff distributions in governing coalitions than when we started, a number of interesting problems remain unresolved. Two of these strike us as being particularly important. First, we still know very little about ministerial distributions in the qualitative dimension. If the number of ministries distributed in governing coalitions is a given in the bargaining context, this means that most of the politically relevant activity which occurs in distributions of the payoff is relatively unexplored. Which ministries go to which parties? What variables are responsible for distributions in the qualitative dimension?

²⁰ Thomas C. Schelling, *The Strategy of Conflict* (New York: Oxford University Press, 1963), especially Chap. 3.

Our research has only been able to illuminate a small corner of this question. By discovering that a particular set of ministries is disproportionately received by parties overpaid in the numerical distribution, we may initiate an inquiry into the question of why this particular set was received rather than some other. Does the fact that Radical/Liberal parties make up a sizable proportion of the parties in this group influence the composition of the particular set of ministries it receives? If Communist parties constituted a large proportion of this overpaid group of parties, would the Interior Ministry have been included in this particular group of ministries? In short, is ideological status an important variable in determining the qualitative distribution of government ministries, or has it no effect?

The second problem which warrants investigation is the connection our research has with the development of coalition theory. We began our analysis with a proposition derived from Gamson's theory of coalition formation. Our findings have vindicated the accuracy of his judgment regarding the proportional nature of payoff distributions in governing coalitions. However, the fact that Gamson's major hypothesis has been at least partially verified empirically does not in itself justify our confidence in the ability of his theory to explain coalition formation. In fact, using the same universe of

coalitions to test the ability of Gamson's theory to predict coalition formation outcomes, one of the authors has shown that the theory is deficient in this respect.³⁰ If the theory is unable to predict formation outcomes, but, at the same time, the major assumption upon which its predictive ability rests has been empirically verified, how can these apparently contradictory findings be reconciled?

These are difficult questions for the study of political coalitions. The importance of finding answers for them, however, must not be underestimated. After all, payoffs, and the assumptions made about them, are the foundation upon which coalition theory is built, and until we are informed by more systematic research in this area, the explanations provided by theories of coalition formation will remain somewhat ambiguous and incomplete.

³⁰ See Eric C. Browne, "Testing Theories of Coalition Formation in the European Context," *Comparative Political Studies*, 3 (January, 1971), 391-410. Here, we are concerned with determining the extent to which the theories of Gamson, Riker, and Leiserson are able to predict coalition formation outcomes in our universe of thirteen European democracies. Gamson's theory (and Riker's) only had about two chances in a hundred of predicting correctly the formation of an actual winning from sets including all possible winning coalitions. Leiserson's theory, while somewhat better a predictor than the others, also performed poorly.

The Concept of Organizational Goal*

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Much has been written in recent years about the concept of organizational goal, some of it rather discouraging; the most frequently cited papers offer little hope that the concept will have any real utility for social scientists. This is a most unfortunate state of affairs, for the idea would appear on the surface to have great potential value in political science, as well as in other modern disciplines with an interest in the development of organization theory. A good conceptualization is needed, first of all, as a criterion against which organizational effectiveness and efficiency may be evaluated. Second, the organizational goal is important as a dependent variable. We are interested in whether organizations have goals or not, under what conditions they have goals, and under what conditions some kinds of organizational goals are more probable or more salient than others. In addition, the goal concept could be useful for classifying organizations into categories demanding different explanatory models of behavior.

No doubt, a large proportion of what organizations and their members do is best explained with models that do *not* include the notion of organizational goals, or even, perhaps, of any sort of goals as independent variables.¹ Organizational behavior is Hardy-esque; it depends upon the fortuitous, complex, and obscure confluence of many distantly related streams; it depends upon the solutions that happen to be there when the problems arise. There is utility in developing models for the explanation of behavior at this level, if only to drive home the point that so much of organizational behavior is not clearly goal-directed. On the other hand, some of the most important questions we have about organizational behavior are: (a) Is this organization turning out a good product or ser-

vice? Is it doing a good job? and (b) What kind of an institution is it? What values does it stress in order to remain a viable structure: its professional prestige? its good relations with the local community? its attractiveness to the managerial class? For these kinds of questions, the organizational goal concept and satellite concepts to be discussed below may indeed be useful components of empirically based explanatory models. But further, to neglect to come to terms with the concept may well be to neglect substantially these significant areas of research.

The two primary purposes of the present paper are dovetailed in succeeding sections. One purpose will be to review critically some important views of the organizational goal concept presented by social scientists. In so doing, the attempt will not be to catalog every work that mentions "goal" conceptually, but rather to select for more careful, critical analysis those views that contribute to perspective and that have set important definitional trends. The second purpose is to weave some of these strands together in order to produce a more concrete and comprehensive conceptualization than those presently available. Operationalization will also be discussed, as well as several implications of the kind of goal model offered. Specifically, organizational goals will be viewed as multiple rather than unitary, empirical rather than imputed, and as dichotomized into outwardly- and inwardly-oriented categories, here to be labeled "transitive" and "reflexive," respectively.

Some Seminal Conceptualizations

Cartwright and Zander draw a valuable distinction between the goal of an individual in a group, the goal of an individual *for* a group, and the goal *of* a group, or a group goal.² The first refers to individual attainment, while the latter two refer to group or organizational attainment. In addition, the first two are *held* individually, while the last is held collectively. We will be concerned here primarily with the last, the group goal, collective in both of these senses.

² Dorwin Cartwright and Alvin Zander, "Motivational Processes in Groups: Introduction," in *Group Dynamics*, 3rd ed., ed. Dorwin Cartwright and Alvin Zander (New York: Harper and Row, 1968), pp. 403-407.

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¹ See, for example, Michael D. Cohen, James G. March, and Johan P. Olsen, "A Garbage Can Model of Organizational Choice," *Administrative Science Quarterly* 17 (March, 1972), 1-25.

Without being very systematic about it, the writing of early organizational analysts such as Weber³ and Michels⁴ was permeated with the idea that organizations have identifiable goals, intuitively perceived to be concerned with the organization's official mission or function in society. The idea was strengthened when Parsons made goal orientation in this sense a defining characteristic of organization.⁵ Terms like "the organizational goal," "the goals of the organization," "achieving its goals," and so on, still appear commonly in this sense of primary function within the superordinate social system. This view has weaknesses that make it highly vulnerable. For one, it is nonspecific, nonoperational. For another, it tends to confuse imputed social function with empirical organizational purpose.⁶

An attack on the "goal model" of organizational analysis was launched by Etzioni in 1960⁷ and sustained subsequently by himself⁸ and by other organization theorists, especially Seashore and Yuchtman.⁹ Etzioni does not say what he means by the "goal model," but he appears to refer to the evaluation of organizations in terms of their primary mission or function. His chief criticisms are (a) that such analyses are not worthwhile because they always predictably find organizations to be ineffective, (b) that empirical analysis generally shows organizations to be pursuing objectives not contemplated by the goal model, and (c) that ascribing a particular societal function to an organization is simply a matter of the value bias of the analyst.

Etzioni's conclusion is that the concept of organizational goal should be abandoned, as an evaluation criterion, in favor of what he calls a

"system model."¹⁰ The criterion of effectiveness replacing goal achievement in this model is either (1) an "optimum" or "balanced" allocation of resources¹¹—the "system survival model"—or (2) "a pattern of interrelations among the elements of the system which would make it most effective in the service of a given goal"¹²—the "system effectiveness model." In the first instance, Etzioni offers us no criteria for determining an optimal balance of resources and even suggests that organizational analysts make their own independent determinations!¹³ In the second instance (the system effectiveness model), Etzioni returns explicitly to goal attainment as the criterion of effectiveness. There is a difference from former views, however, in that the central concept is no longer simply assumed to be "primary mission" or "social purpose." Etzioni defines an organizational goal as "a desired state of affairs which the organization attempts to realize," and as "that future state of affairs which the organization as a collectivity is trying to bring about."¹⁴ He recommends operationalizing this concept by asking participants what the organizational goals seem to be and by making certain behavioral observations, such as of budgets and the minutes of board meetings.¹⁵ It is the official mission concept that must apparently be abandoned, then, in favor of an empirical determination of the goals of organizations.¹⁶

In spite of having suggested this different and promising conceptualization, Etzioni does not advocate a focus on goals either for evaluating or for "studying" organizations.¹⁷ Instead, he proposes that we rely upon the system effectiveness model (which, however, contains the concept of organizational goal as its central element). Etzioni's position is not clear. Although he explicitly derogates the utility of the goal concept even after redefining it, let us conclude that he does not foreclose the possibility that it may profitably be used in organizational analysis.

To Etzioni's notion of the optimum allocation of resources, Yuchtman and Seashore have added the idea of ability to acquire resources. In lieu of a focus on goals, they propose defining an organization's effectiveness "in terms of its bargaining position, as reflected in the abil-

³ Max Weber, *The Theory of Social and Economic Organization*, trans. A. M. Henderson and ed. Talcott Parsons (New York: Oxford, 1947), pp. 337-341.

⁴ Robert Michels, *Political Parties*, trans. Eden and Cedar Paul (New York: Dover, 1959).

⁵ Talcott Parsons, "Suggestions for a Sociological Approach to the Theory of Organizations—I," *Administrative Science Quarterly* 1 (June, 1956), 64-67.

⁶ Robert K. Merton, *Social Theory and Social Structure*, 3rd ed. (New York: Free Press, 1968), pp. 114-115.

⁷ Amitai Etzioni, "Two Approaches to Organizational Analysis: A Critique and a Suggestion," *Administrative Science Quarterly* 5 (Sept., 1960), 257-278.

⁸ Amitai Etzioni, *Modern Organizations* (Englewood Cliffs, N.J.: Prentice-Hall, 1964).

⁹ Stanley Seashore and Ephraim Yuchtman, "Factorial Analysis of Organizational Performance," *Administrative Science Quarterly* 12 (Dec., 1967), 377-395; Ephraim Yuchtman and Stanley Seashore, "A System Resource Approach to Organizational Effectiveness," *American Sociological Review* 32 (Dec., 1967), 891-903.

¹⁰ Etzioni, "Two Approaches," p. 261.

¹¹ Etzioni, "Two Approaches," p. 262.

¹² Etzioni, *Modern Organizations*, p. 19.

¹³ Etzioni, "Two Approaches," p. 270.

¹⁴ Etzioni, *Modern Organizations*, p. 6.

¹⁵ Etzioni, *Modern Organizations*.

¹⁶ Etzioni, *Modern Organizations*, pp. 14-16.

¹⁷ Etzioni, *Modern Organizations*.

ity of the organization, in either absolute or relative terms, to exploit its environment in the acquisition of scarce and valued resources."¹⁸

Unfortunately, without the concept of organizational goal, resource acquisition fails to satisfy as a criterion of effectiveness. Yuchtman and Seashore themselves admit that "a crucial problem in this context is the determination of the relevant and critical resources to be used as a basis for absolute or comparative assessment of organizational effectiveness."¹⁹ In truth, the determination of which resources one should count either must be arbitrary or must appeal to the concept of organizational goal. It is possible to find in some manner that the goal of a specific organization is, purely and simply, to acquire certain resources. If that is not the case, then one must identify other goals in order to establish which resources are important for attaining them. Either way, the concept of goal is essential, although in the second case the goals themselves may often be assumed or implicit.

A third view calling the utility of the organizational goal concept into question comes from Simon and his colleagues.²⁰ The basis of the attack is, in essence, that no goal is *the* goal—that in order to understand why people do the things they do in organizations, one must take into consideration a whole host of goals. Instead of a goal, then, a set of constraints is offered; which of these, if any, might be considered principal or major depends upon one's vantage point from within the organizational coalition.

In the decision-making situations of real life, a course of action, to be acceptable, must satisfy a whole set of requirements, or constraints. Sometimes one of these requirements is singled out and referred to as the goal of the action. But the choice of one of the constraints, from many, is to a large extent arbitrary. For many purposes it is more meaningful to refer to the whole set of requirements as the (complex) goal of the action. This conclusion applies both to individual and organizational decision making.²¹

This conceptualization has been helpful in building detailed simulations of organizational decision making,²² but it is unsatisfying as a general definition for two reasons. First, it is

cumbersome. For example, to evaluate organizational effectiveness in terms of the organizational goal, or goal set, one must first determine the composition of the set, which will generally be impressively large, and then sum across the levels of attainment of all constraints in the set. This task would rarely if ever be undertaken by an analyst, thus rendering the idea of the effectiveness and efficiency of an organization almost completely useless. The same may be said for the goal concept as an element of behavioral hypotheses. For example, it would be most awkward to test the proposition that, "The less the subjective operationality of organizational goals, the greater the differentiation of individual goals in the organization."²³ The constraint set that constitutes "organizational goals" in this hypothesis would be unmanageably large for research, except in the simplest of organizations. Such research is therefore very unlikely to be carried out. Second, and more important, Simon's conceptualization merely shifts the need for definition from the term "goal" to the term "constraint." We have no rule for determining what is or is not a constraint that is part of the set of constraints that comprises the organizational goal. There is a modicum of further specification in terms of "the goals and constraints appropriate to the role,"²⁴ but this is only slightly less precise than the term "constraint set" itself.

Although the theoretical treatments reviewed thus far intentionally lead away, in many respects, from a clear conceptualization of the organizational goal, they also make several constructive contributions. One is that a useful conceptualization of this concept is likely to call for the empirical determination of goals, not for the declaration of an official mission or some imputation of goals by the analyst. Another is that organizations may and often do seek to achieve not just one but a great variety of goals. No unitary definition or conceptualization is likely to be useful.

Indeed, there are overwhelming numbers of intentions swarming in, over, and around organizations, all potentially eligible to be considered an organizational goal. The problem is to impose order upon them productively so that not everything is admissible, for in that case nothing is useful. This problem further divides into two subproblems, as follows:

Common usage, as indicated by all leading dictionary definitions, suggests that a goal is *an intent to achieve some outcome*. To determine

¹⁸ Yuchtman and Seashore, p. 898.

¹⁹ Yuchtman and Seashore, p. 901.

²⁰ Richard M. Cyert and James G. March, *A Behavioral Theory of the Firm* (Englewood Cliffs, N.J.: Prentice-Hall, 1963), pp. 26-43; Herbert A. Simon, "On the Concept of Organizational Goal," *Administrative Science Quarterly* 9 (June, 1964), 1-22.

²¹ Simon, p. 7.

²² Cyert and March.

²³ James G. March and Herbert A. Simon, *Organizations* (New York: Wiley, 1958), pp. 125-126.

²⁴ Simon, p. 13.

what is an organizational goal, then, one must separate out (1) what is *organizational*, i.e., how *intent* is to be considered at the collective level, and (2) what is a *goal* rather than, say, a subgoal, function, motive, or activity, i.e., what *kind of outcome* is to be admitted. Each of these subproblems will be considered with some care.

Intent

Let us recall some of the surprisingly few attempts to define the term "organizational goal" directly and fully. Etzioni: "An organizational goal is a desired state of affairs which the organization attempts to realize;"²⁵ Cartwright and Zander: "A *group goal* specifies a preferred state for the group as an entity and guides collective action toward achieving it;"²⁶ Warner: "By an organizational goal I mean a state of affairs or situation which does not exist at present but is intended to be brought into existence in the future by the activities of the organization."²⁷

Considering the question, "Whose intent?" it is quite clear upon close inspection of the wording of these definitions that they do not directly meet the personification, or reification problem,²⁸ that is, they do imply that the collective body "desires" or "intends" or "prefers" something but they are evasive when it comes to communicating what is meant by such notions when attached to a group, which is but an abstraction, rather than to some individual person with the physiological capacity to "intend."

Thompson provides a definition that purports to overcome the personification problem. He considers the goals of the organization, or organizational goals, to be the goals for the organization held by those in the dominant coalition.²⁹ Thus, the element of intent is transferred from the abstract group to specific individuals. Upon close inspection, Thompson's definition of organizational goals is troublesome in two respects. First, it discreetly attempts to substitute for the burden of defining the goals of an organization the equally difficult task of defining the goals of a group within it. Second, it imposes another difficult and heavily burden-

some chore—that of successfully defining and identifying the group that is dominant, that has power.³⁰ Moreover, Thompson is frank to say that he does not really define the collective purpose; rather, he attempts a substitution. Let us, instead, constitute the collective goal in part by naming and employing a *collective analog* of individual intent, thus meeting the personification problem directly and specifically.

Collectivities have two relevant kinds of measured characteristics—let us call them "global" and "analytical," after Lazarsfeld and Menzel.³¹ A global characteristic is one that inheres in the collectivity itself and is not some statistical combination of the traits of individuals. Organizational size and growth, for example, are global characteristics; we cannot measure them by taking the size or growth of organizational members and somehow adding them up. Organizational professionalization and morale, on the other hand, generally are analytical, or aggregated characteristics; we determine their levels by summing or averaging the professional attainment or the morale of the individual organizational members. Organizational prestige exemplifies a characteristic that might be of either type.

If we are to retain the idea of intent in the idea of goal, then an organizational goal must be an analytical characteristic, for only the individual human members can intend anything. Thus, we must aggregate, and it remains only to select a convincing mode of aggregation, such as average intensity, unanimity, or majority opinion. The choice is essentially arbitrary, but the best candidate would seem to be the concept of consensus, as in Thompson's "domain consensus."³² It is preferable because it connotes a certain unanimity, but it does not necessarily imply enthusiasm, spontaneous selection, top priority or exclusive preoccupation. Consensus means explicit or tacit agreement among those concerned that a certain behavior will under the circumstances be followed, notwithstanding the possibility that some might prefer other available alternatives.³³ A group

²⁵ See James G. March, "The Power of Power," in *Varieties of Political Theory*, ed. David Easton (Englewood Cliffs, N.J.: Prentice-Hall, 1966), p. 70.

²⁶ Paul F. Lazarsfeld and Herbert Menzel, "On the Relation Between Individual and Collective Properties," in *Complex Organizations*, ed. Amitai Etzioni (New York: Holt, Rinehart and Winston, 1961), pp. 422-440.

²⁷ Thompson, pp. 28-29.

²⁸ Cyert and March, *A Behavioral Theory of the Firm* (p. 28), reject the idea of consensus, but only because, undoubtedly, there is rarely if ever a common preference ordering among organizational members. Surely, this is asking too much. Even an individ-

²⁵ Etzioni, *Modern Organizations*, p. 6.

²⁶ Dorwin Cartwright and Alvin Zander, "Individual Motives and Group Goals: Introduction," in *Group Dynamics*, 2nd ed., ed. Dorwin Cartwright and Alvin Zander (New York: Row, Peterson, 1960), p. 349.

²⁷ W. Keith Warner, "Problems in Measuring the Goal Attainment of Voluntary Organizations," *Journal of Adult Education* 19 (Fall, 1967), 5.

²⁸ Cf. James D. Thompson, *Organizations in Action* (New York: McGraw-Hill, 1967), pp. 127-128; Simon, p. 1.

²⁹ Thompson, p. 128.

goal, then, connotes collective intent, e.g., a *consensus of intent*.

Perrow's concept of "operative goals" is closely allied, though not identical: "Operative goals designate the ends sought through the actual operating policies of the organization."³⁴ Both Thompson and Perrow are suggesting a force that would appear to be as important in research as the goal concept itself. What they emphasize is not a definition of organizational goals, but an empirical assessment of the objectives, of whatever kind, that determine main streams of organizational behavior. The goals for the organization held by certain individuals or sets of individuals can be extremely influential; chief executives, to take just one example, commonly are able to impose their wills upon much of the behavior of their organizations. In the sequel, we will refer both to true organizational goals, which connote collective intent, and to operative goals.

Outcome

Having considered intent, let us turn to the second essential element in the definition of goal, the idea of an outcome. At this point, several terms will be introduced as they are discussed in a most helpful paper by Deniston, Rosenstock, and Getting.³⁵ Their concern was with the evaluation of public health programs. A *program* they define essentially as an objective plus the collection of resources and activities organized to attain it. An *objective* in their terminology is some state or condition of people or other components of the environment of the program that is deemed desirable for the program to attain. An important distinction is also made in their analytical framework between objectives and *activities*—an objective is a condition; an activity represents some effort of program personnel to bring that condition about. For example, the program objective can be "clean restaurants," but not "inspecting restaurants." A final significant distinction is drawn between a program objective and a sub-objective. The latter is an instrumental condition; it is a prerequisite that must be attained in

order that the program objective may be attained.

Thinking of a program, for the moment, as the collectivity of concern, the program goal would be the collective intent of program members to achieve some outcome. We may follow Deniston et al.,³⁶ Warner,³⁷ and others in specifying that this outcome must be a state or condition, but of what? Deniston and his colleagues concentrate almost exclusively on conditions external to the program, but they do also mention the possibility of "survival programs."³⁸ It is well that they do, for a very substantial proportion of organizational effort is often unmistakably oriented in this direction. On the other hand, Gross³⁹ and Cartwright and Zander⁴⁰ specify a state or location of the group itself, but this criterion alone is not adequate either. Considering that one of a health agency's important goal types, for example, may be framed in terms of better health status within the community, it would be stretching matters considerably to consider this objective somehow to refer to a state of the organization.

Perrow has suggested classifying goals into two groups according to whether their referent outcomes are internal or external to the organization.⁴¹ At about the same time, Gross also identified the dual goal concept and included it in research. He has suggested that organizations may be considered as having *output* goals and *support* goals.⁴² Output goals are "reflected in" some product or service that will affect society outside the organization.⁴³ Support goals have to do with attaining the conditions necessary for system survival—that is, with maintaining the organization itself.⁴⁴ In a survey of American universities, Gross specified 47 separate goals, divided them into these two categories, and further allocated them to subcategories. The output goals are misleadingly stated in terms of organizational activities or product characteristics, but they tend to suggest an im-

³⁶ Deniston.

³⁷ Warner, p. 5.

³⁸ Deniston, p. 330.

³⁹ Edward Gross, "The Definition of Organizational Goals," *British Journal of Sociology* 20 (Sept., 1969), 293.

⁴⁰ Cartwright and Zander, "Motivational Processes," *Group Dynamics*, 2nd ed., p. 401.

⁴¹ Charles Perrow, "Organizational Goals," in *International Encyclopedia of the Social Sciences*, ed. David Sills (New York: Macmillan and Free Press, 1968), Vol. 11, pp. 306-309. These ideas have been substantially elaborated by Perrow in his recent book, *Organizational Analysis: A Sociological Review* (Belmont, Calif.: Wadsworth, 1970), pp. 133-174.

⁴² Gross, pp. 282-286.

⁴³ Gross, p. 287.

⁴⁴ Gross, pp. 282-283.

ual may well not have a clear preference ordering among his values, yet each value in the list is undeniably a value nonetheless. In the same sense, there may be a consensus on several goals within a group, but little interpersonal consensus on their *ranking*.

³⁴ Charles Perrow, "The Analysis of Goals in Complex Organizations," *American Sociological Review*, 26 (Dec., 1961), 855.

³⁵ O. L. Deniston, I. M. Rosenstock, and V. A. Getting, "Evaluation of Program Effectiveness," *Public Health Reports* 83 (April, 1968), 325-326.

part of the organization upon something outside of itself. The support goals are also stated primarily in terms of activities, but with few exceptions each denotes the objective of keeping the organizational coalition together by providing the members of a subgroup some inducement to remain and contribute.⁴⁵ Thus, it appears that we may with great profit consider goals to be of two basic types, those referring to a state of the program or organization itself and those referring to a state of its environment.⁴⁶ Accordingly, we will consider that a program and an organization may have two distinct types of goals: a "transitive," externally oriented, or functional goal, and a "reflexive," internally oriented, or institutional goal.⁴⁷ Moreover, it is important to note that this dichotomy applies not only to true collective goals, but to operative goals and other "goals for" the organization, as well.

We may now specifically define a program goal as the collective intent of program members to bring about some specific state of the program itself or of its environment.

One begins by identifying any product or

service that is collectively produced, such as restaurant inspections, refrigerators, or antitrust prosecutions. The program is the collectivity, including human effort and other resources, that is oriented toward producing it. The program goal is its intended impact. If the intended impact is a condition outside of the program (e.g., the good health of people drinking inspected milk), then the goal is transitive in character; if it is the program itself (e.g., the prestige of the political science department or the profitability of the firm), then it is reflexive.

Optionally, one may define *organization* as being precisely synonymous with program. Let us accept that definition here. For terminological convenience, however, we will want to consider an organization as "complex," that is, as a program containing several other programs. In this sense, then, what is an *organizational goal*?

An *organizational goal* is the goal of a program occurring within the organization and under its auspices whose direct referent is either the organization itself as an institution or some aspect of the organization's environment. For example:

(a) That the line units in a manufacturing firm find their equipment dependable may be a goal (transitive) of the equipment maintenance program, but it is not an organizational goal for the firm, since its referent is neither the whole organization nor its environment.

(b) The survival goal of the heart disease control program of the New York City Health Department may be a goal (reflexive) of the program, but not of the department, for its referent is, again, suborganizational.

(c) The goal specifying improvement in the level and severity of heart disease in New York City is an organizational goal (transitive) of the health department, since its referent is indeed the environment of the health department; so, in precisely the same sense, is the product quality goal of the manufacturing firm, insofar as this is directed toward the utility of the product for the consumer.

Elaboration of the Goal Concept

Before turning to operationalization and implications, let us elaborate briefly on several key aspects of the concept of organizational goal.

Transitive Goals. A transitive goal is one whose referent is outside of or in the environment of the organization in question. Several aspects of the concept of organizational goal in general are important to bear in mind when considering what would and what would not

⁴⁵ Edward Gross, "Universities as Organizations: A Research Approach," *American Sociological Review*, 33 (Aug., 1968), 518-544. Gross suggests that these two types of goals are co-equal ("Organizational Goals," pp. 291-292), but his discussion in general, especially as it includes the label "support goals," implies that these latter are subordinate and instrumental to the output goals. What is lacking is explicit recognition of what may be deduced by careful scrutiny of the goals he investigated, viz., that the "support goals" are in fact oriented toward benefiting the members of the organization itself.

⁴⁶ See Robert F. Bales and Philip E. Slater, "Role Differentiation in Small Decision-making Groups," in *Family, Socialization and Interaction Process*, ed. Talcott Parsons and Robert F. Bales (Glencoe: Free Press, 1955), pp. 259-306; Chester I. Barnard, *The Functions of the Executive* (Cambridge: Harvard University Press, 1938), pp. 55-56; Dorwin Cartwright and Alvin Zander, "Leadership and Performance of Group Functions: Introduction," in Cartwright and Zander, *Group Dynamics*, 3rd ed., pp. 306-307; Karl W. Deutsch, *The Nerves of Government* (New York: Free Press, 1963), pp. 92-93; James K. Dent, "Organizational Correlates of the Goals of Business Management," *Journal of Personnel Psychology* 12 (Autumn, 1959), 365-393; Amitai Etzioni, *A Comparative Analysis of Complex Organizations* (New York: Free Press, 1961), pp. 89-126; Frank Friedlander and Hal Pickle, "Components of Effectiveness in Small Organizations," *Administrative Science Quarterly* 13 (Sept., 1968), p. 292; and Daniel Katz and Robert L. Kahn, *The Social Psychology of Organizations* (New York: Wiley, 1966), p. 66.

⁴⁷ The usage of the terms "transitive" and "reflexive" here is somewhat closer to their application in grammar than in mathematics. "Transitive" has the sense of a verb taking a direct object—the organization has an impact on something else. "Reflexive" refers to what the organization does for itself.

qualify as a transitive organizational goal. Particularly, the referent is a condition, not an activity of organizational personnel, and it must be at the organizational level, so that subgoals are excluded. *A transitive goal is thus an intended impact of the organization upon its environment.*

An organization providing goods or services (a "transitive" organization) has as a final activity the exportation of a product or service to its environment: inspecting restaurants, selling refrigerators, lending library books, disseminating antiwhiskey propaganda. These interface transactions do not quite extend into the area that may be considered "impact." Rather, it is the *program hypothesis* that this transaction will have a certain result—that restaurants will be clean, food will be unspoiled, people will be informed and entertained, alcoholism will be controlled. These dependent variables are the transitive goals, and testing this hypothesis is what program evaluation is all about.

It is useful to refer at this point to a concept from Deniston et al.,⁴⁸ that we may call the "transitive objective line." Clearly, the program goal may always be rationalized in terms of some further objective.⁴⁹ For example, a possible organizational goal of a highway department such as "more traffic to and from this specific area" may be seen as a subobjective of goals further to the right along the objective line, such as "increased commercial activity in the area." One may well want to select only the most immediate impact of a program as its transitive goal. At times, however, justification of a program's existence or budgetary support may require that one also consider a further objective—one that might make the validity of the resulting program hypothesis much more dubious. For example, wiretapping may clearly increase the rate of convictions in narcotics cases (a "proximate" goal), but does it reduce traffic in narcotics (an "ultimate" goal)? Restaurant inspections may result in cleaner restaurants (proximate goal), but do they result in a reduction in the incidence of food-borne diseases (ultimate goal)? Awareness of the concept of objective line may inform both policy debate and organizational planning.

Reflexive Goals. Turning to reflexive considerations, one readily concludes that the idea of internally oriented goals, institutional goals, maintenance goals, system goals, or support

goals, as they have been variously called, is ultimately best expressed in terms of inducements-contributions theory.⁵⁰ Thus, we may identify as the general reflexive goal of an organization that *inducements will be sufficient to evoke adequate contributions from all members of the organizational coalition.* The term "adequate" in this context can only mean adequate for survival.

This broad statement in no way precludes or questions the utility of various subclassification schemes, such as those used by Perrow⁵¹ or Gross.⁵² For example, one might strive for a productive classification of reflexive subobjectives into categories such as organizational prestige, growth, and profitability (or "fair share" of appropriations).⁵³ Furthermore, within the general framework of the reflexive goal, each organization works out its own rules of the game. The criteria used in partitioning organizational resources and the resultant distribution of inducements—money, power, status, psychological experience, etc.—will certainly differ from organization to organization. It is largely these differences that make each organization an *institution* and provide it with an ethos. The manner and nature of this distribution of inducements, then, constitute an alternative characterization of the program whose objective is the organization's survival goal.

Coequality. The transitive and reflexive goals are best considered as being coequal—neither is conceptually major⁵⁴ and each may be viewed both as a limitation upon and a subgoal of the attainment of the other. In trying to accomplish some transitive purpose, it is plain that reflexive concessions must almost invariably be made; survival is necessary for effectiveness. On the other hand, where there is consumption by others of organizational output, some concessions toward transitivity will also generally have to be made, at least to the extent of giving the *appearance* of being functional, of producing a product or service of acceptable quality. Legitimacy is necessary for survival.

Existence and Importance of Goals. The above should in no way be construed to imply that all organizations offering a product or service have transitive and reflexive goals. Whether an organization has a goal and how important it is are

⁴⁸ See March and Simon, *Organizations*, pp. 83–88.

⁴⁹ Perrow, *Organizational Analysis*, pp. 133–174.

⁵⁰ Gross, "Organizational Goals," p. 287.

⁵¹ Aaron Wildavsky, *The Politics of the Budgetary Process* (Boston: Little, Brown, 1964), pp. 16–18.

⁵² Cf. Gross, pp. 291–292.

⁴⁸ Deniston, "Evaluation of Program Effectiveness," pp. 325–330.

⁴⁹ Abraham Kaplan, *The Conduct of Inquiry* (San Francisco: Chandler, 1964), pp. 394–396.

empirical matters. For example, if a manufacturing firm has little or no transitive purpose in the sense of putting out a good and serviceable product—a “public service” goal in Dent’s terminology⁵⁵—then that is the simple fact. We must accept this organization as having a self-seeking or primarily reflexive orientation. We should expect participants and close observers to see this fact reflected in the dominance of reflexive over transitive considerations in the making of certain decisions. Nor is it claimed that members of all organizations have a strong interest in organizational survival. For any number of reasons they may not, and to the extent that they do not, the organization is simply weak in reflexive intent, or institutionally weak.

Federated Organizations. Let us examine more closely the nature of transitive and reflexive organizational goals when the organization is composed of many component programs. The reflexive goal of a component such as the Frigidaire Division or the Heart Disease Control Division forms no logical part of the reflexive goal of the parent organization. In fact, its own strength may serve to divert and weaken reflexive goal strength in the organization as a whole. An organization will almost always have a survival goal, i.e., some measurable consensus of intent to survive as an entity, but this may be either weak or strong. The reflexive goal strength of a “federally” organized or multi-product organization, for example, is likely to be less impressive than that of a “unitary” organization because of the priority of loyalties to its component bureaus.⁵⁶ The case with transitive goals is different in form but similar in import. It is different in that the transitive goals of certain component programs (e.g., the major subunits of the Department of Health, Education, and Welfare) are indeed organizational goals. If one wishes to speak of the transitive organizational goal, it can be nothing but the aggregate of these components. As in the former case, however, transitive goal strength in a large, federally organized company or agency may be small—consensus on the attainment of its whole collection of heterogeneous goals may be low and is likely to be located exclusively in the organization’s central leadership and bureaucracy. Some individuals may barely have heard of the programs to which they do not belong.

Both transitively and reflexively, then, many

entities that are organizations in the legal sense are only barely organizations in the sociological sense; they are lacking in general commitment to outcomes that are truly organizational.

Product Goals. In the terms introduced above, a small number of the goals of universities listed by Gross would not be in the same category as the remainder. They would be seen as subobjectives or perhaps activities, but not as organizational goals. For example, to “Keep up to date and responsive,” or “To make sure the university is run by those selected according to their ability to attain the goals of the university in the most efficient manner possible.”⁵⁷ This is not to say that these preoccupations are unimportant, just that they are on a different level. They are instrumental to the achievement of other organizational purposes.

Product goals must be seen in the same light; “production of automobiles” is no more an organizational goal than “inspection of restaurants.” Some may find this difficult to accept, but it is eminently reasonable. An organizational goal must be a *guide to action*,⁵⁸ but a product, pure and simple, is an empty affair. It does not guide action. It will never cause changes in itself. Consider, for example, an organization that manufactures and markets lunch bags. Why does it not put a long metal handle on each lunch bag it packages? Surely, the answer is not to be found by examining the bags themselves. If members of the organization were asked this question they would no doubt respond either (1) that people would then not buy the lunch bags and the business would fail, or (2) that lunch bags with metal handles would be less serviceable than those without, or both. Lunch bags, it appears, are being made for *reasons*, and the reasons dictate whether or not they might have long metal handles. It is these reasons and not the product itself that indicate either organizational goals or operative “goals for.” In truth, the organization that concentrates exclusively on its product is probably guilty of displacement of goals—substituting means for the ends of impact on the public or system survival.⁵⁹

⁵⁷ Gross, “Organizational Goals,” p. 290.

⁵⁸ Cartwright and Zander, “Motivational Processes,” in *Group Dynamics*, 3rd ed., pp. 409–411; Etzioni, *Modern Organizations*, p. 5; March and Simon, pp. 155–156; and Mayer N. Zald, “The Comparative Analysis and Measurement of Organizational Goals: The Case of Correctional Institutions for Delinquents,” *Sociological Quarterly*, 4 (Summer, 1963), 206–230.

⁵⁹ Rensis Likert, *New Patterns of Management* (New York: McGraw-Hill, 1961), pp. 71–76; Merton, pp. 249–260; and W. Keith Warner and A. Eugene Havens, “Goal Displacement and the Intangibility of

⁵⁵ See Dent.

⁵⁶ See Herbert A. Simon, Donald W. Smithburg, and Victor A. Thompson, *Public Administration* (New York: Knopf, 1950), pp. 269–272.

Operationalization, Identification

It is not desirable to attempt to specify in detail how concepts such as the foregoing shall be operationalized. Optimally, this will be done in different ways by different scholars for different organizational circumstances. There are, however, a few available guidelines and distinctions concerning operationalization and goal identification that may be helpful.

We will often be interested in salient operative goals and will not want to confine research attention to goals that qualify conceptually as organizational goals. One may also want to collect information on various subgoals, "goals for," individual goals, and inducement priorities. In using such information in organizational analysis, however, it should be more productive of insight to categorize it systematically into the classes suggested, rather than simply to lump all of these objectives indiscriminately under the rubric, "organizational goals."

There are several ways to get information about goals in organizations. One way is to hypothesize the existence of a specific goal and to test this hypothesis by documenting organizational behavior predicted from it, subsequently revising the goal hypothesis if necessary. This might be done, for example, in what Cohen and Cyert call "descriptive" simulations.⁶⁰ Several commentators have suggested that goal information may be obtained by a combination of observing certain organizational behavior and asking people directly about goals.⁶¹ Although there is some evidence that observation helps ascertain organizational goals,⁶² there is also reason to believe that such observation is unreliable. Warriner notes from his research experience that activities have many, many consequences and that it is most difficult to demonstrate which were intended.⁶³ Furthermore, if comparative research across many organizations is contemplated, direct observation will usually be prohibitively expensive. At present, direct observation of organizational behavior may be of greatest value in hypothesis generating or exploratory studies, where it need not be

entirely systematic, rather than in hypothesis-testing research. For the latter, and for most multiorganization studies, the social scientist will probably have to depend upon the judgment and report of participants.

The next relevant question, then, is which participants shall be selected to provide information. The most valid strategy at present is probably to use "informants" rather than "respondents."⁶⁴ That is, one does not ask for the intent or priorities of all program or organizational members and simply aggregate the lot. Rather, one asks for the perceptions of selected knowledgeable individuals with a variety of vantage points and goes along, so to speak, with their collective measurement. In essence, one does not elicit the informant's own objectives and priorities, but rather his view of the organization's characteristics. In pursuing this measurement strategy, program and organizational members will no doubt be considered to make excellent informants because of their acquaintance with relevant behaviors. However, one need not confine oneself to members of the organization alone, and one certainly need not use all of them.

To determine the relative importance of goals, one might well ask knowledgeable individuals, as did Gross,⁶⁵ a question such as "How important is each of the listed aims in this organization?" with responses scaled from very important to not important at all. To determine existence, two strategies are suggested. One is to ask knowledgeable individuals whether a consensus exists among the specific group identified with the program, that is, whether there is some basic explicit or tacit agreement that this specific goal is one of the ends to be attained as a result of the collective effort of these individuals, within their organizational roles: (The existence of operative goals is then determined by elimination: "goals for" and other objectives that do have substantial importance in determining behavioral directions, but that are not true collective goals, are operative goals of the organization.) Alternatively, the nonexistence of goals may at times be indicated within tolerable limits of error by zero or near zero importance.

There are basically two ways in which one may compose a list of goals for questions about relative importance. The more expensive way is to conduct the research in two stages—a first in which possible goals are discovered through a

Organizational Goals," *Administrative Science Quarterly*, 12 (March, 1968), 545-549.

⁶⁰ Kalman J. Cohen and Richard M. Cyert, "Simulation of Organizational Behavior," in *Handbook of Organizations*, ed. James G. March (Chicago: Rand McNally, 1965), pp. 308-314.

⁶¹ Etzioni, *Modern Organizations*, p. 6; Warner, "Problems in Measuring," p. 8; and Charles K. Warriner, "The Problem of Organizational Purpose," *Sociological Quarterly* 6 (Spring, 1965), 139-146.

⁶² For example, see Perrow, "The Analysis of Goals," p. 856.

⁶³ Warriner, p. 142.

⁶⁴ See Morris Zelditch, Jr., "Some Methodological Problems of Field Studies," *American Journal of Sociology* 67 (March, 1962), 569-572.

⁶⁵ Gross, "Universities as Organizations," p. 523.

survey of the organization itself, followed by the primary stage in which priorities within the resultant list of goals are judged by the same or different informants. The other method of developing the list is to use armchair techniques and library research on such organizations in general. Since the first method is organization-specific one can perhaps have more confidence in it, but in neither case can one be sure of having identified a proper list of major aims and orientations out of the infinity of possibilities. No method can give such assurance.

Implications

In this concluding section, let us briefly note some research directions and some normative implications of the foregoing discussion.

Evaluation Research. Evaluation of organizational effort has different implications depending upon whether the concern is with transitive or reflexive effectiveness. In the former case, program evaluation becomes essentially a challenging application of experimental design, in which the operation of the program is the independent variable and attainment of the transitive goal is the dependent variable.⁶⁶ When certain subobjectives have been spelled out, the evaluation may also serve to inform program personnel where they may have failed to achieve a necessary precondition, or perhaps where they have been overexpending resources.⁶⁷ If an organization has several programs, the transitive effectiveness of the organization is best expressed as a profile: For example, health department A is highly effective in its communicable disease surveillance, moderately effective in both heart disease and cancer control, but ineffective in the abatement of air pollution; or housing authority B is highly effective in its urban renewal program but ineffective in its efforts to improve race relations.⁶⁸

A further consideration tends to enter into evaluation of transitive effectiveness in the case of a multiprogram organization, and that is the efficacy of its priority system for achieving an overall or "umbrella-type" goal, such as "better health for the community."⁶⁹ In most cases, such a goal is merely imputed by the observer-

appraiser; no true organizational objective or operative goal exists empirically whose function is to maximize some ultimate transitive condition by means of judicious priority setting among component programs. On the other hand, there may upon occasion actually be a transitive program of this nature within an organization. The members of this program are not at all likely to belong to specific transitive components but rather to be confined to central leadership personnel. The goal of "better health for the community," for example, might indeed induce a health department's leadership to put more resources into air pollution control than into well-baby clinics, and so on. Such umbrella-type organizational goals, however, are generally quite difficult if not impossible to operationalize satisfactorily, and this difficulty should make the organizational analyst look twice at any claim for their existence or their utility.

Reflexive effectiveness presents different problems entirely. We are able to see without research that the organization has survived; the question is, How well? It is here, it seems to me, that the method introduced by Seashore and Yuchtman would prove most valuable,⁷⁰ for we may answer this question by measuring the organization's success in exploiting its environment in the acquisition of resources. Note that the question, "Which resources?" is now answered by identifying both the requirements for pursuing transitive goals and the kinds of inducements sought by members of the organizational coalition. In addition, the satisfaction of members with the distribution of inducements provides a measure of the extent to which the allocation of resources is optimal, which is at least part of the system effectiveness model advocated by Etzioni.

It is important to recognize whether organizational activity is to be appraised in terms of organizational goals or, alternatively, of goals for the organization held by particular individuals or groups. The former would seem in general to make more sense, but the latter is by far the more prevalent selection. Even if "goals for" are to be the criteria, however, there is still reason to have information on actual organizational goals. For example, the "official mission" of a federally funded antipoverty project can be considered the federal government's goal for that organization. The project must generally, by grant regulation, be evaluated in those

⁶⁶ Donald T. Campbell, "Reforms as Experiments," *American Psychologist* 24 (April, 1969), 409-429.

⁶⁷ Deniston, "Evaluation of Program Effectiveness."

⁶⁸ Cf. Edward C. Banfield, "Ends and Means in Planning," in Sidney Mailick and Edward Van Ness, eds., *Concepts and Issues in Administrative Behavior* (Englewood Cliffs, N.J.: Prentice-Hall, 1962), pp. 75-79.

⁶⁹ Cf. the term "appropriateness" as used by Deniston, pp. 323-324.

⁷⁰ Seashore and Yuchtman, "Factorial Analysis of Organizational Performance"; Yuchtman and Seashore, "System Resource Approach to Organizational Effectiveness."

terms. However, the relative importance of this objective as an empirically determined organizational goal not only might be problematic, but, if known, might also shed light on why the "goal for" was not effectively attained or, perhaps, why this was an inappropriate goal to impose in the first place. Similarly, we frequently evaluate organizational activity in terms of "goals for" held by the chief executive. When we do so we must recognize that there are two questions involved—not only that of the soundness of his plan of means to achieve the ends, but also his effectiveness in getting the rest of the organization to follow the plan. If at an early stage his "goal for" is not also an organizational goal of substantial importance, his task is more difficult than it could be. If the same is true at a later stage, it may certainly help to explain his failure.

Other Behavioral Research. Gross has already pointed the way to a research strategy in which goals are identified, compared in importance, and examined in connection with other variables for the existence of theoretically relevant relationships.⁷¹ It is of great interest, it seems to me, that among the top seven goals of universities reported in his research, as marked off in standard deviation units from the mean, the first two are apparently reflexive goals (academic freedom for the faculty and university prestige), while the next three are indefinite, and only the sixth and seventh are transitive in character (the scholarly training of students and the conduct of pure research). On the other hand, in the respondents' ranking of the goals according to the importance they *should* have, transitive concerns receive much greater emphasis and university prestige slips from second place to eleventh.

Gross treated the goals as dependent variables and explored the predictability of their importance from type of university ownership, university prestige, and the relative power of various groups within and outside of the formal organization. Many other variables may have a theoretically significant relationship with the importance of goal types, e.g., the size of the organization, its growth rate (as in Dent's research), its security within its environment, its category within a federal-unitary classification, its age or stage of development (as in Perrow's research on hospitals), and the personal values of hierarchical leaders. The goals may also be considered as causes rather than effects of a great many salient organizational characteristics. A hypothesis of considerable interest, for

example, would be that the greater the relative importance of the transitive goal, the more the organization will be characterized by rational rather than political decision-making processes. Dalton's excellent book⁷² provides supporting evidence at least in one cell of the table—the "Low-Low" cell. In the organizations he considered it appears that almost all decision-making was political and at the same time that actors were rarely if ever motivated by transitive concerns—the quality or serviceability of the product.

The distinction between transitive and reflexive goals *within* an organization suggests a similar distinction *among* organizations, that is, we may say that whole organizations are primarily either transitive or reflexive.⁷³ Zander, for example, has recently documented some structural differences in the characteristics of transitive and reflexive voluntary associations.⁷⁴ Essentially, a transitive organization is one that is oriented toward the exportation of a product or service to its environment; a reflexive organization is one that exists primarily for the mutual benefit of its members. The latter category includes not only fraternities, recreational clubs and criminal gangs but also other kinds of groups that have been extremely important in the development of both organizational and political theory—professional societies, trade associations, marketing associations, political interest groups, labor unions and even Marxian social classes. The most important reflexive organization of all is the society itself: In general, a society has no output, no transitive goal. Many highly significant works—by Michels,⁷⁵ Lipset et al.,⁷⁶ Olson⁷⁷—that seem to be about organizations in general actually concern only reflexive organizations. Their contents are not necessarily wrong when applied to transitive organizations, but essentially irrelevant.

It may be objected that the transitive side of the dual goal concept is of little value because it hardly exists in reality, especially in private, profit-making organizations. The objection is

⁷² Melville Dalton, *Men Who Manage* (New York: Wiley, 1959).

⁷³ Cf. Tom Burns, "The Comparative Study of Organizations," in *Methods of Organizational Research*, ed. Victor Vroom (Pittsburgh: University of Pittsburgh Press, 1967), pp. 132–133.

⁷⁴ Alvin Zander, "The Purposes of National Associations," Research Center for Group Dynamics, The University of Michigan (Ann Arbor: 1970).

⁷⁵ Michels.

⁷⁶ Seymour M. Lipset, Martin Trow, and James S. Coleman, *Union Democracy* (Glencoe, Ill.: Free Press, 1956).

⁷⁷ Mancur Olson, *The Logic of Collective Action* (Cambridge: Harvard University Press, 1965).

⁷¹ Gross, "Universities as Organizations."

not valid. First, it is apparently not true, at least as evidenced by responses to direct questions. Gross did find a substantial, though perhaps secondary, transitive orientation in universities.⁷⁸ Moreover, 42 per cent of the top executives interviewed by Dent did mention a transitive goal as one of the major concerns of their (private) organizations.⁷⁹ Second, the importance of transitive goals is a variable, not a supposition, and as such it is apparently correlated with a number of other important organizational structures and behaviors—power distribution, conflict, decision-making style, size, growth rate, type of ownership, etc. In short, the *degree* of outward orientation is a theoretical element of consequence. Third, the transitive goal category is significant normatively, that is, it is well to retain it simply *because* it may be underemphasized in modern organizations.

This latter, normative research implication should not pass unnoticed. Few will deny that they would prefer transitive organizations actually to supply a societal good, but recent treatments of the organizational goal tend to distract us from this orientation. With the goal concept as rendered in terms of systems, system resources, and constraint sets, for example, we have begun to view transitive organizations as existing for themselves alone, and the notion that they are supposed to be doing something is all but forgotten. Seashore and Yuchtman are explicit: "... conceptual schemes that link organizational effectiveness solely to the values of some element in the environment must be rejected; instead the standards of effectiveness are to be sought with reference to the organization itself."⁸⁰ Simon is less explicit, but his orientation leads in the same direction. The opposing

view, however, is taken here. It cannot be detrimental to view ordinary private, public, and voluntary organizations as doers, as well as existers, and to question how well they are doing. Do they have transitive goals? Are they meeting them?

The extent to which the transitive goals of individuals and organizations are unimportant has significant social implications. Although it would be difficult to demonstrate, it appears that members of a society derive a sense of fulfillment in the occupation of transitive roles that may well be a *sine qua non* of successful long-term social living. This orientation comes easily during the nation-building period and in crises of threatened destruction from without; it is perhaps difficult to sustain in a relatively safe and affluent culture. Although there is a definite transitive side to the things we commonly do, there unfortunately appears to be a tendency to emphasize the reflexive, instead. Let me refer first to my own professional milieu. The academician could mention the student he inspired, but prefers instead to talk about the pressures upon his time, the recognition he received, the relative progress of his colleagues, and other reflexive concerns. What is true in general of us seems to be equally true of builders, manufacturers, electricians, physicians, and so on. We do not provide much manifest social support for transitive roles—in fact, the individual who takes pride in his own transitive function is embarrassing. As a practice this must certainly inhibit us in grappling with the problems we still face as a society and as an aspect of socialization it cannot help but contribute to a sense of futility and resentment among many young people in the culture as they scan the menu of possible adult roles. Whereas to be exclusively transitive is hardly possible, to be exclusively reflexive may be suicidal; a society that does not honor the transitive roles of its members may not be viable.

⁷⁸ Gross, "Universities as Organizations," p. 529.

⁷⁹ Dent, pp. 365, 366, 369, 388, 391.

⁸⁰ Seashore and Yuchtman, p. 395.

Attitudes of the Arab Elite Toward Palestine and Israel*

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The behavioral revolution in the study of politics and the concomitant emphasis on political socialization have had their greatest impact in the economically advanced societies and Western democratic countries in particular.¹ For one thing, it is more feasible to do such research, especially survey analysis, in these countries. But it is also true that in the so-called developing areas, a large percentage of the population is politically inactive or unimportant in influencing policy decisions. In such countries, therefore, the political attitudes and behavior of the elite deserve and have received special attention.² Particularly in the Arab world, however, such accounts have tended to be impressionistic or descriptive or both. Concerning the Arab-Israeli conflict, for instance, hardly any studies have been carried out to delineate explicitly the attitudes of the Arab public or any sector of it. Furthermore, the determinants of those attitudes are generally not investigated at all. In this paper, an attempt is made to help fill this gap.

* This is the revised version of a paper presented at the Middle East Studies Association Meeting, November 6-7, 1970, Columbus, Ohio. It is part of a larger study of the political attitudes of the Arab elite. Much of the research was conducted during my tenure as a Ford Foundation Faculty Research Fellow. I also thank Kansas State University for providing computer time and other assistance. Michael C. Hudson, Malcolm H. Kerr and William L. Richter read an early draft and made many valuable suggestions.

¹ For bibliographic references in the area of political attitudes and socialization, see John P. Robinson, J. G. Rusk and K. B. Head, *Measures of Political Attitudes* (Ann Arbor, Mich.: Survey Research Center, 1968); Richard E. Dawson, "Political Socialization," in James A. Robinson, ed., *Political Science Annual*, 1966 (Indianapolis, Ind.: Bobbs-Merrill Co., 1966), pp. 1-84; Kenneth P. Langton, *Political Socialization* (New York: Oxford Univ. Press, 1969); and Norman Adler and Charles Harrington, *The Learning of Political Behavior* (Glenview, Ill.: Scott, Foresman & Co., 1970).

² Since parties and elections are usually corrupt or nonexistent, the "political dynamics" in many developing countries are described in terms of elite competition for the top offices in the state. Three good studies of elites in the Middle East are: Frederick W. Frey, *The Turkish Political Elite* (Cambridge, Mass.: M.I.T. Press, 1965); Marvin Zonis, *The Political Elite of Iran* (Princeton, N.J.: Princeton Univ. Press, 1971); and R. Hrair Dekmejian, *Egypt under Nasser: A Study in Political Dynamics* (Albany, N.Y.: State University of New York Press, 1971).

While it is necessary to present a cursory statement of the main attitudes of the Arab elite concerning Palestine and Israel, the major thrust of this study is to pinpoint the factors which have been most significant in shaping those attitudes. It is a pioneering investigation of the forces of political socialization which have a bearing on the most difficult issue facing the Arab and Middle East region today. Although we are dealing with a Middle Eastern problem, the determinants of political attitudes discussed here, such as age, religion, etc., have been shown to be of importance in many other countries, a fact that will facilitate comparative analysis.³

The "Arab elite" under consideration were those resident in the Asian part of the Arab world plus Egypt as well as those of their colleagues who came to the U.S. for work or study. Among them were journalists, lawyers, engineers, doctors, and members of other professions, as well as university students. Those with a modern education do in fact constitute an elite in the sense that they must have been favored to have received that education at all; and by virtue of that education, they occupy or will occupy high positions in society and government. For the purpose of this paper, however, the terms "intelligentsia," "intellectuals," and "the educated classes" will be used interchangeably and in substitution for the word "elite."

Methodology

An adequate study of the Arab elite must take account of three broad sources of data. The influential writings by the intelligentsia, especially since the 1967 war, constitute a primary and essential source. Interviewing is another useful means of gathering data on Arab intellectuals, but since several important ideological movements are banned in the Arab world, and since political dissenters are often jailed, arrested, exiled or executed, it has not been feasible to interview a representative or a

³ See Donald R. Matthews, *The Social Background of Political Decision-Makers* (Garden City, N.Y.: Doubleday, 1954); Gabriel A. Almond and Sidney Verba, *The Civic Culture* (Princeton, N.J.: Princeton University Press, 1963); and the various election studies in Western democracies.

properly random sample of the elite in the region. Nevertheless, more than 60 nonrepresentative, nonrandom depth interviews with Arab intelligentsia from Egypt, Jordan (including Arab Palestine), Iraq, Kuwait, Lebanon, and Syria were conducted in the summers of 1968, 1970, and 1971. The sessions ranged from one to three hours and proved useful in yielding insights not easily obtainable through formal structured interviews.

A more structured sample survey, however, was possible with regard to another group, namely the Arab students and professionals in the United States. In April, 1968, a questionnaire was mailed to 1,800 Arab or ex-Arab nationals resident in the United States. Of these, 363, or slightly more than 20 per cent of the sample, filled out and returned the questionnaire. Different sources were tapped for the names and addresses of the persons to be interviewed, though the mailing list of the Organization of Arab Students in the United States (O.A.S.) constituted the basic source. The questionnaire was mailed to alternate individuals on the list, after having excluded those names which did not sound like Arab names.

The survey sample was composed of individuals from nine Arab countries, namely Egypt (76), Saudi Arabia (63), Iraq (57), Palestine (51), Syria (41), Kuwait (30), Lebanon (23), Jordan (9), Libya (5), and others (8), i.e., those from sheikdoms or those who gave no answers. In round figures, 95 per cent of the respondents were males, almost two-thirds (65 per cent) were under 30 years of age, and 43 per cent were married. Moslems outnumbered Christians by a ratio of 3:1. Overwhelmingly, the respondents were born or raised in the city. Not unlike the intelligentsia in the Arab world and developing countries generally, the respondents primarily came from middle- and upper-class families.

The question arises, How representative are the survey respondents of Arab elite generally? It should be pointed out that Arab students and professionals in the United States are not cut off from the political ferment in the Arab world. Some of them, in fact, actively participate in the debate by contributing articles to various magazines and journals. The Nasser-Kassem conflict, for instance, was clearly reflected among the Arab students in North America. More recently, the split between the advocates and opponents of the cease-fire resolution and the so-called Rogers plan for ending Arab-Israeli hostilities spilled over to the U.S., where two "official" meetings of the O.A.S. were held. It should also be mentioned that those on

the O.A.S. mailing list are, by and large, more interested, informed, and active in matters dealing with Arab affairs than those who are not on the list. The same argument applies in particular to those who participated in the survey. It is reasonable, therefore, to conclude that the views of the survey respondents are not unrepresentative of the political ideas of Arab intelligentsia in general. In fact, they are probably a truer reflection of Arab opinion on certain issues than are the views of the interviewees in the Middle East, since the answers were given "without the fear of ending up in jail," as one respondent put it.

Nevertheless, the data from the survey are obviously not sufficient for testing or developing hypotheses about Arab intelligentsia in general. The purpose is to utilize these data in conjunction with the personal interviews and the political literature to help us better understand the political attitudes and behavior of Arab elite. Until the situation changes and research can be conducted freely, this appears to be the best approach.

Results

Four questions in the survey dealt directly or indirectly with the Palestine-Israel dispute. Respondents were asked, for instance, whether or not Arabs should make a distinction between Jews and Zionists in considering who their enemies are. Some 75 per cent unequivocally stated that there definitely should be a distinction, and an additional 14 per cent argued that it would depend on the circumstances involved. In other words, less than 10 per cent of the Arab intelligentsia sampled indiscriminately referred to "the Jews" as the enemy. Also, *all* interviewees (and here it is not important to name any of them but simply to record their attitudes) made the distinction. Furthermore, Arab political writers are generally eager to differentiate between Jews and Zionists. It is among the religiously-oriented of both Christians and Moslems that a blanket denunciation of "the Jews" may occasionally be found.⁴ It is interesting to note, however, that the propensity

⁴ The usual technique employed is to "describe" the character of the Jew by using quotes and references from the Old Testament, the Koran, the Talmud and the "Protocols of the Learned Elders of Zion," an anti-Jewish tract. Among books of this nature, almost all of which appeared since the 1967 war, are: Abduh al-Rajih, *al-Shakhsiyya al-Isra'iliyya* [The Israeli Personality] (Alexandria: M.K. Press, 1968); Ibrahim Khalil Ahmed (an ex-Anglican minister converted to Islam), *Isra'il wa-al-Talmud* [Israel and the Talmud] (Cairo: al-Wa'i al-Arabi Press, Oct., 1967); and Kamal Yusuf el-Hajj, *Hawl Falsafat al-Sahniyya* [On Zionist Philosophy] (Beirut: n.p., late 1967).

Table 1. Association Between Elementary Schooling and Propensity to Differentiate Between Jews and Zionists

Jew and Zionist Differentiated?	Elementary Education	
	Govt. or Moslem	Private Christian
Yes or depends	91.6%	81.2%
No	8.4	18.8
	100% N=214	100% N=64

$\chi^2 = 5.5$; $p < .02$

to differentiate between Jews and Zionists is not so much affected by religious affiliation of the Arab elite as it is by the type of school they attended. This is not unusual, since other studies have demonstrated the importance of the school, especially on the elementary level, as a socializing agent.⁵ It is intriguing, however, to discover that, among the Arab elite, those educated in a private Christian elementary school are more likely *not* to make the distinction between Jews and Zionists than are those who attended a government or Moslem elementary school. Nonetheless what should be emphasized is that a very small percentage of Arab intelligentsia was willing to lump all Jews together as enemies.

The firm belief expressed by the overwhelming majority of Arab intelligentsia in this sample is that Zionism and Israel constitute the most important problem and the gravest threat facing the Arab world today.⁶ In fact, the threat is so great that any and all others appear

⁵ The importance of the school as a socializing agent has been studied extensively. See, for instance, Robert D. Hess and Judith V. Torney, *The Development of Political Attitudes in Children* (Chicago: Aldine, 1967); and James S. Coleman, ed., *Education and Political Development* (Princeton, N.J.: Princeton University Press, 1965). See also the selections on the school in Roberta S. Sigel, *Learning about Politics* (New York: Random House, 1970). It is worth noting, however, that Kenneth Prewitt and Joseph Okello-Oculi argue that manifest political socialization in schools is not sufficient to "help resolve the critical needs of nation-building" in Uganda and other new nations. See their "Political Socialization and Political Education in the New Nations," in Sigel, p. 609.

⁶ Two related questions were asked, one of them open-ended, the other not. They were (1) List in the order of their importance, beginning with number one for the most important, the major issues facing the Arab world today; and (2) List in the order of their importance, beginning with number one for the most important, the major threats facing the Arab world today: Communism and communist infiltration —; Reactionary and traditional forces —; Zionism and the state of Israel —; Fascism —; Military dictators and adventurers —; Imperialism —; Other (please specify) —.

to be of minor significance. Also, serious concern was expressed about future Israeli encroachments. This is quite apart from, and in addition to, the great sense of loss experienced when Palestine was first partitioned. Especially since the 1967 Israeli conquests, the feeling is strong and widespread among the Arab elite that Zionist-Israeli aims go beyond "little Israel" to eventually encompass "greater" or "Eretz" Israel. This fear is also articulated in personal interviews and in the writings on the subject. Even moderates who advocate a Lebanese nationalism, for instance, have expressed great concern in the past four years lest Israel expand to incorporate the southern part of their country.⁷

These fears were clearly reflected in the responses to the survey question: "What do you believe to be Israel's goals in the Arab world?" About 75 per cent of the respondents were thoroughly convinced that Israel harbors grandiose expansionist designs—perhaps running as far as from the Nile to the Euphrates. What is more significant is that these views are equally shared by the young and the old, the Egyptians as well as the Saudis, the rich and the not so rich, the married and unmarried, the highly educated and the university students. A significant difference, however, was noted between the views of the Christian and Moslem Arab elite. Thus, Table 2 shows that more Christian than Moslem intelligentsia were likely to give Israel the benefit of the doubt. Nonetheless, it is striking

⁷ See, in particular, Basim el-Jisr and David Sayegh, *Lubnan wa-at-Tahaddi al-Isra'ili* [Lebanon and the Israeli Challenge] (Beirut: Dar al-Funun Press, 1968); and *Lubnan wa-al-'Amal al-Fida'i al-Filistini* [Lebanon and the Palestinian Commando Movement] (Beirut: Dar al-Ilm lil-Malayin, 1969)—the latter being a collection of essays by some members of the Development Studies Club.

Table 2. Association Between Perceived Israeli Goals and Religious Affiliation

Perceived Israeli Goals	Religious Affiliation	
	Moslem	Christian
Expand and take over the area from Nile to Euphrates or as much as possible and/or be an agent for imperialism	91.3%	79.5%
Seek a negotiated settlement with only minor border adjustments	8.7	20.5
	100% N=254	100% N=73

$\chi^2 = 8.0$; $p < .004$

ing that almost eighty per cent of the Christian respondents believed the Israelis either have expansionist designs or tend to act as imperialist agents in the area or both.

Once more these results are not unrepresentative of opinion in the Arab world proper, although the more moderate views are generally unpublished except by members of fairly well-established groups such as the Maronites in Lebanon.⁸

Although there is general agreement on the existence of a formidable enemy and a singular threat to the Arab world, much debate focuses on the disagreements about how to deal with Zionism and Israel. There seem to be five distinct and different approaches to the problem of confronting the Israeli challenge, namely, Arab unity, communism, a return to Islamic faith, development, and the Palestinian revolution as expressed in the resistance movement.⁹

While these five positions represent the main approaches now popular among Arab intelligentsia, it would be a serious error to think that the advocates of one particular stand are so committed to that view that they are not prepared to entertain others if the situation changes, or even under the same circumstances at a later date. In fact, it is not unfair to state that an educated Arab is likely to be simultaneously anti-imperialist and pro-development, -unity and -armed Palestinian struggle. This, indeed, is the main tragedy of the situation, namely that the Zionist-Israeli issue spotlights all the weaknesses of Arab society while at the same time preventing the Arabs from tackling their other problems because of their crippling

preoccupation with Israel. Israel cannot be ignored, nor can it as yet be defeated, nor can an influential Arab majority psychologically come to terms with it. The survey responses to the question, "What is the best way for the Arabs to deal with Israel?" probably reflect Arab elite opinion generally. Almost two-thirds were willing to continue the struggle to eventual victory, while another 8 per cent were not yet ready for a settlement, favoring continued economic boycott and nonrecognition indefinitely. Still, some 25 per cent expressed a desire for an end to the conflict in some form.

Our next task, then, is to delineate as much as possible the "characteristics" of the Arab elite which incline them to an attitude of compromise or to a more militant stance on the Palestine-Israel question. Four variables were employed to measure respondents' background, namely, the father's social status, his occupation, his annual income, and his education. In the survey study, there was no statistically significant correlation between attitudes toward Israel and the social status, educational level, or yearly income of the respondents' fathers. This finding was not unexpected since personal observation and interviews in the Arab world had indicated that extreme hostility toward Israel was not confined to certain socioeconomic classes rather than others. A similar situation may be observed in the United States with regard to the Jewish vote in national elections.¹⁰ In both cases, the emotional and ideological factors are more important than are status and income.

There was also no measurable difference concerning policy suggestions *vis-à-vis* Israel between those who were raised in the city and those brought up in the countryside. Here it is important to remember that we are confining our remarks to educated Arabs. For them, the city and the city's political milieu have become the accepted norms, and the village and its parochial attitudes have been abandoned as "backward" and "uncivilized."¹¹

⁸ In particular, see the publications of Al-Kataeb (Phalanges Libanaises) political party.

⁹ A good collection of Arab nationalist writings in English may be found in Kemal H. Karpat, ed., *Political and Social Thought in the Contemporary Middle East* (New York: Frederick A. Praeger, 1968). For the opinions of Arab communists and Marxists, see Fuad Qazan, *al-Thawra al-Arabiyya wa-Isra'il* [The Arab Revolution and Israel] (Beirut: Dar al-Talia, 1968). The Moslem fundamentalist viewpoint may be gleaned from Salaheddine al-Munajjid, *A'mida' al-Nakba* [The Pillars of the Disaster] (Beirut: Dar al-Kitab al-Jadid, 1968); and Ramadan Lawand, *al-Insan al-Arabi baad al-Karithethain*, 1948-1967 [The Arab Personality after the Two Catastrophes, 1948-1967] (Kuwait: Maktabat al-Amel, n.d., late 1967?). The main spokesman for development as a solution is Constantine Zuraiq, *Ma'na al-Nakba* [The Meaning of the Disaster] (Beirut: Dar al-Ilm lil-Malayin, 1948); and *Ma'na al-Nakba Mujaddadan* [The Meaning of the Disaster Renewed] (Beirut: Dar al-Ilm lil-Malayin, 1967). A good brief account of the resistance movement in English is found in Michael C. Hudson, "The Palestine Arab Resistance Movement: Its Significance in the Middle East Crisis," *Middle East Journal*, 23 (Summer, 1969), 291-307.

¹⁰ Since the 1930s, American Jews have strongly supported the Democratic Party. See Lawrence H. Fuchs, *The Political Behavior of American Jews* (Glencoe, Ill.: The Free Press, 1956). See also Angus Campbell, Philip E. Converse, Warren E. Miller, and Donald E. Stokes, *The American Voter* (New York: John Wiley & Sons, Inc., 1960), pp. 301-302.

¹¹ In a 1960 study, it was found that Republicans and Democrats in the U.S. manifested less disagreement on foreign policy than on any other issue. The questions at the time, however, concerned attitudes toward the U.N., military alliances, and foreign aid. A more viable comparison with the Middle East situation would be the attitudes of Americans toward the Second World War. See Herbert McClosky, Paul J.

Table 3. Association Between Attitude Toward Israel and Age, Level of Education, and Marital Status

Policy Suggestion	Age Level		Educational Level			Marital Status	
	Up to 30 yrs.	Over 30 yrs.	Student-B.A.	M.A.	Ph.D.-M.D.	Married	Unmarried
Prepare for its defeat	71.3%	59.5%	74.0%	65.3%	50.7%	60.8%	72.3%
Continue economic boycott	8.5	8.3	8.1	8.4	9.9	7.4	9.2
Compromise and recognize it de facto or de jure	20.2	32.2	17.9	26.3	39.4	31.8	18.5
	100% N=223	100% N=121	100% N=173	100% N=95	100% N=71	100% N=148	100% N=195
	$\chi^2=6.3$; $p<.04$		$\chi^2=13.9$; $p<.01$			$\chi^2=8.1$; $p<.01$	

Differences in political opinion between the young and their elders have been noted in various election studies in the West, with the older citizens generally being more conservative in their views.¹² This is also true of the Arab elite, as is shown in Table 3, with regard to the survey respondents. The more "radical" stance of the young Arab intelligentsia is not merely the product of a lower degree of sophistication or the absence of responsibility. It can best be explained by the general atmosphere in which these young members of the intelligentsia were raised, namely, extensive and intensive indoctrination against the acceptance of an Israeli state in the area. None of them was more than ten years old when Israel came into being. Thus all spent their formative years in a period of active hostility between Israel and the Arab world.

While age is itself important in the espousal of certain beliefs, other attributes related to age are also significant. The relevance of education, for instance, to political efficacy and to political orientation has been shown in various country and crosscultural studies.¹³

The survey results generally substantiate these impressions. Furthermore, those with graduate degrees, in addition to being older and more educated, are probably beginning to worry about a career and a family. Also, those who had their undergraduate training in the Arab world are likely to have received an education that is not up to Western standards, with a heavy dosage of political propaganda. A

graduate education, however, is a clear indication of a serious commitment to a profession.¹⁴

Married people everywhere tend to be more conservative socially and politically than those who are unmarried. This is also true among the Arab elite, as shown in Table 3 concerning the survey respondents. On the whole, married members of the Arab elite both at home and abroad display a kind of political quietism, and an absence of enthusiasm for any grandiose projects in the area. On the question of Israel, while more of them are willing to contemplate a compromise solution, the majority agree with their unmarried colleagues about the necessity of continued struggle.

Among the Arab elite in the U.S., one observes a monotonic and positive relationship between length of residence in the host country and the willingness to negotiate an end to the dispute over Palestine-Israel. Furthermore, as Table 4 shows, there is a definite increase in the desire to conciliate around the sixth year of residence and another one after that.

Both attitude changes are in part the result of living away from the scene of the conflict and from the generally anti-Israeli atmosphere. Furthermore, these educated Arabs find themselves in a country in which the reporting of Middle East news is generally pro-Israeli and anti-"radical" Arab regimes.¹⁵ Consequently,

¹⁴ For a critical evaluation of the state of Arab colleges and universities as well as the educational process generally, see Antoine Zahlan, "Traces of the Defeat and Our National Universities" (Arabic), *Arab Studies*, 11 (Sept., 1968), 61-76; "Science and Backward Countries," *Scientific World*, 6 (1967), 5-12; and *The Brain Drain: Lebanon and Middle Eastern Countries* (Beirut: mimeographed, Nov., 1969).

¹⁵ For an analysis and discussion of pro-Israel and anti-Arab bias in the Western press during and after the 1967 war, see American Institute for Political Communication (AIPC), "Domestic Communications Aspects of the Middle East Crisis," A Special Report (Washington, D.C., July, 1967); and Michael W. Suleiman, "American Mass Media and the June Conflict,"

Hoffmann, and Rosemary O'Hara, "Issue Conflict and Consensus Among Party Leaders and Followers," *American Political Science Review*, 54 (June, 1960), 406-427.

¹² For election studies in the U.S., see Bernard R. Berelson, Paul F. Lazarsfeld and William N. McPhee, *Voting* (Chicago: University of Chicago Press, 1954); and Campbell et al.

¹³ Almond and Verba, *Civic Culture*; and Coleman, *Education and Political Development*.

they become sympathetic to the "moderates" and favor their willingness to compromise.

The second major increase in compromise might also be the result of a somewhat deliberate effort to spare oneself the agony of continuous conflict in the old country and its ramifications in the new one—since after six years' residence, it is certain that the decision has been made to stay permanently in America. This explanation is further supported when we compare Arabs already employed with those who are still studying. It is quite clear that when the idealism and lack of responsibility of youth are replaced by definite commitments and responsibilities, a more "realistic" and compromising attitude emerges.

Students of the Middle East generally and of the Arab world in particular have long been struck by the importance of religion in the area's political affairs. The pervasive influence of Islam not only as a religion sacred to the overwhelming majority of the Arabs but also as a cultural force has already been noted. Under Moslem rule, the "national" unity or identity group was the religious sect, with the Moslem collectivity constituting the *umma* (nation) and the various non-Moslem (primarily Christian and Jewish sects) allowed to function as somewhat autonomous subunits under the *millet* system. With the collapse of the Ottoman (Moslem) empire, a new basis for identification and solidarity became available, namely local, regional, ethnic, or Arab nationalism articulated in Western terminology even when it was not the same product. But the transformation could not take place overnight, and apart from a very tiny minority of Western-educated Arabs, the bulk of the population continued to think and act

on the basis of sectarian solidarity. Even among the educated, however, it was not easy to shed the traditions and habits of centuries, especially since Western values were more easily accepted by the Christian Arabs than by their Moslem counterparts.

Gradually, however, the idea of a secular state, the division of the Arab world into numerous political units and the increasing popularity of nationalism as well as other Western ideologies—all these developments weakened the religious bond. The question of how much weakening has taken place thus far is not entirely clear, however. Thus, a recent study showed that educated Arabs are beginning to experience some alienation from religion.¹⁶ Also, while in 1935 religious solidarity was still very strong even among the Arab elite, by the early 1950s the social distance among *national*, i.e., country, groups became greater than that among religious sects. When the study was replicated in the early 1960s, both religious and national social distances were found to be still slightly higher. On the other hand, a study using a different, and in my opinion better, method of measuring group affiliation found that the religious bond is far stronger than mere citizenship in a particular Arab country.¹⁷

¹⁶ Habib Hamam, "A Measure of Alienation in a University Setting," M.A. thesis (Beirut: American University of Beirut, 1969), cited in Halim Barakat, "Alienation and Revolution in Arab Life," (Arabic), *Mawaqif* (Beirut), 1 (July-Aug., 1969), 14-44.

¹⁷ E. Terry Prothro and Levon Melikian, "Social Distance and Social Change in the Near East," *Sociology and Social Research*, 37 (Sept.-Oct., 1952), 3-11; Levon Melikian, "Social Distance" (Arabic), *al-Abhath* (Beirut), 18 (March, 1965), 64-77; and Levon H. Melikian and Lutfy N. Diab, "Group Affiliations of University Students in the Arab Middle East," *Journal of Social Psychology*, 49 (May, 1959), 145-159. In the latter study, Christian Arab students generally ranked the ethnic bond second and religion third, whereas the Moslem respondents generally reversed those rankings.

in *The Arab-Israeli Confrontation of June 1967: An Arab Perspective*, ed. Ibrahim Abu-Lughod (Evanston, Ill.: Northwestern University Press, 1970), pp. 138-154.

Table 4. Association Between Attitude Toward Israel and Length of Residence in North America, and Employment

Policy Suggestion	Length of Residence in N. America			Employment Status	
	2 yrs.	6 yrs.	More than 6 yrs.	Student	Professional
Prepare for its defeat	84.3%	68.7%	55.1%	73.6%	54.7%
Continue economic boycott	6.3	8.6	8.7	7.4	10.3
Compromise and recognize it <i>de facto</i> or <i>de jure</i>	9.4	22.7	36.2	19.0	35.0
	100%	100%	100%	100%	100%
	N=32	N=233	N=80	N=216	N=117
	$\chi^2=11.2: p<.02$			$\chi^2=12.7: p<.002$	

Table 5. Association Between Attitude Toward Israel and Religious Affiliation

Policy Suggestion	Religious Affiliation	
	Moslem	Christian
Prepare for its defeat	71.3%	47.4%
Continue economic boycott	8.5	9.2
Compromise and recognize it <i>de facto</i> or <i>de jure</i>	20.2	43.4
	100% N=258	100% N=76

$\chi^2 = 17.6; p < .001$

The above studies concentrated on the individual's sense of solidarity with a particular social unit. What interests us here, however, is to determine whether or not mere membership in a religious group or a political entity predisposes the individual to internalize certain attitudes or to behave in a particular manner. One study, for instance, found that though the political culture of the area is generally authoritarian, members of the Moslem Arab elite scored higher on the authoritarian scale than did their Christian counterparts.¹⁸ But in another study, no significant difference was found between Christian and Moslem Jordanian teachers concerning their political alienation or achievement orientation.¹⁹ This is not surprising or unexpected. We now need to consider the various

¹⁸ E. Terry Prothro and Levon Melikian, "The California Public Opinion Scale in an Authoritarian Culture," *Public Opinion Quarterly*, 17 (Fall, 1953), 353-363.

¹⁹ Robert B. Cunningham, "Political Alienation Among Jordanian Teachers," paper delivered at the Middle East Studies Association (MESA) Meeting in Chicago, Ill., Dec. 9, 1967 and "Patterns of Achievement Orientation Among Jordanian Teachers," paper delivered at the MESA Meeting in Austin, Texas, Nov. 15, 1968.

political and social attitudes of Christian and Moslem Arab elites of different professional backgrounds. The two groups will also be compared cross- "nationally," i.e., on a country-by-country basis.

When asked how the Arabs should deal with Israel, as Table 5 shows, more Christians than Moslems among the educated Arabs are willing to accept the state of Israel as an unpleasant but nevertheless real fact in the Middle East.

It might be recalled from Table 2 that some 80 per cent of the Christian survey respondents believed Israel to be bent on further expansion. That more than 20 per cent of those who hold such belief are nevertheless willing to compromise might well mean that, in their view, this is the best way to contain Israeli expansion. It is also true, of course, that it is psychologically easier for a Christian Arab than it is for a Moslem Arab to "accept" the Israeli reality. This is because the dominant culture-religion in the area since the seventh century has been Islam.

It should be pointed out, however, that the difference in the feeling and thinking of Moslem and Christian Arabs on the question of Israel is a matter of degree and not of kind. Neither is religion the only or indeed the most important factor. When we analyze the attitudes of Christian and Moslem Arabs on the basis of their country of birth, as in Table 6, we find that a Palestinian or an Iraqi Christian is likely to be more militant on the Israeli question than a Lebanese—be he Christian or Moslem. Also Egyptians of both religious faiths appear to be among the most willing to compromise.²⁰

²⁰ Although the N's are very small in Table 6, our primary interest is in the trend indicated. Personal interviews and on-the-spot observation strongly support these findings. It should be noted that the political literature is not likely to be much of a guide here because (1) censorship laws and political repression in several Arab countries often discourage, if not com-

Table 6. Attitude Toward Israel, by Country and Religion

Country of Birth	% Moslem			% Christian			N	
	Defeat	Boycott	Compromise	Defeat	Boycott	Compromise	Moslem	Christian
Palestine	81%	4%	15%	55%	20%	25%	26	20
Iraq	76	11	13	83	0	17	45	6
Saudi Arabia	76	6	18	—	—	—	63	—
Kuwait	70	19	11	—	—	—	27	—
Syria	70	15	15	41	6	53	20	17
Egypt	63	2	35	27	13	60	54	15
Lebanon	44	22	33	33	0	67	9	12
	N=174	N=21	N=49	N=31	N=7	N=32	244	70

Table 7. Association Between Attitude Toward Israel and Country of Birth

Policy Suggestion	Country of Birth						
	Iraq	Saudi Arabia	Kuwait	Palestine	Syria	Egypt	Lebanon
Prepare for its defeat	78.6%	76.2%	72.5%	71.4%	60.0%	55.1%	38.1%
Continue economic boycott	8.9	6.3	17.2	10.2	10.0	4.3	9.5
Compromise and recognize it <i>de facto</i> or <i>de jure</i>	12.5	17.5	10.3	18.4	30.0	40.6	52.4
	100%	100%	100%	100%	100%	100%	100%
	N=56	N=63	N=29	N=49	N=40	N=69	N=21

$$\chi^2 = 33.1; p < .001$$

Up to now, it has been implicitly assumed that an Arab nation does exist, an assumption that is often made both in the West and in the Arabic-speaking countries. Recent studies have also indicated that affiliation with and attachment to Arabism is generally stronger than local or regional nationalism in the eastern part of the Arab world. Melikian and Diab, for instance, found that citizenship ranked below family, ethnic group, and religion as a source of solidarity among Arab students at the American University in Beirut.²¹

Despite popular notions of the oneness of the Arab world, students of the area have long observed variations in character, sentiments, and bonds of solidarity. Militant Arab nationalists attribute these differences to imperialist machinations and claim that they will disappear once the Arab world is completely independent and united. Evidence from other countries, however, shows that "sectionalism" is quite likely to persist even after unification.²²

Table 7 shows that significant differences of opinion exist among the survey respondents from the various Arab countries. The Lebanese

and Egyptian elites are clearly the most eager for a political compromise. Personal interviews and the extensive political literature support these findings. The Syrian sample is apparently more representative of the alienated and disgruntled intelligentsia who have left their homeland than of those who are in control of the country's affairs. This conclusion gains credence when the responses of the young (under 30 years of age) and the old are compared. In each country, except Saudi Arabia, the young are more militant. The most striking difference, however, is to be found among the Syrian respondents, where 85 per cent of the young opted for boycott and eventual defeat of Israel, as opposed to 47 per cent of their elders.

The least compromising are the Iraqis, followed by the Saudis and Kuwaitis. Thus, the longer the conflict lasts, the more radicalized the fringe areas of the Arab world apparently become.

Conclusions

The results of this study essentially substantiate research in other, especially economically advanced, countries where religion, sectionalism, age, and level of education were shown to be important determinants of political attitudes and behavior. Among the Arab elite, marriage, residence and employment in the West are factors which contribute to moderation on the Palestine-Israel question. Neither locale (urban-rural) nor socioeconomic background, however, was found to be a significant factor. It is suggested that, with respect to the Arab-Israeli conflict, the emotional and ideological commitments of Arab elite far outweigh the influence of class, status or locale.

pletely silence, any unpopular views; and (2) political writers do not always or necessarily reflect accurately the opinions of their coreligionists. Only in Lebanon, where freedom of expression is fairly well tolerated and where sectarian attitudes flourish, is it possible to attempt an ascertainment of the views of the differentiated public from the published political literature.

²¹ Melikian and Diab, p. 159.

²² For evidence from the U.S., see V. O. Key, *Politics, Parties, and Pressure Groups* (New York: Thomas Y. Crowell, 1964); and for Britain, see J. Blondel, *Voters, Parties, and Leaders: The Social Fabric of British Politics* (Baltimore, Md.: Penguin Books, 1965).

Parties as Utility Maximizers*

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This article introduces two new models of political party decision making. Both models assume that political parties are solely interested in policy and that winning the election is just a means to that end. In one, the parties are competitive, while in the other the parties collude. The main result, in either case, is that the parties tend to be unresponsive to the interests of the voters.

This analysis is in marked contrast to the work by Anthony Downs and others (henceforth referred to as the traditional model), which assumes that the political parties are only interested in winning the election.¹ An im-

portant implication of this literature is that parties are very sensitive to the interests of the electorate because both parties take a position close to the preferred position of the median voter.

In order to compare the three models I shall first discuss the traditional model.

The Traditional Model

In this model the following assumptions about political parties are made: (a) *The party is a team*—a group of individuals which acts as a single decision maker. The reason for assuming a team, instead of a coalition, is that this makes the analysis easier, but does not greatly distort the results. (b) *The goal of the party leaders is to win the election*—so that they may enjoy the prestige and income of office. (c) *There are infinite barriers to entry of a new party*. This assumption is justified on empirical grounds, for the set of parties which wins elections is quite stable over a long period of time. (d) *The incumbent party makes known its platform first* (platform stands here for policies and actions of a party); i.e., the incumbent implements policy to which the opposition responds by agreeing with the proposals or suggesting alternatives. It is also usually assumed that the world is single peaked, meaning that there exists an ordering of all possible platforms such that every voter's utility function is a single peaked function of it. This ordering can be conceived of as a left-right continuum. Any position to the left (or right) of a person's most preferred position will be less favored the farther it is to the left (or right). (See Figure 1.)

Under these conditions, both parties will "look alike," each taking a position close to that of the median voter.² The proof can be

maximizing the probability of winning the election). Consequently some of them are not meant to be a description of winner-take-all elections (as in the U.S.) but of an ideal type (such as a benevolent dictator) or of a coalition government.

²With single peakedness the incumbent need not present its platform first. While Downs and others have claimed that abstention will pull the parties apart, this is not true if the parties want to maximize the probability of being elected (although it is possible to have other globally stable equilibria with both parties having a 50 per cent chance of winning and the parties not being identical). See Donald Wittman, "Theories of Optimal Political Party Decision Making," unpublished Ph.D. thesis in Economics, University of California, Berkeley, 1970.

* I would like to thank George Akerlof, Michael Leiserson, George Von der Muhll, and Benjamin Ward for their helpful comments.

¹This refers to the following works: Duncan Black, *The Theory of Committees and Elections* (Cambridge University Press, 1958); Otto A. Davis and Melvin J. Hinich, "A Mathematical Model of Policy Formation in a Democratic Society," in *Mathematical Applications in Political Science*, II, ed. J. Bernd (Dallas: Southern Methodist University Press, 1966), 170-208; Davis and Hinich, "On the Power and Importance of the Mean Preference in a Mathematical Model of Democratic Choice," *Public Choice*, 5 (Fall, 1968), 59-72; Davis and Hinich, "Some Results Related to a Mathematical Model of Policy Formation in a Democratic Society," *Mathematical Applications in Political Science*, III (Charlottesville: University of Virginia Press, 1967); Robert Dorfman, "General Equilibrium with Public Goods," IP 95 (Berkeley: Institute of Business and Economic Research, Center for Research in Management Science, June 1966); Anthony Downs, *An Economic Theory of Democracy* (New York: Harper and Bros., 1957); Gerald Garvey, "The Theory of Party Equilibrium," *American Political Science Review*, 60 (March 1966), 29-38; Melvin Hinich and Peter Ordeshook, "Plurality Maximization vs. Vote Maximization: A Spatial Analysis with Variable Participation," *American Political Science Review*, 64 (September, 1970), 772-791; Harold Hotelling, "Stability in Competition," *The Economic Journal*, 39 (March, 1929), 41-57; Jerome Rothenberg "A Model of Economic and Political Decision Making," (Rothenberg has a number of models; in one the party is interested in platform, and thus similar to the alternative model, but it does not take into account counter moves by the other party), in *The Public Economy of Urban Communities*, ed. J. Margolis (Baltimore: Johns Hopkins Press, 1965); Joseph A. Schumpeter, *Capitalism, Socialism, and Democracy* (New York: Harper Bros., 1950); Arthur Smithies, "Optimum Location in Spatial Competition," *The Journal of Political Economy*, 49 (June, 1941), 423-439; Gordon Tullock, "A Simple Algebraic Logrolling Model," *American Economic Review*, 60 (June, 1970), 419-426.

A number of these articles assume that the party either maximizes the number of votes it receives or maximizes its plurality (these are not equivalent to

characterized as follows: Each party's platform places the party on some part of the left-right continuum. All the voters whose most preferred position is to the left of the Left Party's platform will vote for the Left Party, because they prefer the Left to the Right. Since the Left Party is solely interested in winning the election, it is to the advantage of the Left Party to move right, as it will not lose the vote of those citizens to its left, but will be more sure of attracting the voters who stand between it and the Right Party. The same logic applies to the Right Party. The result is that each party has a platform close to the desire of the median voter, and therefore the two parties look alike.

The model has a number of interesting implications. If a left-right continuum exists, then the notion of an extremist is created and well defined. An extremist is a person who is at one of the two ends of the continuum. If the voters are clustered in the middle, the extremist's view is a minority view; for him to have his way would be undemocratic. A left-right continuum also defines who a voter's political enemies are:

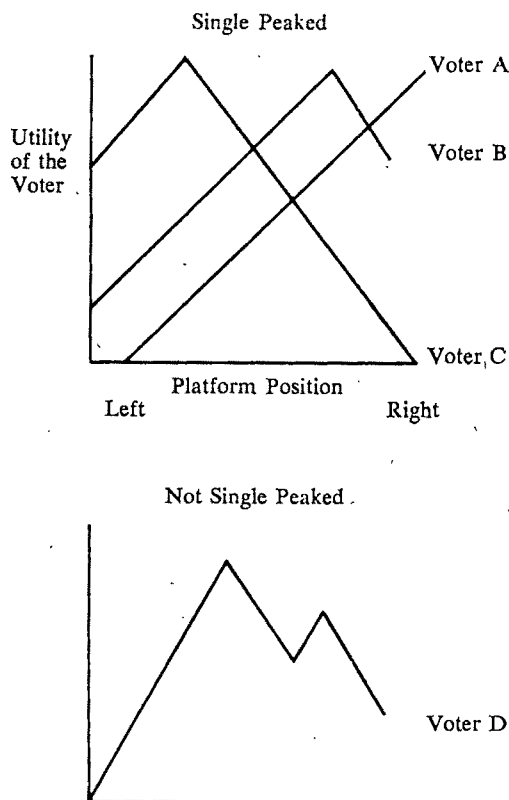


Figure 1. The world is single peaked if there exists a continuum such that each voter's utility function is a single peaked function of the continuum.

Table 1. Intransitivity of Majority Rule^a

	X	Voter Y	Z
First Choice	A	B	C
Second Choice	B	D	E
Third Choice	C	E	D
Fourth Choice	D	C	A
Fifth Choice	E	A	B

^a No platform exists such that a majority of voters do not prefer another platform; i.e., a majority prefers A to B, B to C, C to D, D to E, and E to A. Furthermore there are several platforms preferred to any one platform. E.g., a majority of voters prefer D to E, C to E and B to E.

Enemies are people whose positions are farther away, while friends are closer to your most preferred position. The extremes can share no common interest with each other that are not shared with the middle. The friends of the Left are always the moderate Left, while the enemies of the Left are always the Right.³

In the traditional model, each party works for its own self-interest, yet each ends up presenting the same platform—which is preferred by a majority of voters to any other one; selfishness and competition by the parties are shown to lead to an optimal result. Adam Smith's "Invisible Hand" has been applied to politics.

The traditional model yields very meager results if the world is intransitive. In an intransitive world, no platform exists which is preferred to all the others.⁴ This can best be seen in the following chart (Table 1). A majority of voters prefers A to B, and a majority prefers B to C, but a majority does not prefer A to C. Instead a majority prefers C to A; thus whatever platform the incumbent chooses, the opposition can always find another platform which a majority of voters prefers. Consequently, the incumbent always loses. The traditional model does not predict which platforms either the incumbent or the opposition will choose.

Intransitivity not only makes the model uninteresting but also undermines the assumption that each party's goal is solely to win the election. An intransitive world may mean that for any given set of policies of the incumbent

³ If the parties present lotteries it is possible for the extremes to prefer a risky lottery and the median voter to prefer a nonrisky (degenerate) lottery; but then there always exists another risky lottery which the median voter and one (and only one) of the two extremes prefer to the original risky lottery.

⁴ For a more detailed presentation see Kenneth Arrow, *Social Choice and Individual Values*, Cowles Commission for Research in Economics, Monograph No. 12 (New York: Wiley, 1951).

party, the opposition has a large number of platforms, any one of which is preferred to the incumbent's platform by a majority of voters (e.g., in Table 1; B, C and D are all preferred to E). Since the opposition has so many ways of defeating the incumbent party, *the problem is not how the opposition party will be able to obtain a majority but which majority it will choose*. Thus the opposition will find some other objective besides just winning the election. The incumbent party knows that it will lose the election no matter which platform it presents, therefore it too will have some other objective than winning. Uncertainty (or imperfect information) does not reduce the problem, for it allows situations to arise where the majority votes for a party, even though perfect information would result in the majority voting against. Consequently, the leaders have a choice of many platforms which, owing to lack of information, look just alike to the voters. Again they will have some other rule besides just maximizing the probability of winning the election.⁵ Therefore it cannot be said that the goal of the leaders in the political parties is to get elected or reelected so that they may enjoy the prestige and income of office.

Since the lack of transitivity is so devastating to the model, it is important to investigate whether the world is single peaked or not.⁶ Ar-

⁵ Other ways of making the model more complicated do not change the essential point that the parties have a wide variety of platforms which will yield the same probability of winning, and thus the parties will have some other criterion for choosing among platforms than just winning the election. For example, if the voters are very hesitant to vote against the incumbent, then the incumbent can win on almost any platform. The interesting question becomes: which winning platform will the incumbent choose?

⁶ Actually single peakedness does not imply transitivity if lotteries are allowed (see Richard Zeckhauser, "Majority Rule with Lotteries on Alternatives," *Quarterly Journal of Economics*, 83 (November, 1969); 616-703. Nor does lack of single peakedness imply a lack of transitivity; however, it is not clear when these cases such as (1) Latin-squarelessness or (2) closed compact sets would arise.

For theoretical arguments against single peakedness see David Chapman, "Models of the Working of a Two-Party Electoral System, Part I," *Papers on Non-Market Decision Making*, 3 (Fall, 1967), 19-38; Amartya Sen, *Collective Choice and Social Welfare* (San Francisco: Holden-Day, Inc.); Benjamin Ward, "Majority Rule and Allocation," *Journal of Conflict Resolution*, 5, No. 4 (December, 1961), 379-389; for empirical arguments see Maurice Duverger, *Political Parties: Their Organization and Activity in the Modern State*, trans. Barbara and Robert North (London: Methuen and Company, 1954); Donald E. Stokes, "Spatial Models of Party Competition," in *Elections and the Political Order*, A. Campbell, P. Converse, W. Miller, and D. Stokes (New York: John Wiley and Sons, 1966), 161-179.

guments against single peakedness have been made on theoretical as well as on empirical grounds yet many still accept the notion, probably because it seems reasonable (i.e., people who are Left on one issue are often Left on another). Unfortunately, the concept of single peakedness is extremely restrictive, and thus, even if a liberal were liberal on every issue and a conservative conservative on every issue, it would not necessarily imply single peakedness. The reason is that the voters may weight the issues differently, i.e., there may be a great deal of difference to the Left voter between a liberal and moderate position on the first issue, and a relatively small difference between a liberal and conservative position on the second and third issues. Thus the Left voter prefers a platform which is liberal on the first issue and conservative on the second and third to a platform which is moderate on the first and liberal on the second and third issues. Similar possibilities arise for the conservative and moderate voters. Thus even if all the Left voters are consistently left on each issue and the Right voters consistently right, this still does not imply single peakedness or transitivity.⁷ All this can be seen in Table 2, where the ranking of each platform by L (left), R (right) and M (middle) are given.

Even the strongest cases for single peakedness often fail to hold when actually tested. For example, most political scientists would place Wallace, Nixon, Humphrey, and McCarthy in that order from right to left. In fact, 40 per cent of those Californians who were going to vote for Wallace in the election requiring a choice among Wallace, Nixon, and Humphrey would have voted for McCarthy if he had run.⁸ This means that for at least two-fifths of the Wallace supporters, Wallace and McCarthy

For articles on the other side see Kenneth J. Arrow, "Tullock and an Existence Theorem," *Public Choice*, 6 (Spring, 1969), 105-112; Duncan Black, "On Arrow's Impossibility Theorem," *The Journal of Law and Economics*, 12 (October, 1969), 227-248; Richard G. Niemi, "Majority Decision Making with Partial Unidimensionality," *American Political Science Review*, 63 (June, 1969), 488-497; and Gordon Tullock, "The General Irrelevance of the General Impossibility Theorem," *Quality Journal of Economics*, 81 (May, 1967), 256-270.

⁷ It should be noted that all the work concerned with a multidimensional issue space has had to put restrictions on the weighting of the issues or on the distribution of voter attitudes in order to avoid intransitivity.

⁸ See *San Francisco Chronicle* (Final edition, September 23, 1968), p. 1, col. 2. It is also very likely that a considerable number of the 60 per cent who still would have voted for Wallace had McCarthy as their second choice.

Table 2. Intransitivity of voting even when the individual voters are consistently on the left, right, or in the middle on each issue

Each Voter's Ranking of the Platforms											
Left Voter L				Middle Voter M				Right Voter R			
Rank of Platform	1	Issue 2	3	Rank of Platform	1	Issue 2	3	Rank of Platform	1	Issue 2	3
1	L	L	L	1	M	M	M	1	R	R	R
2	L	L	M	2	R	M	M	2	M	R	R
3	L	L	R	3	L	M	M	3	L	R	R
4	L	M	L	4	M	L	M	4	R	R	M
5	L	M	M	5	M	R	M	5	M	R	M
6	L	M	R	6	R	L	M	6	R	R	L
7	L	R	L	7	L	L	M	7	L	R	M
8	L	R	M	8	R	R	M	8	M	R	L
9	L	R	R	9	L	R	M	9	L	R	L
10	M	L	L	10	M	M	L	10	R	M	R
11	M	L	M	11	R	M	L	11	R	L	R
12	M	L	R	12	L	M	L	12	M	M	R
13	M	M	L	13	M	M	R	13	M	L	R
14	M	M	M	14	R	M	R	14	L	M	R
15	M	M	R	15	L	M	R	15	L	L	R
16	M	R	L	16	M	L	L	16	R	M	M
17	M	R	M	17	M	R	L	17	R	L	M
18	M	R	R	18	M	L	R	18	M	M	M
19	R	L	L	19	M	R	R	19	R	M	L
20	R	L	M	20	R	L	L	20	M	L	M
21	R	L	R	21	L	L	L	21	R	L	L
22	R	M	L	22	R	R	L	22	L	M	M
23	R	M	M	23	R	L	R	23	M	M	L
24	R	M	R	24	L	R	L	24	L	L	M
25	R	R	L	25	L	L	R	25	M	L	L
26	R	R	M	26	R	R	R	26	L	M	L
27	R	R	R	27	L	R	R	27	L	L	L

The preceding information can be put into the following format:
Rank of Platform by Voter

Platform	L	M	R	Platform	L	M	R	Platform	L	M	R
LLL	1	21	27	MLL	10	16	25	RLL	19	20	21
LLM	2	7	24	MLM	11	4	20	RLM	20	6	17
LLR	3	25	15	MLR	12	18	13	RLR	21	23	11
LML	4	12	26	MML	13	10	23	RML	22	11	19
LMM	5	3	22	MMM	14	1	18	RMM	23	2	16
LMR	6	15	14	MMR	15	13	12	RMR	14	14	10
LRL	7	24	9	MRL	16	17	8	RRL	25	22	6
LRM	8	9	7	MRM	17	5	5	RRM	26	8	4
LRR	9	27	3	MRR	18	19	2	RRR	27	26	1

I.e., L ranks LLL as his most preferred platform, LLM as his second most preferred platform, etc.; M ranks LLL as his 21st choice, etc. For any given platform there exists another platform preferred by two of the voters. For example, LRM is preferred to MMM by L and R while LLM is preferred to LRM by L and M.

Table 3. Presidential Preference Polls, 1968 (Harris)^a

	Per cent Preferring Democrat Contender		Given that Wallace is a Third Party Candidate	
	Per cent Preferring Republican Contender			
	A McCarthy Nixon	B McCarthy Rockefeller	C Humphrey Rockefeller	D Humphrey Nixon
June 10-17	$\frac{44}{36}$	$\frac{41}{35}$	$\frac{40}{36}$	$\frac{43}{36}$
July 8-14	$\frac{42}{34}$	$\frac{38}{32}$	$\frac{34}{37}$	$\frac{37}{35}$
July 25-29	$\frac{43}{35}$	$\frac{34}{40}$	$\frac{34}{40}$	$\frac{41}{36}$

^a In each poll McCarthy did better than Humphrey against either Nixon or Rockefeller, and Rockefeller did better than Nixon against either McCarthy or Humphrey. The Electoral College might have given Rockefeller an even better chance of winning the election than Nixon. If McCarthy, Humphrey, Rockefeller, and Nixon (in that order) represents an arrangement from left to right, then both parties chose their right-leaning candidates and each party would have done better if it had moved to the left. The results of the Gallup Poll tended to contradict the Harris Poll, while the Crossley Poll tended to support the Harris Poll.

were not poles apart; that is, there was no universally accepted definition of what constituted Left and Right.

Unfortunately no polls have been designed in order to test directly for the existence of intransitivity; however, the results of several Gallup polls do indicate that intransitivity is fairly prevalent. For example, in October 1967 a poll asking the voters whether they preferred Richard Nixon or Nelson Rockefeller to be the presidential choice of the Republican Party yielded the following percentages: Nixon—47 per cent, Rockefeller—45 per cent, and no opinion—8 per cent.⁹ It is reasonable to infer from this poll that a majority of voters preferred Nixon over Rockefeller not only for the Republican candidacy but also for the presidency. In November 1967 a presidential preference poll between Lyndon Johnson and Rockefeller resulted in the following percentages: Rockefeller—54 per cent, Johnson—40 per cent, and no opinion—6 per cent;¹⁰ while a presidential preference poll taken in December 1967 had the following results: Johnson—47 per cent, Nixon—43 per cent, and no opinion—10 per cent.¹¹ This is an intransitive situation, for Nixon is preferred to Rockefeller, and Rockefeller is preferred to Johnson, and Johnson is preferred to Nixon. Similar results were obtained in March 1968 when all three of these polls were repeated;

however, the two presidential contests included George Wallace as a third party candidate, making the results more ambiguous.¹² These, as well as other Gallup polls, suggest that intransitivity is not just a mathematical oddity, but rather a fact of political life.

In addition to the evidence against single peakedness, there is also considerable empirical evidence against the assumption that the parties are only interested in winning the election. For example, in the 1964 U.S. presidential election, it was clear that Barry Goldwater was not the best choice for the Republican Party (if the ultimate goal was to win the election). The same situation occurred in 1968, when although the polls had shown that Nixon would do worse than Rockefeller against either Humphrey or McCarthy and that Humphrey would do worse than McCarthy against either Nixon or Rockefeller, nevertheless, Nixon and Humphrey won their party nominations (See Table 3). Kenneth Prewitt has shown that on the local level, most office holders do not intend to run for reelection.¹³ There are also many offices in which the official is legally prevented from being reelected (e.g., a second term president). In both these cases it is unreasonable to assume that they make political decisions while in office based on maximizing the probability of being reelected.

⁹ *Gallup Opinion Index*, No. 30, 4.

¹⁰ *Gallup Opinion Index*, No. 32, 8.

¹¹ *Gallup Opinion Index*, No. 32, 7.

¹² Nixon—36, Johnson—44, Wallace—12, No Opinion—8. (*Gallup Opinion Index*, No. 33, 11); Rockefeller—40, Johnson—37, Wallace—13, No Opinion—

A large body of political science literature claims that the President is very important in molding public opinion. If this is true, then he can mold it toward his own purposes. The *Pentagon Papers* revealed how President Johnson tried to manipulate both the events and the news of the war so that the American people would be willing to fight in Vietnam.¹⁴ He was trying to get the will of the people to coincide with his policies more than he was trying to make his policies coincide with the will of the people. Certainly if he only chose those policies which maximized the probability of his reelection he would not have made so great an effort to conceal the nature of the war (if this is what the American people wanted).

In conclusion, considerable evidence suggests not only that the world is not single peaked but also that the parties are not solely interested in winning elections. It appears that the traditional model rests on a very weak foundation.

The Alternative Competitive Model

The alternative competitive model assumes that the parties are solely interested in policy and that winning the election is just a means to that end. Some insight into the differences between the alternative competitive model and the traditional model may be found by looking at the analogous models in the business world. The traditional model of political party decision making is a sales maximization model: the firm which has sold the most cars is the winner (similarly, the party with the most votes is the winner).¹⁵ This interpretation, however, is in-

correct in both cases. The firm is interested in maximizing its profits, so, depending on the structure of the market, it might sell fewer cars at a higher price or perhaps sell none at all. Because we may observe that firms are trying to sell more cars does not mean that this is their goal; rather, it is a method of maximizing profits.

In the alternative model it is assumed that the parties are trying to maximize their expected utility. In so doing they will sometimes use the method of increasing the expected probability of winning the election, but winning the election is still the method and not the goal. Some political scientists see the parties as having two contradictory goals when choosing a platform. On the one hand, the party wants that platform which will maximize the probability of the party's winning the election; on the other hand, the party wants that platform which gives the party the most utility if it wins. The problem involved with this view of the parties can best be seen by again looking at a similar situation in economics. We could say that an automobile firm has two contradictory goals: (1) to have as great a profit per car as possible and (2) to maximize its sales; however, a more correct view is that the only goal is to maximize total profits. Similarly, a party chooses that platform which maximizes its expected utility. This will become clearer as the model is developed.

As in the traditional model, it is assumed that there are infinite barriers to entry of a new party, that the incumbent party presents its platform first, and that the party is a team.

An election that is only concerned with income distribution will be analyzed first. In this situation the voter wants as much income as possible for himself and does not care how much any of the other voters receives. This is similar to a constant sum game—characterized by conflict.

The allocation of income is intransitive (i.e., no one platform is preferred by the majority to every other platform) as Table 4 shows. A majority (X and Y) prefers platform B to A; a majority (Y and Z) prefers C to B; similarly D is preferred to C, and A is preferred to D; so whichever platform the incumbent chooses, the opposition can always find another platform to defeat it. What is the optimal strategy for the parties in a case like this? In a situation

10. (*Gallup Opinion Index*, No. 33, 13); Nixon—48, Rockefeller—42, No Opinion—10. (*Gallup Opinion Index*, No. 34, 9). It should be noted that in earlier surveys, the relative ranking of the two major party candidates was not affected by the inclusion (or omission) of George Wallace as a third party candidate.

¹³ Kenneth Prewitt, "Political Ambitions, Volunteerism, and Electoral Accountability," *American Political Science Review*, 64 (March, 1970), 5–17. On page 10 Prewitt says, "Among the nearly 500 men who govern the cities of the San Francisco Bay Area, about one-fifth will be planning to retire voluntarily at the end of the present term and another 30 to 50 per cent will be planning to retire after only one more term." Furthermore, he shows that the councilmen do not worry about going against the majority and thus they do not seem to formulate policies in order to win elections.

¹⁴ *Pentagon Papers* (New York: Bantam Books), 1971.

¹⁵ There are some other variants. For example, Riker in *The Theory of Political Coalitions* (New Haven: Yale University Press, 1962) suggests that the parties are only interested in a minimal winning coalition. This is in response to the usual formulation of the traditional model which assumes that parties maxi-

mize the numbers of votes they receive. One reason for forming a minimal winning coalition is that the rewards are divided among fewer people. These rewards may be of office or of policy.

where the incumbent knows that he is bound to lose the election, the incumbent will choose that platform which will result in the greatest benefit to himself by forcing the opposition to give the incumbent as much as possible. The incumbent thereby "wins" even in losing the election. The following example will show the underlying concepts. Let there be five voters—X, Y, A, B, and C (where X and Y not only vote, but also present platforms; henceforth X will be referred to as the incumbent party and Y as the opposition party). For any given platform presented by the incumbent, the opposition will offer himself more income than he was offered by the incumbent (which might have been zero) and just enough income to the other participants in order to win the election. The opposition will offer income to those $(N - 1)/2$ players besides himself who were offered the least by the incumbent, and to each of these he will only offer a small amount more—to offer more to these players or to other participants would mean that he would have less for himself, but would not mean that the probability of his winning the election would be any higher as he is already winning the election with probability 1 (because $(N - 1)/2 +$ the vote by Y is a majority). The incumbent, realizing that the opposition will take the above strategy, will make sure that he offers himself as much as possible and still be one of the $(N - 1)/2$ voters who is offered the least by the incumbent. With five players, this means that X will offer the following platform:

X	Y	A	B	C
$1/3 - 2e$	0	$1/3 + e$	0	$1/3 + e$

(e an infinitesimally small number).

And in response Y will offer the following platform:

X	Y	A	B	C
$1/3$	$2/3 - d$	0	d	0

(d an infinitesimally small number).

If Y had offered any other platform, he would not have received as much, while if X had offered a platform such as the following:

X	Y	A	B	C
$1/2$	$1/2$	0	0	0

X would have ended up with nothing as Y would have presented the following platform:

X	Y	A	B	C
0	$1 - 2e$	0	e	e

The parties end up with all of the income and

the voters with nothing.¹⁶ Although this example is an extreme simplification of reality, the result itself is very robust; i.e., whether there is one election or an infinite series of elections, perfect information or imperfect information, one issue or many issues, an advantage to being the incumbent or a disadvantage, transitivity or intransitivity, the parties tend to have their way. For example, in a single peaked world with perfect information (note: it is not necessary to assume the incumbent presents his platform first in this case) even if the voters were to the left of both parties, neither party would present a platform to the left of the Left Party's preferred position.¹⁷ To do so would make both parties worse off, as both are interested in policy and not in winning the election. Of course when the preferred position of the parties are more extreme than the preferred positions of the voters, (i.e., the voters are between the positions of the parties), then the situation looks very similar to the traditional model, and both parties will have platforms close to the median voter's preferred position.

If the voters are uninformed, then they may receive some income, but the parties always have the option of offering nothing to the voters; e.g., with *totally* uninformed voters, whichever platform a party offers has the same chance of winning the election, so each party would offer itself 100 per cent of the income. Similarly, in a single peaked election the par-

¹⁶ This example also shows why the concept of extremist may not be very useful. The notion of extremist is no longer well defined nor can friends and enemies be as easily distinguished as in the single peaked case. To have been in a minority in an election does not imply that had your most preferred position been offered as a choice to the voters it would not have been preferred by a majority. Thus the "extremist" position may not be extreme.

¹⁷ This probably characterizes the 1968 U.S. presidential election where each party would have had a greater probability of winning if it had moved "left" (see Table 3).

Table 4. Intransitivity of Allocation of Income

Platform	Voter		
	Allocation of Income to Voter		
	X	Y	Z
A	1/3	1/3	1/3
B	1/2	1/2	0
C	0	3/4	1/4
D	1/4	1/4	1/2

X and Y prefer B to A; Y and Z prefer C to B; X and Z prefer D to C; X and Y prefer A to D.

ties would present their own most preferred position. Unlike the traditional model, competition does not lead to an optimal result (except for the parties).

In the single peaked case, as the voters become less informed, the parties become less similar.¹⁸ Again assume that each party's most preferred platform is to the right of all the voters. Then, if the voters are slightly uninformed, the Right Party will be slightly to the right of the Left Party, hoping that the voters will mistakenly vote for it. The Right Party will not move very far right as it knows that the greater the difference between the parties, the less likely the voters are going to vote for it. If, however, the voters are totally uninformed, the Right Party will move all the way to its most preferred position.

The Restricted Competition Model

If parties are only interested in winning the election, there cannot be collusion, as winning the election is a zero-sum game.¹⁹ But in a situation where the parties are interested in policy, collusion may bring great rewards.

In an election solely concerned with income distribution, when the voters are imperfectly informed, marginal rewards lose their potency. This may mean that the parties will offer more than infinitesimally small amounts to the voters (as a whole) and thus the parties will receive less. Much more important, the outcome becomes very risky. To illustrate, if the voters are totally uninformed, then the optimal choice of each party is to offer itself all of the income and nothing to any of the other participants. The result is that each party has a 50 per cent chance of receiving all the income and a 50 per cent chance of receiving nothing; but if the parties have decreasing marginal utility of income, then they would prefer a 100 per cent chance of receiving one-half (or perhaps one-third). See Figure 2.

Whenever the outcome is uncertain and the parties are risk averters, there exists a collusive agreement with both parties presenting the same platform, which results in both parties being

¹⁸ Given certain restrictions on the utility functions of the parties and on the probability of the voters making an incorrect choice, it can be shown that the platforms that the parties end up choosing will result in each party's having a 50 per cent chance of winning the election.

¹⁹ Unless there is more than one office, in which case the parties may agree to split the offices. There may be limited collusion with respect to campaign spending. See R. Curry and L. Wade, *A Theory of Political Exchange* (Englewood Cliffs, New Jersey: Prentice-Hall), 1968.

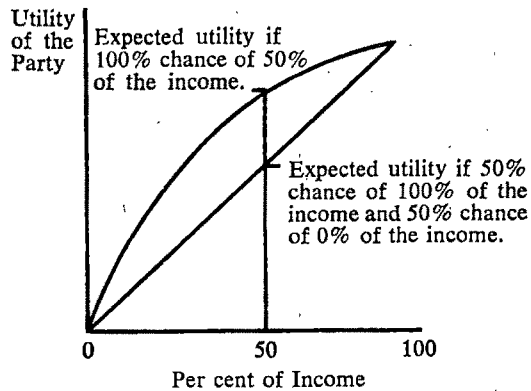


Figure 2. The Party's utility as a function of income. With decreasing marginal utility of income, the party is a risk avoider, preferring a sure thing to the gamble which, on average, gives the party the same percentage of income.

better off.²⁰ Thus if the voters are completely uninformed, both parties have a higher expected utility if each presents a platform offering one-half to each of the two parties than if each offers everything to itself. Once again it is shown that the parties will receive all of the income. The model also predicts that the parties will look just alike (to reduce risk): the same results hold when the election is concerned with issues. It "pays" the parties to collude and to present similar platforms.

When oligopolists collude, competition is, for the most part, limited to advertising and product differentiation—price competition is avoided. The parties, too, avoid "price competition" and compete only in relatively unimportant areas. Major issues rarely become "issues" as the parties are unlikely to offer a choice to the voters on more fundamental questions. The collusion between the parties can be seen as a major coalition. To the degree that the parties compete with each other, a party makes a minor coalition with some of the voters. The distinction between major and minor coalitions is very useful, for often in political science literature the parties are seen as two coalitions against each other. Suppose the election is concerned with income allocation, and suppose also that party B, the big business party, offers 53 per cent of the income to itself, 47 per cent to L (the unionized labor party), and 0 to M, the cultural minority, while L offers 50 per cent to itself, 47 per cent to B, and 3 per cent to M, then it is not correct to say that L and M

²⁰ Whenever collusion occurs no assumption about the order of presentation of the party's platforms need be made.

are in a coalition against B, for B is better off than M, even though it loses the election. It is more useful to say that L and B are in a major coalition and L and M are in a minor coalition for L and B never "lose" very much.

Although there may be explicit agreements (such as between the Conservatives and Liberals in Colombia), it is much more likely that collusion is implicit. The parties may find many ways of restricting competition with each other; bipartisanship, promotion of mutually acceptable ideologies, marginal changes in the previous administration's policies, and recruitment of those who are not antagonistic to the other party. Even the belief in the impossibility of certain platforms being able to win the election may be a form of implicit collusion if there is more fiction than substance to the belief. Thus, the parties compete more *with* the voters than *for* the voters or *with each other*.

Conclusion

This article suggests that the traditional approach has taken the wrong economic model to

explain political party behavior. The traditional model views the parties as being similar to perfectly competitive sales-maximizing firms. This is in contrast to the restricted competition model which sees the parties as being analogous to profit-maximizing oligopolists. Just as oligopolists often collude against the consumers, the parties may collude against the voters. In such cases the distinction between multiparty systems and one-party systems may not be very great.

Both the alternative competitive model and the restricted competition model assume that the parties are only interested in policy. If these models are correct, then political scientists will have to do more than just count votes in order to see who won the election. For it is which party's policy is implemented, not which party receives the most votes that determines the winner.

Finally, both models have a number of interesting results—the most important one being that the political parties are not very accountable to the electorate.

The Learning of Legislative Norms*

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Studies of legislatures have uncovered the existence of informal norms or folkways or rules of the game that are presumed to be important for the maintenance of the legislative system.¹ It is often argued that the institution, be it the House or the Senate or a state legislature, must transmit its norms to legislative newcomers in order to insure the continued, unaltered operation of the institution, and that the member himself must learn these norms if he is to be an effective legislator. Whether or not this be necessary, it certainly is plausible to view freshman members of the legislative body

as undergoing a socialization process which involves the learning of legislative norms. Yet previous studies have largely concentrated on the identification of legislative norms and have devoted little attention to their transmission to the newcomer. Is the freshman legislator aware of the expected types of behavior prior to taking his seat or does he learn them while in office? If he learns them in office, who actually are the agents of transmission? Or is the socialization process so informal that we cannot even speak of well-defined agents?

These questions and others have not been fully addressed, and thus the focus of this paper is on the learning of norms by freshman members of the United States House of Representatives.² My interest is in individual learning of legislative norms, regardless of whether the content learned is in conformity or opposition to the norms. Too often the emphasis in socialization research has been on system maintenance rather than individual adaptation, leading one to dismiss findings of deviance too readily. The data come from a broader panel study of the learning that freshman members elected to the House of Representatives in November, 1968, underwent with respect to perceptions of their job, legislative norms, and sources of voting cues.³ Since a research inter-

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¹ The literature on legislative norms is quite extensive. For a discussion of Senate norms, see Donald R. Matthews, *U.S. Senators and Their World* (New York: Random House, 1960) and William S. White, *Citadel: The Story of the U.S. Senate* (New York: Harper & Brothers, 1957) and two articles by Ralph Hitt, "The Outsider in the Senate: An Alternative Role," *American Political Science Review*, 55 (September, 1961), pp. 566-75 and "The Morse Committee Assignment Controversy: A Study in Senate Norms," *American Political Science Review*, 51 (June, 1957), pp. 313-29. For information on House norms, see Charles L. Clapp, *The Congressman: His Work as He Sees It* (New York: Doubleday & Company, 1963); Donald G. Tacheron and Morris K. Udall, *The Job of the Congressman* (Indianapolis and New York: The Bobbs-Merrill Company, 1966); and Clem Miller, *Member of the House: Letters of a Congressman* (New York: Charles Scribner's Sons, 1962). Also relevant to the House is the growing body of literature on committee integration and the norms that promote it. The foremost articles in this area are Richard F. Fenno, Jr., "The House Appropriations Committee as a Political System: The Problem of Integration," *American Political Science Review*, 56 (June, 1962), pp. 310-24 and John F. Manley, "The House Committee on Ways and Means: Conflict Management in a Congressional Committee," *American Political Science Review*, 59 (December, 1965), pp. 927-39. See also the broader studies by these authors: Richard F. Fenno, Jr., *The Power of the Purse: Appropriations Politics in Congress* (Boston: Little, Brown and Company, 1966) and John F. Manley, *The Politics of Finance* (Boston: Little, Brown and Company, 1970). At the state legislative level, the reader can turn to John Wahlke et al., *The Legislative System* (New York: John Wiley & Sons, 1962); Malcolm E. Jewell, *The State Legislature: Politics and Practice* (New York: Random House, 1962); and James D. Barber, *The Lawmakers: Recruitment and Adaptation to Legislative Life* (New Haven: Yale University Press, 1965). Finally, for an example of research in a non-American setting, see Allan Kornberg, "The Rules of the Game in the Canadian House of Commons," *Journal of Politics*, 26 (May, 1964), pp. 358-80.

² Recently, there has been a greater emphasis on the legislative newcomer and his adaptation to the institution, with one article specifically concerned with the rules of the game, although from a different perspective than employed herein. In a panel study of freshman California assemblymen, Bell and Price found that norm learning did take place. They wrote: "A whole host of 'in-house' norms were cited after legislative experience had been acquired. Rules pertaining to committee decorum and management of bills were frequently cited in the second and third interviews; they were not often cited in the first interview." See Charles M. Price and Charles G. Bell, "The Rules of the Game: Political Fact or Academic Fancy?" *Journal of Politics*, 32 (November, 1970), p. 855. For more general discussions of freshman adaptation, see Charles G. Bell and Charles M. Price, "Pre-Legislative Sources of Representational Roles," *Midwest Journal of Political Science*, 13 (May, 1969), pp. 254-70 and Irwin N. Gertzog, "The Socialization of Freshman Congressmen: Some Agents of Organizational Continuity," Paper prepared for delivery at the 66th annual meeting of the American Political Science Association, Biltmore Hotel, Los Angeles, September 8-12, 1970, pp. 1-26.

³ Herbert B. Asher, "The Freshman Congressman: A Developmental Analysis" (Ph.D. dissertation, University of Michigan, 1970).

est in learning is a longitudinal concern, a two-wave panel design was employed, the first set of interviews conducted in late January and February of 1969, and the second set the following May. The purpose of the panel was to capture the changes that the 91st class members underwent in the first few months of their legislative service, a period that seemed *a priori* to be crucial in talking about learning. Of the 37 freshmen in the 91st Congress, 30 were interviewed at t_1 (late January and February) and of these 30, 24 were reinterviewed at t_2 .⁴

A norm has been defined herein as a rule or standard of conduct appropriate to a person in a specified situation within a group. The norm describes the type of behavior expected by almost all of the other members of the group and often, though not necessarily, has associated with it sanctions for deviance. Since concepts such as role and norm are useful because of their normative or mutual expectations component, and since a definitional attribute of a norm is that it be shared to a high degree by the members of the group, a sample of non-freshman members of the House was also interviewed. Certainly if incumbent representatives

⁴ The selection of the times for the two waves, particularly the first, was no easy matter. Since a prime concern of my research was freshman attitudes to legislative norms, I wished to interview sufficiently early so as to ascertain these attitudes while uninfluenced by House service. But seminars for freshman representatives were held early in the session (January 8 through January 13) and at these meetings a large amount of material, some of it relevant to legislative norms, was presented to the freshmen. Thus, it would have been advantageous to talk to the newcomers before these seminars were held. But in terms of my interest in internal House voting cues, interviewing in early January would have made little sense as almost no legislative business was underway. In retrospect, the problem was not very serious as evidenced by a question included in the interview schedule designed to measure the impact of the freshman seminars. Freshmen generally indicated that the seminars were interesting and highly informative with respect to parliamentary procedures and the services available to congressmen, but generally attributed little influence to the seminars vis-à-vis House norms. Similar opinions were expressed by freshman participants in the 1959 seminars. My thanks go to Representative Morris Udall for allowing me to rummage through his files on previous freshman seminars.

There remains the possibility that substantial norm learning may have occurred in the postelection period prior to the freshman's formal entry into the House, a "waiting period" during which many freshmen took some action with regard to their future committee assignment or journeyed to Washington to handle personal business. Thus, there was certainly opportunity for norm learning to have occurred in this period, although freshmen indicated little such activity in response to questions about the waiting period. Unfortunately, resource constraints prevented a third wave of interviews shortly after the November election.

agreed to the norms there would be an environment more supportive of freshman learning them. Conversely, if there were marked disagreement about the norms on the part of non-freshmen, we might then wish to reformulate or even reject these "norms."⁵

A variety of approaches can be used to ascertain information and attitudes about legislative norms. Wahlke and his colleagues employed an open-ended question that asked the respondents to identify the rules of the game in their respective state legislatures.⁶ Such an approach is particularly valuable for identifying those norms most salient to the representative, but it may fail to elicit the nascent attitudes that freshmen early in their careers are likely to possess. Hence, freshmen and nonfreshmen were queried about specific norms, the determination of which was based upon a survey of the existing literature. The main norms investigated were specialization, reciprocity, legislative work, courtesy, and aspects of apprenticeship including learning the House rules, restrained participation, and attendance on the floor and in

⁵ The nonfreshman members of the House were stratified according to party, region, and seniority, and proportionate samples were selected randomly within these strata. The interviews obtained were very representative of Republican House membership, while they underrepresented Democrats in general and senior, southern Democrats in particular. The following figures indicate how well the nonfreshman sample matches the overall population in terms of party and seniority where a threefold classification of seniority based on the number of terms served prior to the start of the 91st Congress is used. Low seniority was defined as three or fewer terms, moderate as four and five terms, and high as more than five terms.

	Republicans		Democrats	
	% of population	% of sample	% of population	% of sample
Seniority				
Low	55	49	36	48
Moderate	18	20	22	26
High	27	31	42	26
Total %	100%	100%	100%	100%
N	(174)	(35)	(224)	(31)

Despite the underrepresentation of senior Democrats, there were no significant ideological differences between the samples selected from each stratum and the corresponding parent stratum as measured by CQ conservative coalition support scores and ADA and ACA ratings.

⁶ The actual question that Wahlke and his colleagues used was: "We've been told that every legislature has its unofficial rules of the game—certain things they must do and things they must not do if they want the respect and cooperation of fellow members. What are some of these things—these 'rules of the game'—that a member must observe to hold the respect and cooperation of his fellow members?" See Wahlke et al., *The Legislative System*, p. 143.

committee.⁷ A focused interview approach was selected to collect information about these norms, although this procedure raises certain problems. For example, one cannot simply ask the representative whether a norm of reciprocity exists in the House, for such a label may be without meaning to the legislator. We must attach some behavioral tag to reciprocity, and in so doing we have a wide leeway. Thus, we might ask the representative whether he thought members should do favors for one another, but unless the representative was particularly misanthropic, we would expect unanimously affirmative responses to such an item, thereby reducing the discriminatory capacity of the question to zero. Therefore, reciprocity was operationalized in this study by placing it in the context of a voting situation, with all the attendant ambiguities and pressures. The actual question was: "Would you vote a certain way on a bill that you cared little about in order to gain the vote of a fellow representative on a bill that you did care about?" The general point to be made here is that questions seeking to uncover information about norms are best framed within a fairly specific behavioral situation. If we cannot observe behavior directly (and most often we cannot), then our questions should be as behaviorally oriented as possible.

Nonfreshman and the Norms

Table 1 presents the level of agreement expressed by our sample of nonfreshman members of the 91st Congress to a series of items about House norms.⁸ The highest level of

agreement was reached on the importance of maintaining friendly relationships, with the importance of committee work running a close second, while the weakest consensus was found on the norm of apprenticeship. Some detailed analysis of the nonfreshman responses is very illuminating.

Specialization. Among the norms, specialization appeared to be adhered to largely because of certain properties of the House: Many members asserted that the heavy and varied workload of the House, as well as its large membership, made specialization mandatory. Of the eight incumbents who did not agree that Congressmen should specialize rather than generalize, six said that the member must do both because of the interdependence of issues, while two opted for being a generalist. The two representatives who cited a generalist orientation were both extreme liberals within their party and viewed the standing committees of the House as "the hand-maidens of special interest groups."

In an attempt to learn whether their attitudes toward specialization had changed since their first term, the nonfreshmen were asked whether they had felt the same way (about specialization) when they were freshmen, and, if not,

learning the House procedural rules is? Would you ever personally criticize a fellow representative on the floor of the House? Would you vote a certain way on a bill that you cared little about in order to gain the vote of a fellow congressman on a bill that you did care about?

It should be noted here that the question about where the important work of the House is done does not tap a norm in the same sense as the other items, but rather elicits a belief or opinion. Once we have found marked disagreement with certain items thought to be legislative norms, then by definition these items no longer are norms. But for simplicity of presentation, the word norm will be used in the subsequent analysis to apply to those items such as apprenticeship about which there was substantial disagreement.

Table 1. Nonfreshman Attitudes to the Norms

	% Agreeing	N
Friendly relationships important	97	40
Important work of House done in committee	95	40
House rules important	82	40
Would not personally criticize a fellow representative	82	40
Would be likely to trade votes	81	21
Congressman should be a specialist	80	40
Freshman should serve apprenticeship	38	65

⁷ The norm of institutional patriotism was also investigated, but its operationalization was so narrow that the responses obtained were not very interesting. Members were asked whether they would ever criticize the House, and, not unexpectedly, most said that they would which presumably implies that institutional patriotism is not a viable norm. But institutional patriotism is far more complex than merely refraining from criticism so that nothing more will be said about the norm in the remainder of this paper.

⁸ Some of the nonfreshman interviews were taken at t_1 and the others at t_2 . There was very high agreement on some of the norms questions asked at t_1 and these were omitted at t_2 so that more attention could be devoted to other items. This accounts for the various N's on the norms questions. There was no reason to believe that the responses to the norms questions that were omitted at t_2 contained any systematic biases. The actual questions for each of the norms were: Do you think congressmen should specialize in a field or should try to be generalists? Do you think most of the important work of the House is done on the floor or in committees? How important do you think it is to maintain friendly relationships with your fellow congressmen? Do you think that freshman congressmen should serve a period of apprenticeship, that is, be more an observer than an active participant in the legislative process? How important do you think

what had led them to change their views. Twenty-six said yes, eleven said no, and two could not recall their earlier opinions. All eleven whose views about specialization had changed said that when they entered Congress, they did not realize how necessary specialization was, but that subsequent legislative and committee experience in the House had taught them differently. Two of the eleven said they initially thought it was possible to be knowledgeable on most important issues, but that the energy and time required for this was prohibitive.

Committee Work. While 38 of the 40 incumbents interviewed claimed that most of the important work of the House was done in committee rather than on the floor, the remaining two representatives felt that neither place was crucial. Their responses revealed skepticism about the House's effectiveness as an arena for legislation.

Most of the important work is done neither on the floor nor in committee. The most important work is done in the submission of bills to Congress by an administration when it has a majority in Congress. They get sorted and acted upon and we just screw around with them. Every time Congress tries to initiate legislation, it does a bad job—Smith Act, Taft-Hartley, Landrum-Griffin. Congress legislates in the heat of emotion as it is threatening to do now on campus unrest and did on draft resisters.

If one means by the important work of the House constituent services and the like, which I do, then it is done neither on the floor nor in committee.

Six of the incumbents who asserted the pre-eminence of committee work stated that they had not held that point of view when they were freshmen. A midwestern Republican claimed that his previous state legislative experience had misled him, for he "came from a state Senate where the important work was done on the floor, where the substance of bills was often changed on the floor." This suggests that in some situations previous political experience may be dysfunctional and hence may have to be unlearned. Two representatives said that it was legislative experience in general that led them to recognize the primacy of committee work. Finally, a southern Republican gave a very interesting reply as to his perception of the importance of committees when he was a freshman.

To some extent I felt that committees were most important. It became clear to me early that if you do not have the power, then you have to go to where the power is. It is difficult for a freshman to

do much on the floor, but if he goes to his committee chairman or ranking minority member and has already proven his worth to him, if he makes a suggestion and leaves it at that and does not try to grandstand, he can influence legislation from the outset through the senior member of his committee.

Friendly Relationships. The level of agreement on the importance of friendly relationships was truly impressive. One reason cited for this attitude was instrumental: friendly relationships would presumably make one more effective in getting his ideas accepted and bills passed. The other reason was the vague belief that life in general was more pleasant when friendly relationships were the rule. Some responses exemplifying these reasons were:

Friendly relationships are important because you never know when you'll need others' help or votes. For example, I've been working on a bill on the hijacking of airplanes and got a good reception from friends while testifying at the Interstate and Foreign Commerce committee hearings.

Friendships are extremely important; that's why the gym is so valuable. You meet guys from the other side of the aisle, people you normally would not get intimate with.

But a number of representatives qualified what was meant by friendly relationships:

It depends on what you mean by friendly. In terms of operating as a club, I'm not attracted by the sort of thing in which the gym is the key to friendship or advancement. But if by friendship, one means courtesy and respect, then, sure, it's important.

Friendly relationships are important, but they do not require camaraderie. There's not too much of that that bears directly on getting things passed. One should be a person one feels he can deal with, one who will keep his word. But representatives recognize that one may have to violate this, that one may have to be a demagogue on an issue important to the district. Being buddy-buddy is not as important in the Congress as it is in the state legislature which is less sophisticated.

House Rules. A fairly high consensus was attained on the importance of learning the House rules. The few cases of disagreement, however, are quite interesting. One representative well known to his colleagues for his ambition and career plans beyond the House declared that the rules were not very important to learn. He said that he had not taken the time to learn them, that he worked around the system rather than within it, and that he had not been hurt by this approach so far. It may very well be that members who do not view the House in career

terms will consider the rules to be less significant. This viewpoint was expressed by another representative, who said that the importance of the rules depended on what you wanted to do; if you wanted to get into the leadership, then they were "damned important." Another incumbent thought that the rules were not important for freshmen:

The rules are important, but those used most often are those used in legislative actions. If one is in a position of power, then the rules are important. Most members never come close to knowing the procedures at all, yet they can do a lot. Freshmen need know only the amenities and not the rules.

A northern Democrat thought that the rules were not all that important as the will of the majority usually prevailed; members did not use the rules against you. Another representative voiced a contrasting opinion; he said that the rules were unimportant because they were so often flouted.

Personal Criticism. While the level of agreement on the norm of refraining from personal criticism of fellow representatives was quite high, this item proved to be very difficult to answer for some members. Almost one-third of the responses were conditional or hypothetical: "I don't think I would," "I shouldn't," "It's not likely," "Not unless someone drove me to it." One representative admitted to having engaged in personal attacks, but vowed never to do it again because of the severe social consequences that he had suffered.

Reciprocity/Trading Votes. Most representatives were willing to trade votes, a manifestation of reciprocity, but many attached provisos to this type of action. Typical responses were:

Yes, I'd trade votes, but this does not happen often. It is not a specific trade, but more a matter of good will. I never had a specific trade; this happens more in the Senate.

It depends on the importance of the bill. On local bills, I think I would. For example, I am interested in a potato referendum bill in 48 states and I'm sure that my good friend — of New York couldn't care at all about the bill, but he'll probably support it because we're friends. And I'd do the same for him.

The negative responses to this item were usually spoken sharply and intensely, indicating an almost moral disapproval of this type of action. Unfortunately, the N involved here is only 21.

In summary, friendly relationships and the importance of committee work were the most commonly agreed upon norms. A somewhat lower level of consensus characterized attitudes toward specialization, reciprocity, restraint in personal criticism, and the importance of the House rules. The marked disagreement on apprenticeship calls into question the very existence of this norm.

Freshman Attitudes Toward the Norms at t_1

Table 2 presents the replies of the newcomers to the same norms questions asked of incumbents, as well as to some special freshman items; the nonfreshman figures are repeated as an aid to the reader.

The responses of the newcomers are quite similar to those of their more senior colleagues, suggesting a generally shared set of expectations. Both groups agree most strongly with the importance of friendly relationships; perhaps this implies that this norm, rather than being specific to the House, is carried over from general life experience. The freshmen also unanimously believed the rules to be important. The major difference between the newcomers' and incumbents' responses concerned the norm of apprenticeship; an additional 20 per cent of the freshmen agreed that it is necessary. This may reflect a caution on the part of freshmen early in their incumbency.

Since the concept of norms involves shared

Table 2. Freshman Attitudes to the Norms at t_1

	% Freshmen Agreeing	N	% Nonfreshmen Agreeing	N
Friendly relationships important	100	30	97	40
House rules important	100	30	82	40
Important work of House done in committee	90	30	95	40
Congressman should be a specialist	73	30	80	40
Worthwhile to spend time on House floor	73	30		
Would be likely to trade votes	72	29	81	21
Senior members favor the specialist	71	28		
Would not personally criticize a fellow representative	71	28	82	40
Freshman should serve apprenticeship	57	30	38	65

expectations, the freshmen were queried about their perceptions of nonfreshman attitudes. They were asked whether they thought that more senior representatives favored the specialist or the generalist. All the newcomers who could answer said that senior members favored the specialist, which in effect confirmed their own earlier endorsement of specialization and committee work, but 29 per cent (8 of 28) said that they did not know the preferences of senior members. And while the newcomers generally cited committee work over floor work, fully 73 per cent thought it worthwhile, especially for freshmen, to spend time on the House floor. Attendance on the House floor was deemed important because it enabled the freshman representative to learn the procedures and thereby complete his apprenticeship much sooner.

Deviant Responses of the Freshmen. One can conclude from this brief outline that freshmen shared to a similar degree the norms of non-freshmen, a situation certainly conducive to the learning of norms, but not one that automatically implies that norms were formally transmitted from senior to freshman members. It is interesting to examine some of the deviant responses given by freshmen. Numerous hypotheses were entertained about the causes of noncompliance to the norms. For example, it was thought that freshmen with prior political experience, especially in the state legislature, would be more apt to give "correct" responses. It was also thought that freshmen with a strong dedication to a career in the House would be more sensitive to the norms. These hypotheses and others were neither confirmed nor refuted because of the very small number of cases involved. The most fruitful approach to the problem is a norm-by-norm analysis of the deviant responses.

Specialization. Eight of the thirty freshmen interviewed at t₁ did not unqualifiedly opt to be specialists. Two freshmen asserted that Congressmen should be generalists, five said that it depended on the individual or that a member must be both, and one did not know. One representative who chose to be a generalist cited the heterogeneous nature of his district as the determining factor in his decision. This representative had the most extensive state legislative experience of any freshman in the 91st Congress, thereby leading one to reject the too facile linkage of state legislative service with specialization. The other generalist appeared to be motivated by two forces: a suspicion of accept-

ing others' advice and a recognition that congressmen had to vote on a wide range of issues and should therefore be broadly informed. The theme of distrust occurred throughout this interview; the representative was very much an ideologue with little confidence in the judgment and motives of his colleagues and with a very rigid view of his job. This suspiciousness was reflected in his responses to questions on voting cues: he was very wary of members giving him wrong information and was generally expecting his votes to be uninfluenced by his colleagues. Newcomers who said that a member had to be both a specialist and a generalist argued that assignment to a committee forced one to specialize, while floor voting on a wide variety of issues compelled one to be a generalist. Another representative rejected specialization partially because of the narrowness of his committee assignment. He said:

One has to do both. I can tell you now that I will not spend the rest of my time becoming an expert on the price of hayseed oil.

An analysis of such background variables as prior political experience uncovered no systematic differences between specialists and nonspecialists.

Committee Work. Only three of thirty freshmen did not assert that most of the important work of the House was done in committee. One member, a southern Democrat with state legislative experience and highly directed toward a career in the House, said that he did not know where the work was done since the committees had only just organized. Another freshman, our distrustful ideologue mentioned above, said without qualification that most of the important work was done on the floor, since that was where the actual passage or defeat of a piece of legislation occurred. For him, voting was the crucial aspect of the job with the events preceding any vote being of only minor importance. The third freshman who failed to assert the primacy of committee work thought, in fact, that neither the committees nor the floor was the most significant arena:

The important work is not done in either. It is too early to say where the important work is done, if, in fact, important work is done. The important work is to stir up public opinion about the important issues.

This representative more than any other freshman had an active national constituency. His energies were constantly spread thin over a number of liberal causes that were outside the immediate context of House legislative activity.

He was one of the most active and reformist freshmen, yet his behavior very much followed the traditional, accepted patterns even though he denied the importance of committee and floor work. For example, in the interview, he coupled a stinging attack on the proceedings on the House floor with a statement and explanation as to why it is important to spend time there.

Yes, it is worthwhile to spend time on the House floor. This is where the member lobbies, where he talks to others about bills, trades information, does logrolling. People will think you are arrogant if you don't spend much time there. Debate on the floor is a sham; it's dishonest. People get things in the *Record* without saying them in debate as happened in the HUAC controversy.

And his response to the item about the importance of friendly relationships probably best typifies the difference between the activist who is careful to observe the amenities and the would-be reformer who allows personal relationships to deteriorate.

It's very important to maintain friendly relationships. I think a big mistake is made when people who come up here hoping to change things, are frustrated and allow their frustrations to create personal animosities. I get along with everyone here; I like everyone. I even get along well with Mendel Rivers; I tease him a lot.

This same freshman's remarks on the House floor on the occasion of Speaker McCormack's announced retirement further illustrate proper, yet dissenting behavior.

Mr. Speaker, I want especially to join in this tribute to the Speaker today because it is no secret that on some questions we have disagreed, and it is important—if this occasion is to be what it should be—that those of us who have not always agreed with him make it clear that we are no less grateful for having known him than those who have agreed with him.

. . . I will always be grateful that we overlapped here so I could have the opportunity to know this man, generous, considerate, and fair to everyone, never vindictive no matter how great his disagreement or disappointment.

The rules of this body continue to dismay many of us who find them neither democratic nor conducive to efficient procedures. But within these rules, the Speaker has done everything he could to protect the rights of every Member . . .⁹

⁹ It is very difficult to gather systematic information on the "proper" activist who observes the rules of the game vs. the one who does not, but scattered references to various individuals in the course of interviewing suggested that these two general types were meaningful for some members of Congress. For example, specific comparisons twice were made between the

Personal Criticism. There were eight freshmen out of twenty-eight who said they would personally criticize a fellow representative. For three of these representatives, this willingness to criticize their colleagues seemed to be rooted in a certain outspokenness of personality and intensity of belief that they realized would surface at some point in their congressional careers, although they did not consciously intend to make personal criticism a regular occurrence. Included among these three is our distrustful ideologue; he said that it was possible he would criticize a fellow representative "if he needed it." None of the five other freshmen had served in the state legislature, so perhaps their earlier experiences had not taught them that personal attacks were normally out of bounds. For one of these five, the lack of state legislative experience was probably less influential than the member's own weak attachment to the House. Two factors that lessened his commitment to the House were his age and his relative financial independence: he perceived himself to be too old to have any lengthy career in the House, and therefore far removed from the worries of career advancement; furthermore, as he mentioned more than once, he had a lucrative business to return to if House service ever became too compromising. Even if he were to criticize, he said that he would do it "with velvet gloves."

Reciprocity/Trading Votes. A striking similarity emerges among the freshman representatives who were not likely to trade votes: All six were Republicans. And the three members of the nonfreshman sample who unqualifiedly refused to trade votes were also Republicans.

"proper" liberal described above and another liberal Democratic freshman from the same region and with similar issue concerns. These comparisons were unfavorable toward the latter freshman because of his excessive behavior. He was criticized for shooting off his mouth too much and for making too many entries in the *Congressional Record*. An examination of the actual number of entries indicated that the perceptions of his behavior were accurate: he was the most verbose newcomer with nearly twice as many distinct items in the *Record* as his "proper" freshman colleague. It is noteworthy that such behavior is salient to Congressmen and that reputations, whether good or bad, can be created by such activity. In one of the rare attempts to talk in systematic and quantitative terms about the sanctions invoked against nonconformists, Wayne Swanson found that liberals, and especially anti-establishment liberals, lagged behind in advancing up the committee hierarchy in the Senate. See Wayne R. Swanson, "Committee Assignments and the Non-Conformist Legislator: Democrats in the U.S. Senate," *Midwest Journal of Political Science*, 13 (February, 1969), pp. 84-94.

Four of the freshman responses were resounding "No's," while another was "I hope not," which seemed to imply that vote trading was wrong, but that somehow the Representative might be tempted to participate in it. The only respondent who tried to explain the negative opinion toward vote-trading stated that ten years earlier in the state legislature he had been "burned" because he did not anticipate the consequences of the bill to which he had given his vote. Perhaps what we have here is a tendency by Republicans to view politics more moralistically or ideologically so that trading votes becomes tantamount to catering to unworthy special interests and to abdicating one's own sense of right and wrong.¹⁰

The Usefulness of Time Spent on the House Floor. The final two norms to be investigated for deviant responses were the ones asked only of freshmen. One inquired whether they thought that senior representatives favored the specialist or the generalist. As mentioned earlier, while eight freshmen did not know the answer to this at t_1 , none answered "generalist." The second item was concerned with the value of spending time on the House floor. At t_1 , five representatives (four Democrats and one Republican) said that it was not worthwhile because so little was going on. Since we are interested in behavior as well as in attitudes, a crude measure of time spent on the House floor was constructed; it is simply the percentage of the quorum call votes that the member answered in the first session of the 91st Congress. This measure has severe limitations, including our expectation that members from districts relatively

close to the Capitol would score low because of the extensive amount of time they would spend in their easily-reached constituencies.¹¹ And, obviously, members who do answer quorum calls regularly may spend very little time on the floor. Be that as it may, there was a weak, yet consistent relationship between attitudes and behavior. The 22 freshmen who felt floor-time was worthwhile missed 13 per cent of the quorum calls; the 3 who were neutral missed 19 per cent; and the 5 who felt floor experience was not worthwhile missed 28 per cent.¹²

Forces Contributing to Deviant Responses to the Norms. From this discussion of deviant responses, we can winnow out a number of influences that promote compliance and noncompliance to the norms, but we cannot weight these influences in any quantitative fashion because of the very small N involved. Career orientation to the House appeared to be important in furthering adherence to the norms, while aspirations to other office encouraged nonadherence. Also reducing the salience of the House for the newcomer were financial security and age; older freshmen realized that they would be less likely to advance to any great degree up the House hierarchy. Finally, the extremism of the member's ideological views was related to norm compliance: The more ideological members, whether of the left or right, were somewhat more willing to violate the norms, especially those on speaking out in proper fashion. It was significant that only one representative—the distrustful ideologue—gave responses that repeatedly departed from the norms. Personality traits in combination with his view of politics seemed to account quite well for his beliefs. But for most other freshmen, deviant responses

¹⁰ There are numerous other plausible sources of party differences toward vote trading; unfortunately, we do not have adequate data to check these out. One explanation concerns the recruitment of candidates by each party. It may be that Republicans in general turn to less political types, often businessmen, who may be naive about politics and therefore not recognize the validity and usefulness of such activities as vote-trading, bargaining, compromise, and the like. While four of the six Republican freshmen in this group did not serve in the state legislature, it is interesting to note that four of the six also had as their primary occupation a business-related activity rather than the legal profession which was by far the most common occupation of the 91st class. Another explanation of this possible party difference is the position of the Democrats and Republicans as fairly permanent majority and minority parties in the House. This may mean that the responsibility for the passage of legislation is left more to the majority Democrats who must then engage in the vote trading and log-rolling necessary to forge a majority coalition. It may be that Republicans tend to see this activity as illegitimate since they so often come out on the losing side, but this is purely speculative.

¹¹ Contrary to my expectation, it turned out that members from districts closer to Washington did not have higher absentee rates. But then one might argue that their scores would have been even higher had their districts been more distant from the Capitol.

¹² Overall, Republican freshmen missed 13 per cent of the quorum votes and Democrats 17 per cent. Southern freshmen in each party had the lowest absentee rates on the quorum votes—13 per cent for Democrats and 8 per cent for Republicans. This may be due to the fact that southern freshmen were more oriented toward the House in career terms and were therefore more likely to perform those tasks that promote a legislative career. Although I do not have data on this, there is no reason to believe that southern freshmen returned to their districts less often than their nonsouthern colleagues which then would have accounted for their higher attendance rates. In fact, five of the 11 southern districts were easily within three to five hours of Washington by car. Thus, it appears that there were regional differences in attendance rates which may reflect differing career views of the office of representative.

were very infrequent, so that it was impossible to talk of types of freshmen who generally did not abide by the norms. In other words, non-compliance to one norm did not predict very well attitudes toward other norms. Probably the most important finding overall is that non-compliance was minimal, implying that by one means or another, freshmen had learned the norms well, the topic to which we now turn.

Freshmen and the Learning of Norms

As already mentioned, and in contradiction to initial expectations, most of the freshmen at t_1 were giving "correct" responses to the norms items. This observation has implications for the amount of learning that the panel can uncover. If learning be defined in terms of attitude change, then we would expect little learning from t_1 to t_2 because of the correctness of the t_1 responses. While learning might come in the form of strengthening initial attitudes, the inappropriateness of Likert items for elite populations such as legislators makes it difficult to get a handle on changes in attitudinal intensity. As it was, the responses to the unstructured norms questions were coded to incorporate direction and intensity of attitudes, and changes in both are reflected in the coefficients presented in Table 3.

The low incidence of later norm learning may mean that the freshmen knew the norms before entering Congress. If this be true, it is an interesting datum in and of itself. Or perhaps freshmen learned the norms in the short interval between taking office and being interviewed by this investigator, although this possibility appears unlikely (see fn. 4). One way of demonstrating the minimal learning of norms from t_1 to t_2 is to cross-tabulate the t_1 and t_2 responses to each item and examine the intra-item correlations. This is done in Table 3 for those norms questions that were coded in an ordinal fashion with at least four valid codes; the measure of association is tau-b.

Table 3. Intra-Item Correlations on the Norms Question (t_1 and t_2)

	Tau-B	N
Friendly relationships important	1.000	24
Would be likely to trade votes	.721	20
Freshman should serve apprenticeship	.696	24
House rules important	.693	24
Would not personally criticize a fellow representative	.310	22
Worthwhile to spend time on House floor	.169	24

As one can observe, all of the items except two—personal criticism and spending time on the House floor—were very stable. Of the 22 representatives who responded to the personal criticism question at t_1 and t_2 , 13 did not give identical answers at the two time points. Eight of the 13 gave answers at t_2 that made it less likely that they would engage in personal criticism, but the other five indicated a greater willingness to criticize which runs counter to the learning or reinforcement of "proper" behavior. There were no background variables that explained the changes on this item, nor did the freshmen volunteer any information that was helpful in accounting for the shifts. What we may have here is response unreliability elicited by a question whose wording left in doubt just what was meant by personal criticism.

The situation was very different on the other unstable item; here half of the 24 responses were identical from t_1 to t_2 . Four of the six Republicans who changed thought it more worthwhile at t_2 to spend time on the House floor, while five of the six Democrats felt just the opposite, and each group was able to justify its own position. The Democrats were all active liberals dismayed by what they considered the scarcity of significant floor work. Republicans, however, generally cited the instructional value of being on the House floor. Thus, a part of the reason for the low intra-correlation on this item was the growing dissatisfaction on the part of some Democrats. As this discontent was basically programmatic, it did not affect the less issue-oriented Republicans, whose changes on this item in the opposite direction were less easily explained.¹³

Subdividing the freshmen according to characteristics such as party and district competitiveness does not change the tau-b's very much. There is, however, some tendency for freshmen with state legislative experience to exhibit greater stability than those without such experience, and this is especially pronounced on vote trading. While the correlation for this item for all freshmen was .721, it rose to .792 for newcomers who had had state legislative service and plummeted to -.200 for those who had not had such service. Attitudes on vote trading, therefore, appear to be more stable if one has already had the opportunity to confront the situation in reality (as in a state legislature) and

¹³ The issue orientation of the freshmen was determined by impressionistically content-analyzing their responses to a series of questions dealing with their legislative goals, the specific pieces of legislation, if any, that they planned to introduce or work for, and the kinds of legislative matters about which their districts were most concerned.

Table 4. Level of Agreement to the Norms at t_1 and t_2

	Freshmen				Nonfreshmen	
	% Agreeing t_1	% Agreeing t_2	Change	N	% Agreeing	N
Freshman should serve apprenticeship	58	42	-16	24	38	65
Congressman should be a specialist	83	70	-13	23	80	40
House rules important	100	88	-12	24	82	40
Important work done in committee	96	87	-9	23	95	40
Friendly relationships important	100	92	-8	24	97	40
Would be likely to trade votes	68	68	0	22	81	21
Worthwhile to spend time on House floor	71	75	4	24		
Senior members favor specialist	70	78	8	23		
Would not personally criticize a fellow representative	69	82	13	22	82	40

not just hypothetically. That is, members who had experienced vote trading first-hand were more consistent in their attitudes toward it, an intuitively appealing result.

Finally, let us examine the percentage agreement with each of the norms at t_1 and t_2 , looking only at freshmen interviewed at both t_1 and t_2 . This will indicate whether support for the norms increases or decreases (erodes) over time. The appropriate figures are presented in Table 4.¹⁴

The striking point about Table 4 is that support for the norms more commonly decreased than increased, although most of the percentage changes were small and probably mean little. The norm that suffered the greatest and most real erosion was apprenticeship; this finding will be analyzed in depth in the next section. Also suffering erosion of more than 10 per cent were the items on specialization and the importance of the rules. What may be happening here is that at t_1 freshmen knew the "right" responses and uttered them automatically but that subsequent legislative experience enabled them to take a more sophisticated, knowledgeable, and qualified view of the norms. The norm showing the greatest gain in support was restraint in personal criticism, but no ready explanation comes to mind except that attendance on the House floor would indicate to the freshman, if he did not already know, that personal attacks were very rare, indeed.

¹⁴ The percentages of Table 4 are for freshmen in the aggregate. Thus, zero per cent change does not necessarily mean that all the freshmen interviewed at t_1 and t_2 remained perfectly stable; shifts in one direction may have balanced out those in the other. As it was, there was general stability in the replies except for the two items discussed in the text (personal criticism and spending time on the House floor) so that the percentage differences in Table 4 do not mask much additional shifting.

The other interesting result in Table 4 is that the t_2 freshman responses are somewhat closer to the nonfreshman replies than are the t_1 answers. This does not hold for specialization, committee work, and friendly relationships; here the t_1 answers come closer to the incumbent responses, but the differences involved are small. But for the other norms, the t_2 freshman responses are substantially closer than the t_1 responses to the nonfreshman replies. For apprenticeship, the difference between the t_1 and nonfreshman responses was 20 per cent, but only four per cent when the t_2 replies are used. Eight and 13 per cent differences between the t_1 and nonfreshman replies on House rules and personal criticism drop to six per cent and zero when the t_2 responses are substituted. Thus, the freshmen were basically similar to their more senior colleagues at t_1 , and by t_2 the coincidence of views toward the norms was even closer, suggesting that informal socialization to the norms was still occurring at t_2 .

The Norm of Apprenticeship

It is the norm of apprenticeship that is most relevant to freshmen. If the traditional descrip-

Table 5. The Norm of Apprenticeship

	t_1 Freshman Responses	Nonfreshman Responses
	%	%
Agreement	40	24
Qualified Agreement	17	12
Qualified Disagreement	17	20
Disagreement	26	44
Total %	100	100
N	(30)	(66)

tion of the freshman representative as unsure in his actions and ignorant of House rules and procedures is correct, then apprenticeship is obviously the crucial norm for newcomers to follow. The natural expectation is that apprenticeship would be a very commonly accepted norm given the widespread familiarity of the adage about freshmen being seen and not heard. But as Table 5 indicates, the very existence of a norm of apprenticeship as defined herein must be called into question.¹⁵ Almost half of the nonfreshman sample flatly denied the necessity of serving an apprenticeship, and if the qualified disagreements are added to this figure, almost two-thirds of the incumbents rejected apprenticeship. And while there was majority agreement to the norm at t_1 on the part of freshmen, a sizable minority of 43 per cent deemed it unnecessary. Those freshmen who saw the need for serving an apprenticeship were asked how long they felt it would take in their own particular cases. Four newcomers said that they could not set a specific time limit, two said a year, and only one said the full two years of his first term in office. The remaining ten all indicated that apprenticeships between two and six months would be desirable. Thus, even for freshmen who agreed to apprenticeship, the learning period was usually seen as relatively short, most often under six months. The range of freshman responses to the apprenticeship item is illustrated by the following replies:

Yes, I'll serve an apprenticeship. Its length depends upon the individual; it will probably take me less time because of my previous legislative experience. A newcomer needs to be informed to be effective.

Yes. The length depends, probably a few months. You can't lead the army the first day. But you do

¹⁵ As a reminder to the reader, the actual apprenticeship question was: Do you think that freshman Congressmen should serve a period of apprenticeship, that is, be more an observer than an active participant in the legislative process? In the subsequent discussion, apprenticeship will be treated as a dichotomous variable by collapsing the agreement and qualified agreement categories and by combining the disagreement and qualified disagreement categories.

Table 6. Freshman Apprenticeship Responses by Party

	Repub- licans	Demo- crats
"Apprenticeship Necessary"	73	40
"No Apprenticeship Necessary"	27	60
Total %	100	100
N	(15)	(15)

represent a district that elected you for two years and you can't just sit and do nothing.

There is no reason for an apprenticeship. You learn best by jumping right in.

No, you don't have to serve an apprenticeship. But one should not just jump into things. A freshman Congressman is not like a freshman in college; he is a man of some special competence. Senior people will listen to you if you have something to say.

An examination of the apprenticeship replies by party indicates some party differences, as shown in Table 6. Seven of the nine freshman Democrats who rejected apprenticeship were urban liberals from districts with heavy constituent demands. These men were generally active and concerned with programs, and, for them, apprenticeship implied a severe restriction on the activities that they deemed most important, particularly speaking up on the floor on such issues as Vietnam, national priorities, and school desegregation. The intention to engage in specific legislative activity, shared by a number of Democratic freshmen, itself runs counter to the image of the bewildered newcomer. For most Republicans, the idea of an apprenticeship or limited participation was not as restrictive, mainly because they were not as concerned with programs and problems. Thus, one's view of the job of the representative appears to influence one's opinions about apprenticeship.

It is reasonable therefore that freshmen who agreed to the importance of apprenticeship would be less active. Table 7 presents some evidence on this point using as measures of activity three simple indices constructed from the *Congressional Record*.¹⁶ As expected, Democratic nonapprentices were substantially more active than their apprentice colleagues across all three measures. But this pattern does not hold for Republicans; here the differences between apprentices and nonapprentices are small and inconsistent. Interestingly, Republican and Democratic apprentices had comparable levels of activity, but Democratic nonapprentices were far more active than their GOP

¹⁶ These measures are merely counts of the total number of entries in the *Record* and the total number of nonrelief bills and resolutions introduced. They could be refined by grouping remarks in the *Record* according to issue area or by dividing bills according to their scope of impact. But the expenditure of resources in such an endeavor seemed prohibitive. As sponsorship of bills is so prevalent in the House, the number of bills introduced may be a highly misleading measure of legislative activity. A better indicator might be the number of amendments introduced. But since amendments from freshmen are relatively rare (only about one-fourth sponsored any), they cannot provide us with sufficient information.

Table 7. Levels of Activity vs. Apprenticeship by Party

	Republicans		Democrats	
	App.	No App.	App.	No App.
Mean Number of House Remarks	22	17	22	39
Mean Number of Extension Remarks	15	19	21	40
Mean Number of Nonprivate Bills and Resolutions Introduced	54	53	56	81
N	11	4	6	9

counterparts. Thus, Democratic freshmen are less likely to welcome serving an apprenticeship than Republican newcomers, and only for Democrats is the decision to choose or reject an apprenticeship reflected in varying levels of activity.

To help explain this finding one must recall that the Democratic party is the entrenched majority party in the House. The decision to serve an apprenticeship may be more salient for majority party members in general, since it is the majority party that sets the legislative pace, controls the committee, and the like. But more consequential for the freshman Democrat is the domination of his party by senior members, many of them Southerners, who comprise a much larger proportion of his party than senior Republicans do of the GOP.¹⁷ The common perception of the Democratic party is that it is run by its elderly patriarchs. Hence, in making a decision about apprenticeship, the Democratic freshman is choosing a course of action that does have immediate consequences for his legislative career. He is deciding in effect whether "to go along" as Sam Rayburn used to advise freshmen or to strike out on a

more hazardous path. And the freshman Democrat is often forced to make this conscious decision about apprenticeship because he may enter Congress with strong intentions to promote a wide range of legislative activities and at the same time realize that his position as a freshman member in a senior-dominated party works against such conduct. The question of an apprenticeship is not nearly as salient for Republican freshmen—members of a relatively junior, minority party, who early in their House careers had the vaguest legislative plans, a condition that would make serving an apprenticeship less onerous.

In addition to one's programmatic intentions, the other variable that was often cited by freshmen as influential in their apprenticeship decision was their perception of their own legislative competence. Three members specifically stated that because of their previous experience in the state legislature, their period of apprenticeship could either be shortened or be totally unnecessary. The relationship between state legislative service and attitudes toward apprenticeship is presented in Table 8. For all freshmen, service in the state legislature does not materially affect opinions about apprenticeship. Table 8 further indicates that party differences with respect to apprenticeship remain even when previous state legislative experience is considered, thereby suggesting that previous experience is less important than one's own legislative goals in the decision whether or not to serve an apprenticeship. Again, Democratic

¹⁷ At the start of the 91st Congress, 21 per cent of the 243 Democrats had served more than 20 years (ten terms) in the House as compared to under six per cent of the 192 Republicans. Or to restate the figures, the Republicans had only eleven members with more than 20 years of service, while the Democrats had 51 such members. Furthermore, 46.4 per cent of the Republicans had served three or fewer terms as opposed to 31.7 per cent of the Democrats.

Table 8. Apprenticeship vs. State Legislative Service and Party

	State Service			No State Service		
	R	D	All	R	D	All
"Apprenticeship Necessary"	70	43	59	80	38	54
"No Apprenticeship Necessary"	30	57	41	20	62	46
Total %	100	100	100	100	100	100
N	(10)	(7)	(17)	(5)	(8)	(13)

freshmen were more oriented to legislation than their GOP colleagues.

The nonfreshman responses to apprenticeship are truly surprising in the extent of their disagreement with the norm. The common political lore has it that nonfreshmen, particularly the more senior among them, would be the strongest advocates of an apprenticeship norm. The nonfreshmen responses to the apprenticeship item were usually brief and to the point, indicating little difficulty in replying to the item. A few of the more detailed, qualified responses are given below:

Apprenticeship is necessary for a period of time, but I have encouraged them [freshmen] to participate as soon as possible. I suggest that they choose as their area of expertise an area that they've had experience in previously which will let them participate earlier.

I don't think freshman congressmen should serve a period of apprenticeship. As a matter of fact, I don't think that term can apply in Congress. Any member has an equal right with any other member. The very nature of the legislative process does require that new members take more time to become acquainted with committee activities than those who have served for many years.

This is a difficult question to answer because of the varying backgrounds of the new members of Congress . . . Hence, it seems to me that it would be difficult to establish any rule of thumb . . . I am frank to say that I have observed instances of over-enthusiastic freshman congressmen who would have done well to have been a bit more observant of the processes and legislative procedure before offering the panacea to a particular problem.

Table 9 presents the nonfreshman replies to the apprenticeship item by seniority and party.

The results in this table are somewhat unexpected. Overall, the more senior one is, the less likely he will say that apprenticeship is necessary. For all groups except Republicans with less than six years of service, a sizable majority of respondents were against apprenticeship, and even for this group of Republicans the division was almost even. The reader should have greater confidence in the Republican figures

than in the Democratic ones which do not include sufficient senior representation, especially from the South and border states.

Table 9 seems surprising because we have been led to believe that it is senior members who keep junior men in their place. But perhaps only a subset of senior members perform this function. The probable candidates would be the senior southern and border (Democratic) members and others who also dominate the committees. After all, it was Texan Sam Rayburn who was credited with the terse description of apprenticeship as "being seen and not heard" and who advised members to go along in order to get along. There are only four southern and border Democrats in the sample with more than ten years of service in the House (in a representative sample, there would be about twice that number), and three of the four said that no apprenticeship was necessary. These figures are too small to be conclusive, especially since it is almost impossible to interview the real patriarchs of the House. We can, however, divide the sample into gross regional categories to see whether apprenticeship is more common in the South, the likely home of a disproportionate number of carriers of the creed. The results of such a division reveal no substantial regional differences; 4 of 12 southern members said that apprenticeship was necessary as compared to 38 per cent of the non-southern members ($N = 54$). This conclusion must be hedged a bit because of shortcomings in the southern subsample.

Thus, apprenticeship is far from being a universally accepted norm. Indeed, one wonders why a majority of the freshmen at t_1 still subscribed to the norm, unless it represents a false anticipation. As one newcomer observed, "It may be that freshman classes have been browbeaten before they ever got here by the establishment." The browbeating may very well take place through the widespread circulation of such political lore as Speaker Rayburn's advice cited earlier. Such information about their status may be the only kind available to freshmen at first. Thus, in a sense, freshmen may have to

Table 9. Nonfreshman Apprenticeship Responses by Party and Seniority

	6 or fewer years of service			7 to 10 years of service			More than 10 years of service		
	R	D	All	R	D	All	R	D	All
Apprenticeship	53	27	41	43	13	27	36	25	32
No Apprenticeship	47	73	59	57	87	73	64	75	68
Total %	100	100	100	100	100	100	100	100	100
N	(17)	(15)	(32)	(7)	(8)	(15)	(11)	(8)	(19)

be socialized *out of* the norm of apprenticeship. Two freshmen at t_1 said that Speaker McCormack himself had urged them to participate fully right from the outset of their service. Yet because of their prior conditioning, they were leery of such advice; they thought the Speaker was "just being nice." As noted earlier, there was erosion in support for apprenticeship from t_1 to t_2 by the 91st class; perhaps this is indicative of learning that is really unlearning. Freshmen may have seen some of their colleagues participate early, observed that no sanctions were levied, and therefore altered their views about apprenticeship. Of course, it is possible that senior members who deny the importance of apprenticeship are saying one thing and believing another. A sophomore Republican argued:

... [M] any senior congressmen might say that a freshman should not serve an apprenticeship, but when it came to actual practice, the situation was quite different. I know of numerous instances of senior members grumbling when a freshman member spoke on a subject. Typical comments were: "What's he doing talking, he's a freshman," "What does he know about it, he's only a freshman."

But it seems unreasonable to consider the senior members' responses against apprenticeship as largely misstatements of the members' underlying beliefs.

This argument does not mean that apprenticeship is unnecessary or unexpected. There will be many topics about which the freshman will be uninformed because of his inexperience, and in such areas, his more senior colleagues will expect him to proceed cautiously and to learn gradually. But this type of apprenticeship is a far cry from one in which the newcomer is expected to remain silent in all situations, even when he has a contribution to make. Some years earlier, Charles Clapp came to a similar conclusion, although his evidence was incomplete since his roundtable participants tended to be disproportionately liberal, reformist, and receptive to academicians. At that time, Clapp wrote:

The old admonition that new members should observe but not participate in debate was swept aside long ago. Apprenticeship may still precede full partnership, but the increased volume and complexity of the problems with which the Congress is compelled to cope dictate more efficient use of the membership. Freshmen are now advised to defer speaking only until the moment arrives when they have something significant to say—indeed, colleagues counsel them not to wait too long—although they

are cautioned to be sure they are well informed about their topic.¹⁸

Finally, it is clear that the House is not alone in its skepticism about apprenticeship; the norm of apprenticeship has fallen into bad times in the United States Senate as well, especially since 1964. The "Inner-Club" has declined in recent years and a new type of Senator, less concerned with internal Senate operations, has become more prominent.¹⁹

Conclusion

The main finding of this paper is that the amount of norm learning between January and May by the freshman members of the 91st Congress was unexpectedly low. As the concept of norms incorporates the notion of shared expectations, the attitudes of a sample of non-freshmen were first analyzed to insure that we were studying genuine norms. Apprenticeship was found to be less restrictive on freshmen than originally thought, while other norms were largely adhered to. It appeared that freshmen largely knew the general House norms prior to entering Congress, which made it impossible to talk about the formal agents of socialization involved in transmitting the norms to newcomers. And the extent of change once in office was minimal.

Now this finding may be an artifact of the particular freshman class under investigation, in the sense that the 91st class was unusually well-prepared by prior political experience for House service. Of the six most recent freshman classes, the 91st class had the largest proportion of members with state legislative experience—51 per cent. But an alternative explanation seems more satisfactory, that is that freshman representatives would generally know many of the norms simply because they are rules of behavior appropriate to many institutional settings. Thus, one does not have to be a member of Congress to know that personal criticism and unfriendly relationships may be dysfunctional to one's institution or group or one's own career. This argument asserts that almost any type of prior experience would make the freshman sensitive to the basic rules of behavior. Overall, it was difficult to link compliance and noncompliance to the norms to any particular characteristics because of the small number of

¹⁸ Clapp, *The Congressman*, pp. 12–13.

¹⁹ See Randall B. Ripley, *Power in the Senate* (New York: St. Martin's Press, 1969), p. 185 and Nelson W. Polsby, "Goodbye to the Inner Club," in *Congressional Behavior*, ed. Nelson W. Polsby (New York: Random House, 1971) pp. 105–10.

deviant responses, but a number of plausible influences were suggested.

These data do not address the learning of committee-specific norms. These norms may not be as salient or as transferable from other contexts as the ones discussed above, and hence they may actually have to be learned anew by freshmen.²⁰ Unfortunately, the data in this article span only January to May, 1969, a time of very little legislative action, both in committee and on the floor. The 91st Congress in its early months was sharply criticized for the slowness of its legislative pace, a slowness that was due in part to the change in partisan complexion of the national administration. And this slowness

of legislative pace may have retarded the learning of committee-specific norms. In their California study, Price and Bell found that the norms cited at their later interviews were most often those that concerned committee work and the handling of legislation.²¹

In summary, then, it seems as if the traditional image of the freshman congressman as ignorant and bewildered had mistakenly led us to expect substantial learning of norms on the part of supposedly ill-informed newcomers. This expectation was unwarranted on two counts: the general House norms were not so abstruse as to require formal learning, and the traditional image of the freshman Congressman was found to be out of date, a theme to be developed in a future paper.

²⁰ A good example of such research is Fenno's treatment of apprenticeship in the context of the House Appropriations Committee. See *The Power of the Purse*, pp. 166-67.

²¹ Price and Bell, "The Rules of the Game," p. 855.

The Strange Case of Relative Gratification and Potential for Political Violence: The V-Curve Hypothesis*

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In the United States, in addition to an increase in the level of individual violence against persons, the 1960s also saw an increase in group actions involving coercion and violence directed implicitly or explicitly toward political ends. Scholars have sought to explain attitude toward, and participation in, political protest and civil unrest in various ways. One explanation offered by a number of prominent scholars has utilized the concept of discrepancy between a person's goals—or desired level of achievement—and his actual level of achievement: the greater the achievement discrepancy, the greater the potential for political violence. This Achievement Discrepancy (AD) hypothesis has been labeled variously, "relative deprivation," "systemic frustration," "J-curve," and "rising expectations" and it is with this explanation that the present paper will be concerned.

Our AD constructs will be defined and differentiated from other variants in the first section of the paper. These constructs have been built from measures of individuals' perceptions of degree of discrepancy between their own best possible achievement level and their actual achievement level with respect to various welfare values. In the second section we report tests of hypotheses about relationships between these AD constructs and potential for political violence. Here we report a finding that was unexpected (but not just specific to this sample), namely, that potential for political violence does not vary monotonically with direction and rate of change in discrepancy between achievement optimum and achievement; rather, the greatest potential for political violence is manifested both by individuals who perceive negative change (increasing discrepancy) and by individuals who perceive positive change

(decreasing discrepancy), while those who perceive no change manifest the least potential for political violence. In attempting to explicate this finding through the introduction of various control variables, we find that a nonmonotonic "V" relationship is remarkably persistent. Finally, we show that the *absolute magnitude* of change in perceived discrepancy over time is a variable that warrants inclusion in a linear additive model of potential for political violence.¹

Our data come from a survey of the residents of Waterloo, Iowa, a city with a population of approximately 78,000, located some 200 miles west of Chicago. Waterloo is like many other urban communities in the United States. It developed as a manufacturing center linked to Chicago by the Illinois Central Railroad. Now, although the two major manufacturing plants are still in operation, the inner city includes decaying commercial structures and a black ghetto, while the lily-white residential areas that surround the inner city are serviced by large shopping centers in the standard "airport-modern" architectural style. The Report of the National Advisory Commission on Civil Disorders classified Waterloo as having experienced "serious" disorders in 1967 and on Jules Wanderer's Guttman scale of riot severity² Waterloo ranked at the second highest level. Thus, Waterloo was considered an appropriate research site for the testing of explanations of allegiance and opposition to the political regime.

The sample we shall analyze here is a disproportionately stratified (by race) cluster sample of Waterloo residents 18 years of age and older.³ It consists of 503 respondents to an in-

¹ See Edward N. Muller, "A Test of a Partial Theory of Potential for Political Violence," *American Political Science Review*, 66 (September, 1972), 928-959.

² See Table 2 at page 504 of Jules J. Wanderer, "An Index of Riot Severity and Some Correlates," *American Journal of Sociology*, 74 (March, 1969), 500-505.

³ The Survey Division of the Statistical Laboratory at Iowa State University drew the sample and conducted the interviewing between March and May 1970. It is a disproportionately stratified, multistage, random probability sample. On the basis of an update to the 1960 Census conducted by the Waterloo

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interview schedule of approximately 80 minutes duration, administered during the spring of 1970 by the Survey Division of the Statistical Laboratory at Iowa State University.

The Achievement Discrepancy Explanation

Proponents of the AD explanation of potential for political violence have been careless on two counts. First, in the AD formulation the achieved state represents an individual's present (or anticipated) actual level of attainment with respect to magnitudes of a desired value; but the optimum state has no unitary meaning. Achievement optima, or goal-levels, have been defined in various ways—e.g., "just deserts," "aspirations," "social expectations," "expected need satisfaction"—which give rise to a series of not necessarily congruent hypotheses; yet scholars commonly have failed to differentiate clearly between the different AD terms in these hypotheses. Second, and perhaps of greater consequence for the present status of the AD explanation, tests of AD hypotheses often have entailed a disparity between verbal and operational definitions of AD terms. Regardless of the particular achievement optimum used in the definition of discrepancy, the key point to be grasped is that the discrepancy is between *perceived* achievement optima and achievement, a discrepancy which in no way necessarily coincides with a person's objective sociological circumstances. Unfortunately, lacking appropriate data, many scholars have proceeded from a verbal definition in terms of perceived discrepancy between some achievement optimum and achievement to an operational definition in terms of objective social, economic, and political conditions; thus, objective sociological circumstances are let in by the back door, resulting in measurement of something other than the AD concept as verbally defined and a test of something other than an AD hypothesis.

It is important, then, that tests of AD hypotheses clearly specify which variant of the AD concept is under investigation. And from the standpoint of building empirically verified theory it is particularly important that such tests include measures of perceived discrepancy at the level of individual behavior; for a claim to have confirmed or disconfirmed an AD hypothesis is hardly warranted if the purported

measure of the AD concept actually refers to objective sociological circumstances.

Also, it is important to distinguish between static and dynamic versions of the AD concept. Static versions refer to perception of discrepancy between achievement optima and achievement at a single point in time; dynamic versions refer to change over time in perception of discrepancy. Previously, one of us has built measures of the AD concept that are explicitly differentiated by this distinction.⁴ As we shall show in this paper, the form of the relationship between potential for political violence and AD constructs may differ substantially, depending upon whether the AD construct is static or dynamic.

Achievement Discrepancy Hypotheses. Ted Gurr has labeled his version of the AD concept Relative Deprivation, "defined as actors' perception of discrepancy between their value expectations and their value capabilities. Value expectations are the goods and conditions of life to which people believe they are rightfully entitled. Value capabilities are the goods and conditions they think they are capable of getting and keeping."⁵ Gurr equates value expectations with individuals' conceptions of their *just deserts*: "value expectations are defined with reference to *justifiable* value positions, meaning what men believe they are entitled to get or maintain, not merely what they faintly hope to attain."⁶ Gurr distinguishes between present and future perception of discrepancy between just deserts and capability, and argues that anticipated discrepancy in the future may be a more important source of discontent than perception of present discrepancy.⁷ We may derive three Just Deserts hypotheses from the work of Gurr:⁸

JD₁: Potential for political violence will vary strongly and directly with degree of discrepancy between an individual's estimate of his present achievement level and the level of achievement which he thinks he justifiably deserves.

⁴These are Short-Term Welfare Gratification and Long-Term Welfare Gratification. See the appendix in Muller, "A Test of a Partial Theory of Potential for Political Violence," pp. 955-959.

⁵Ted Robert Gurr, *Why Men Rebel* (Princeton: Princeton University Press, 1970), p. 24.

⁶Gurr, *Why Men Rebel*, p. 27.

⁷See the discussion at pages 27 and 28 of Gurr, *Why Men Rebel*.

⁸See pages 24 to 28 of Gurr, *Why Men Rebel*. Here Gurr hypothesizes about relationships between relative deprivation and potential for collective violence. Potential for political violence is a subset of potential for collective violence (see page 158).

City Planning Commission in 1967, the area of the city containing most black residents was sampled at a higher rate than the area containing most white residents. Thus, while blacks make up only 8 per cent of the Waterloo population, they constitute 38 per cent of this sample. Within each stratum a response rate of 80 per cent completions was obtained.

JD₂: Potential for political violence will vary strongly and directly with degree of discrepancy between the level of achievement which an individual expects to maintain in the future and the level of achievement which he thinks he will justifiably deserve.

JD₃: Potential for political violence will vary more strongly and directly with perception of future Just Deserts discrepancy than with perception of present Just Deserts discrepancy. (Where future and present Just Deserts discrepancy are as defined in *JD₂* and *JD₁* above.)

The AD concept proposed by Ivo K. Feierabend, Rosalind L. Feierabend, and Betty A. Nesvold is labeled Systemic Frustration. Systemic Frustration differs from Relative Deprivation in that the achievement optimum of the former is conceived as individuals' *aspirations* rather than individuals' perceptions of just deserts. Feierabend et al. state that by aspirations they mean "the goals that people wish to attain."⁹ We may derive the following two static Aspiration Level hypotheses from the work of Feierabend, Feierabend, and Nesvold:¹⁰

AL₁: Potential for political violence will vary strongly and directly with degree of discrepancy between an individual's estimate of his present achievement level and the level of achievement to which he aspires.

AL₂: Potential for political violence will vary strongly and directly with degree of discrepancy between the level of achievement which an individual expects to maintain in the future and the level of achievement to which he aspires in the future.

Still another version of the AD concept is couched neither in terms of aspiration level nor of just deserts, but in terms of reference group theory. Here an individual's achievement optimum is the level of achievement which he believes has been attained by other individuals with whom he identifies. From the generalized formulation given by Anthony M. Orum and Amy Orum, we may derive the following hypotheses:¹¹

⁹ Ivo K. Feierabend, Rosalind L. Feierabend, and Betty A. Nesvold. "Social Change and Political Violence: Cross-National Patterns," in *Violence in America: Historical and Comparative Perspectives*, ed. Hugh Davis Graham and Ted Robert Gurr (New York: Signet Books, 1969), p. 609.

¹⁰ See hypotheses (1) and (2) at page 609 of "Social Change and Political Violence: Cross-National Patterns."

¹¹ See the discussion and literature cited therein at pages 522 and 523 of Anthony M. Orum and Amy Orum, "The Class and Status Bases of Negro Student Protest," *Social Science Quarterly*, 49 (December, 1968), 521-533. What we call the "reference

RG₁: Potential for political violence will vary strongly and directly with degree of discrepancy between an individual's estimate of his present achievement level and the level of achievement which he ascribes to individuals with whom he identifies.

RG₂: Potential for political violence will vary strongly and directly with degree of discrepancy between the level of achievement which an individual expects to maintain in the future and the level of achievement which he expects individuals with whom he identifies to maintain in the future.

We believe that these three sets of propositions ought to be regarded as separate theories. Any or all of the following might be hypothesized: (1) an individual's aspiration is determined by his perception of the achievement of individuals with whom he identifies; (2) an individual's aspiration is for that level of achievement which he thinks he deserves; (3) an individual's perception of his just deserts is determined by his perception of the achievement of individuals with whom he identifies. But, all such hypotheses are empirical and not tautological.¹² Moreover, we should note that all three sets of propositions are couched in terms of *individual perceptions*, and we must be careful not to take for granted that these perceptions correspond with the scholar's own notion of "objective" reality or "objective" deprivation.¹³

A number of ambiguities and difficulties in

group" version of the AD concept Orum and Orum label the "relative deprivation" perspective. Since the reference group hypothesis formulated by the Orums is devoid of explicit tense references, we have reformulated it in two parts.

¹² For example, we cannot just take it for granted that because an individual's aspirations are not met he is not achieving what he perceives as his just deserts. Cf. Gurr's statement that "people become most intensely discontented when they cannot get what they think they deserve, not just what they want in an ideal sense . . ." ("A Comparative Study of Civil Strife," in *Violence in America*, ed. Graham and Gurr p. 568).

¹³ This particular warning has been made by most proponents of the achievement discrepancy hypothesis. Compare the statement by Gurr at page 568 of "A Comparative Study of Civil Strife": "Relative deprivation is not whatever the outside observer thinks people ought to be dissatisfied with. It is a state of mind. . . ." See also the statement by Feierabend et al. at page 609 of "Social Change and Political Violence: Cross-National Patterns," "It should also be pointed out that it is perceived rather than actual social attainment that is important." Finally compare the statement by James C. Davies at page 6 of "Toward a Theory of Revolution," *American Sociological Review*, 27 (February, 1962), 5-19: "It is the dissatisfied state of mind rather than the tangible provision of 'adequate' or 'inadequate' supplies of food, equality, or liberty which produces the revolution."

operationalizing variables arise when we seek to test any of these propositions. Gurr's own major tests of his hypotheses, as he is clearly aware, are extremely indirect and involve aggregate data at a national level. For example, Gurr assumed that "any short-term decline in economic conditions and any governmental policies that restricted political activities or reduced people's socioeconomic status increased deprivation"¹⁴ and he then related Short-term Deprivation (so defined) to levels of civil strife. But, as we can see, it is far from obvious whether what was measured was deprivation vis-à-vis perceived just deserts (as Gurr himself seems to think) or deprivation vis-à-vis aspirations, or deprivation vis-à-vis the perceived status of appropriate reference groups, or, for that matter, none of these. The latter possibility is by far the likeliest, since the measures used by Gurr reflect "objective" circumstances rather than the perceptions of discrepancy between achievement optimum and achievement with which all three of the above versions of deprivation are, in fact, concerned.

The Just Deserts version of the AD concept lacks clear-cut operational meaning for "just deserts" at the micro level. Gurr himself has suggested that the top of a Cantril Self-Anchoring scale represents an individual's perception of his just deserts, and Gurr uses data derived from Self-Anchoring scale responses to test his relative deprivation hypothesis.¹⁵ But the top of a Cantril scale, defined by an individual's perception of his *best possible* achievement level, is not *identical* with a "just deserts" position. While an individual's perception of discrepancy between his best possible achievement level and his actual achievement may show a strong empirical correlation with his perception of discrepancy between that achievement level to which he believes he is justifiably entitled, they are, nevertheless, analytically distinct achievement optima, and there is no a priori reason to assume that they would yield discrepancy estimates with a correlation near unity.

The work done by Feierabend et al. also draws on aggregate data and again uses very indirect measures of their Aspiration Level version of the AD concept, by inferring deprivation vis-à-vis aspiration level from a measure of national modernity which "combines GNP per capita, caloric intake, telephones, physicians, newspapers, radios, literacy, and urbanization."¹⁶ Clearly, it is rather far-fetched to postu-

late that these standard indices of socioeconomic development are *measures* of perceived discrepancy between aspiration level and achievement. As with Gurr, the data on which the Feierabend et al. hypotheses are tested involve indices of objective conditions, such as social and economic development, which need not correspond at all with subjectively perceived deprivation.

An additional ambiguity in the Aspiration Level version of the AD concept is the lack of precision in the definition of an individual's aspiration level. Feierabend et al., as we previously noted, define an individual's aspiration level as the goals that he wishes to attain. But, since the term "goal" is left undefined, it is difficult to know what meaning to assign to aspiration level, for the term "aspiration level," as it is used in the psychological literature, has a plethora of meanings, *each of which can be used to formulate a quite different "aspiration" version of the Gap concept.*¹⁷

Definition 1: An individual's aspiration level is the utility of that achievement (among those which he can imagine himself attaining) with the highest expected utility.¹⁸

Definition 2: An individual's aspiration level is the utility of that achievement (among those which he can imagine himself attaining) which separates satisfactory from unsatisfactory outcomes, i.e., that achievement whose attainment leaves the individual neither satisfied nor dissatisfied.¹⁹

Definition 3: An individual's aspiration level is the utility of that achievement (among those which he can imagine himself attaining) which is the higher in utility of the two utility-wise adjacent achievements between which the rate of change of utility is maximum.²⁰

Definition 4: An individual's aspiration level is the utility of that achievement (among those

¹⁷ The definitions below are adapted from Bernard Grofman, "Utility and Aspiration," Department of Political Science, State University of New York at Stony Brook, ditto, 1971. For a general introduction to the literature on aspiration theory see W. H. McWhinney, "Aspiration Levels and Utility Theory," *General Systems Yearbook*, 10 (1965), 131-143.

¹⁸ See K. Lewin, T. Dembo, L. Festinger, and P. Sears, "Level of Aspiration," in *Personality and the Behavioral Disorders*, ed. J. McV. Hunt (New York: Ronald Press, 1944), pp. 333-378. By expected utility is meant the subjective probability of an event's occurrence times its utility, where utility is assumed to be expressed on an interval scale which satisfies the Von Neumann-Morgenstern axioms. (See any introductory text on decision theory.)

¹⁹ See Herbert Simon, "A Behavioral Model of Rational Choice," *Quarterly Journal of Economics*, 69 (February, 1955), 99-118.

²⁰ See Sidney Siegel, "Level of Aspiration and Decision-Making," *Psychological Review*, 64 (1957), 253-261.

¹⁴ Gurr, "A Comparative Study of Civil Strife," p. 572.

¹⁵ See Gurr, *Why Men Rebel*, pp. 63-66.

¹⁶ Feierabend et al., "Social Change and Political Violence: Cross-National Patterns," p. 627.

which he can imagine himself attaining) which is the higher in utility of the two expected utility-wise adjacent achievements between which the rate of change of *expected* utility is maximum.²¹

Definition 5: An individual's aspiration level is the utility of that achievement (among those which he can imagine himself attaining) which is the higher in utility of the two expected utility-wise adjacent achievements between which the rate of change of expected utility is maximum and which lie in the neighborhood of the individual's present level of achievement.²²

We introduce these definitions simply to exhibit the diverse range of distinct definitions on which one might base an Aspiration Level version of the AD concept.

Also, much of the work done to "test" the reference group version of the AD concept has employed measures of objective deprivation (e.g., nonwhite median family income divided by white family income) rather than subjective perceptions of achievement discrepancy.²³ Moreover, the reference group selected for comparison has been that which the scholar feels to be appropriate, rather than one whose appropriateness as a basis for comparison has been established by research at the micro level.

Attempts to test the AD explanation in any of its many guises with direct evidence from individual behavior have been few, although we regard micro level tests as crucial—given the form in which AD hypotheses have been couched. Most tests of AD hypotheses at the micro level have used Hadley Cantril's Self-Anchoring scale, a device which measures degree of perceived discrepancy between an individual's best possible level of achievement and his actual level of achievement.

The Self-Anchoring scale is a ladder of eleven rungs, numbered from zero to ten. Respondents are shown a picture of the ladder. In the Cantril version, the top rung of the ladder is defined as the respondent's best possible life, the bottom rung as his worst possible life.²⁴ Respondents are asked to place themselves on the ladder with respect to the present, five years in the past, and five years in the future.

According to Cantril's twelve-nation study,

welfare values, or conditions of life that facilitate physical well-being and self-realization, were by far the most salient personal concerns.²⁵ A variant of the Cantril Self-Anchoring scale, used by Muller, replaces life situation in general as the achievement referent with four categories of welfare values: career satisfaction, economic well-being, satisfactory living conditions, and children's welfare.²⁶ In the Welfare-Value version, respondents are asked to imagine that the top and bottom rungs of the ladder are best and worst possible levels of "work situation," "total family income," "housing accommodations," and "chances of being able to get your children a good education." Present, past, and future estimates of discrepancy between best possible and actual level of achievement are then obtained for each welfare value.

Position on the Self-Anchoring scale gives, we believe, an operationally unambiguous measure of at least one form of achievement discrepancy—discrepancy defined as the difference between an individual's subjective estimate of his level of achievement vis-à-vis some value(s) and his perception of his best possible level of achievement. The high end of the scale, position "10," indicates complete congruence between achievement optimum and achievement, or a state of perfect relative gratification; the low end of the scale, position "0," indicates complete incongruence between achievement optimum and achievement, or a state of perfect relative deprivation. For the Best Possible version of the AD concept, we may formulate the following hypotheses:

BP₁: Potential for political violence will vary strongly and directly with degree of discrepancy between an individual's estimate of his present achievement level and his perception of his best possible achievement level.

BP₂: Potential for political violence will vary strongly and directly with degree of discrepancy between the level of achievement which an individual expects to maintain in the future and his perception of his best possible achievement level.

The relationship between the above propositions and the other three versions of the AD

²¹ Suggested by McWhinney, "Aspiration Levels and Utility Theory."

²² Proposed by Grofman, "Utility and Aspiration."

²³ See pages 640 to 645 of Seymour Spilerman, "The Causes of Racial Disturbances: A Comparison of Alternative Explanations," *American Sociological Review*, 35 (August, 1970), 627-649.

²⁴ See Hadley Cantril, *The Pattern of Human Concerns* (New Brunswick: Rutgers University Press, 1965), pp. 22-26.

²⁵ By Gurr's classification, 62 per cent of all personal concerns had to do with welfare values; if we include the approximately 16 per cent of interpersonal values which relate to the family as part of the welfare value category, then 78 per cent of all personal concerns referred to welfare values. See Gurr, *Why Men Rebel*, p. 70.

²⁶ See the appendix in Muller, "A Test of a Partial Theory of Potential for Political Violence," pp. 955-959.

concept is far from clear. There is no good reason to believe—much less is it tautologically true—that the top of a Self-Anchoring scale represents the level of achievement which an individual would regard as his just deserts (at least in the ordinary language sense of that term); nor need it represent the achievement to which he aspires, where aspiration level is defined as in Definitions 1–5; nor need it represent the achievement which he believes has been attained by members of a relevant reference group. Since our measures of achievement discrepancy in this paper are based on the Self-Anchoring scale, the reader must be cautious in inferring conclusions about variants of the AD concept other than the Best Possible version.

The empirical linkages among the various forms of achievement discrepancy, and the relative efficacy of each form as a predictor of key variables such as potential for political violence, cry out for an investigation which our data do not allow us to provide. Nonetheless, we do conjecture that any of the particular results we find for the Best Possible definition of the AD concept also will hold true for other micro-level achievement discrepancy measures that might be constructed.

Regardless of how achievement optimum is defined, we shall distinguish between two forms of shift in perceived discrepancy. The most straightforward is one we might label the Rate-of-Change (RC) pattern. RC discrepancy shift ranges from positive change, where discrepancy is perceived to decrease over time, through no change, where discrepancy is perceived to increase over time. The particular dynamics which give rise to a shift in perceived discrepancy over time may vary. Discrepancy shift may result from a change in achievement optimum over time relative to a lesser change in, or a constant, actual achievement. An increase in achievement optimum relative to actual achievement would produce the kind of increase in discrepancy that has been labeled "aspirational deprivation" by Gurr and, as a cause of potential for political violence, is often called the "rising expectations" hypothesis.²⁷ Alternatively, discrepancy shift may result from a change in actual achievement relative to a lesser change in, or a constant, achievement optimum. A decrease in actual achievement relative to achievement optimum would produce the kind of increase in discrepancy that has

been labeled "decremental deprivation" by Gurr.²⁸ The basic hypothesis relating RC discrepancy to potential for political violence can be expressed as:

RC: Potential for political violence will vary strongly and directly with degree to which perceived rate of change in discrepancy between achievement optimum and achievement is negative.

To our knowledge, the RC hypothesis has been tested only once at the micro-level.²⁹ The various macro-level "tests"³⁰ have all used objective measures of achievement; but the connection between changes in objective social, political, and economic conditions and individual perceptions of discrepancy shift, as we have suggested before, is not at all clear.

The other major pattern of discrepancy shift which scholars have proposed as a cause of potential for political violence has been labeled "progressive deprivation" by Gurr and is often called the "J-curve" or "rise and drop" hypothesis.³¹

The J-Curve is this: revolution is most likely to take place when a prolonged period of rising expectations and rising gratifications is followed by a short period of sharp reversal during which the gap between expectations and gratification quickly widens and becomes intolerable. The frustration that develops, when it is intense and widespread in the society, seeks outlets in violent action.³²

The J-Curve pattern is one in which positive discrepancy shift (discrepancy is perceived to decrease over time) is followed by negative discrepancy shift (discrepancy is perceived to increase over time). "Tests" of the JC hypothesis have been conducted at the macro-level, and have been extremely inferential, since the objective measures of achievement employed have

²⁷ *Why Men Rebel*, pp. 46–50.

²⁸ Don R. Bowen, Elinor Bowen, Sheldon Gawiser, and Louis H. Masotti, "Deprivation, Mobility, and Orientation Toward Protest of the Urban Poor," in *Riots and Rebellion*, ed. Louis H. Masotti and Don R. Bowen (Beverly Hills: Sage, 1968) p. 187–200. These authors investigated a form of the RC hypothesis in which the measure of perceived change in discrepancy was dichotomized in various ways.

²⁹ See: Feierabend et al., "Social Change and Political Violence: Cross-National Patterns," pp. 638–643; Gurr, "A Comparative Study of Civil Strife," pp. 572–576.

³⁰ See: *Why Men Rebel*, pp. 52–56; James C. Davies, "The J-Curve of Rising and Declining Satisfaction as a Cause of Some Great Revolutions and a Contained Rebellion," in Graham and Gurr *Violence in America*; Davies, "Toward a Theory of Revolution"; Geschwender, "Social Structure and the Negro Revolt: An Examination of Some Hypotheses."

³¹ Davies, "The J-Curve," p. 671.

²⁷ See: Gurr, *Why Men Rebel*, pp. 50–52; James A. Geschwender, "Social Structure and the Negro Revolt: An Examination of Some Hypotheses," *Social Forces*, 43 (December, 1964), 248–256, especially p. 249.

not even been related directly to aggregate measures of political violence.³³

The *J*-Curve hypothesis is not free from ambiguity when we seek to operationalize it in micro terms, but it clearly seems to require at minimum that:

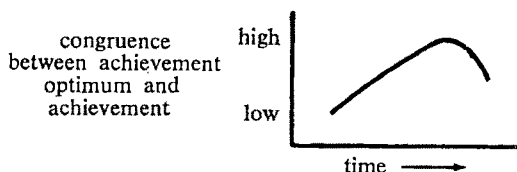
*J*_C: Mean potential for political violence will be greatest for those individuals who have experienced a perceived decrease in discrepancy between optimum achievement and actual achievement followed by a perceived increase in discrepancy between optimum achievement and actual achievement.

A more stringent version of the *J**C* hypothesis incorporates the assumption that, just as the *J*-Curve produces a high degree of dissatisfaction, so will a *reverse J*-Curve produce a high degree of satisfaction.³⁴ This more stringent—and more complete—*J**C* hypothesis requires that:

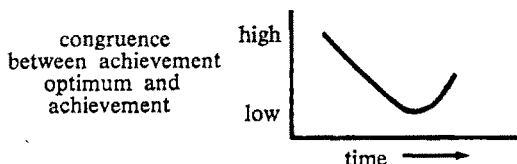
*J*_C: If individuals are classified according to whether they have experienced a perceived Rise and Drop (*J*-Curve) discrepancy shift, a No-Change discrepancy shift, or a Drop and Rise (*Reverse J*-Curve) discrepancy shift, then mean potential for political violence will be highest for those individuals in the Rise and Drop category and lowest for those individuals in the Drop and Rise category.

³³ See Davies, "The *J*-Curve," pp. 698–705.

³⁴ Actually the *J*-Curve label is somewhat confusing, since the pattern of change which Davies has in mind is really an upside down *J*, reversed and tilted to the right, as follows:



Thus, what we call a *reverse J*-Curve is really a rightside up *J*, reversed and tilted to the left, as follows:



In these figures we have adapted the ordinate to our achievement discrepancy concept; in Davies' formulation the ordinate refers to "needs" and the *J*-Curve describes a situation in which expected need satisfaction and actual need satisfaction have been increasing over time, followed by continued increase in expected need satisfaction but decrease in actual need satisfaction.

Now, just as we pointed out that the Self-Anchoring scale is only one among a number of definitions of achievement discrepancy, we must be careful to emphasize that when we come to test either the *RC* or the *J**C* hypothesis with the Self-Anchoring scale, our observations are only of experienced and anticipated shift over time in *actual achievement level* relative to optimum achievement level. When we observe a pattern of, say, decreasing discrepancy between achievement optimum and actual achievement from past to present, followed by anticipated increase in discrepancy from present to future, this could reflect either one of the following: (1) a pattern of rising experienced achievement and then falling anticipated achievement relative to an approximately constant achievement optimum; (2) a pattern of approximately constant experienced and anticipated achievement, and falling then rising achievement optimum. Unfortunately, as far as we know, there have been no investigations of whether the optimum level of achievement as defined by a Self-Anchoring scale is treated by individuals as a constant, or whether the past, present, and future comparisons are based on different optimum achievement levels.

Despite these difficulties, we believe that the tests we perform based on the Self-Anchoring scale measure of achievement discrepancy afford as good a test of the *RC* and *J**C* hypotheses as any to which these hypotheses have been subjected. Observation of shift in Self-Anchoring scale scores over time does provide an unambiguous measure of direction and amount of perceived discrepancy shift, even though the actual dynamics responsible for this shift—change in actual achievement relative to achievement optimum or change in achievement optimum relative to actual achievement or both—are indeterminate. And both the *RC* and *J**C* hypotheses share the common prediction that direction and amount (or at least direction) of discrepancy shift will be related to potential for political violence.

Tests of Relative Gratification Hypotheses

Since the top of a Self-Anchoring scale is a condition of complete congruence between achievement optimum and achievement, we shall call our Self-Anchoring scale variables measures of *relative gratification*. In this section we shall test hypotheses about relationships between potential for political violence and static and dynamic relative gratification constructs.

Our dependent variable is the Potential for Political Violence (PPV) scale developed by

Muller.³⁵ This scale ranges from a low score of zero to a high score of ten. It is a summation of scores on an Approval of Political Violence (APV) scale and an Intention to Engage in Political Violence (IEPV) scale. These scales were built from responses to a series of items referencing dissent behaviors which constitute progressively greater degrees of challenge to the customary and legal norms of a political regime.³⁶ Respondents were asked to report whether they approved or disapproved of each dissent behavior, and whether or not, under present circumstances, they would engage in each behavior. The APV and IEPV variables are Guttman scales which show Reproducibility of .91 and .93, respectively, for this sample, and Reproducibility of .94 and .95, respectively, for a community leaders sample.³⁷ APV and IEPV range from zero to five. Level "0" involves avoidance of any deviation from customary and legal regime norms; level "1," conformity with legal regime norms and nonviolent deviance from customary regime norms; level "2," nonviolent and generally nondisruptive deviance from legal regime norms; level "3," nonviolent but disruptive deviance from legal regime norms; level "4," relatively unplanned and sporadic violence to attack the regime; level "5," the planned use of violence to overthrow the regime. APV and IEPV are strongly correlated both in this sample and in the Community Leaders sample; both variables also correlate at greater than .90 with the composite PPV scale. Furthermore, APV and IEPV correlate strongly with allegiance to groups known to advocate anti-regime dissent behavior, both for this sample and for the Community Leaders sample.

³⁵ See the appendix in Muller, "A Test of a Partial Theory of Potential for Political Violence," p. 955-959.

³⁶ The behaviors were: (1) taking part in protest meetings or marches that are permitted by the local authorities; (2) refusing to obey a law which one thinks is unjust, if the person feels so strongly about it that he is willing to go to jail rather than obey the law; (3) trying to stop the government from functioning by engaging in sit-ins, mass demonstrations, take-overs of buildings, and things like that; (4) trying to stop the government from functioning by engaging in violent protest demonstrations, including actions such as fighting with the police and destroying public and private property; (5) trying to challenge the power of the government by arming oneself in preparation for battles with government authorities such as the police and the National Guard.

³⁷ See Edward N. Muller, "Measurement of Readiness for Unconventional Political Participation," Department of Political Science, State University of New York at Stony Brook, mimeo 1972.

Static Relative Gratification. The static relative gratification variables (range: 0-10) which we shall use to test BP₁ and BP₂ are:

Achieved Relative Gratification—Degree of consequence between

W_p : best possible work situation and work situation in the present.

I_p : best possible total family income and total family income in the present.

H_p : best possible housing accommodations and housing accommodations in the present.

C_p : best possible chances of getting children a good education and chances of getting children a good education in the present.

Expected Relative Gratification—Degree of consequence between

W_{p+5} : best possible work situation and work situation five years in the future.

I_{p+5} : best possible total family income and total family income five years in the future.

H_{p+5} : best possible housing accommodations and housing accommodations five years in the future.

C_{p+5} : best possible chance of getting children a good education and chances of getting children a good education five years in the future.

Assuming PPV and our Achieved Relative Gratification and Expected Relative Gratification variables to be interval scales, we may readily taste the hypotheses:

BP₁': The relationship between PPV and W_p, \dots, C_p will show a relatively close fit to an inverse linear function.

BP₂': The relationship between PPV and W_{p+5}, \dots, C_{p+5} will show a relatively close fit to an inverse linear function.

We shall define "relatively close fit" as an r^2 equal to or greater than .20—i.e., at least 20% of the variance in the dependent variable must be accounted for by the variance in the independent variable according to the function rule.

Table 1 reports the evidence. Both BP₁' and BP₂' are disconfirmed in every instance. For PPV regressed against the Achieved Relative Gratification variables, r^2 ranges from .02 to .05, considerably less than our .20 criterion; for PPV regressed against the Expected Relative Gratification variables, r^2 is zero in every case.

The analysis of variance results presented on the right-hand side of Table 1 do show that there are statistically significant linear relationships between PPV and the Achieved Relative Gratification variables. PPV, however, is only weakly correlated with the Achieved Relative Gratification variables, and PPV scores are considerably underestimated given low scores on these variables.³⁸

³⁸ Since both PPV and the Achieved Relative Gratification variables have the same range, if PPV were a perfect inverse linear function of Achieved Rela-

Table 1. Regression of Potential for Political Violence Against Measures of Static Relative Gratification

Regression Equation	r^2	Analysis of Variance			
			df	mean square	F
Achieved Relative Gratification					
1) $PPV = -.166(W_p) + 3.542$.04	regression	1	97.09	20.99 p < .001
		residual	455	4.63	
2) $PPV = -.122(I_p) + 3.166$.02	regression	1	48.64	10.68 p < .01
		residual	483	4.56	
3) $PPV = -.184(H_p) + 3.589$.05	regression	1	116.23	25.77 p < .001
		residual	487	4.51	
4) $PPV = -.156(C_p) + 3.851$.03	regression	1	44.01	8.81 p < .01
		residual	296	5.0	
Expected Relative Gratification					
5) $PPV = .003(W_{p+s}) + 2.518$.00	regression	1	.03	.01 n.s.
		residual	394	4.74	
6) $PPV = .031(I_{p+s}) + 2.266$.00	regression	1	3.49	.75 n.s.
		residual	436	4.63	
7) $PPV = .016(H_{p+s}) + 2.358$.00	regression	1	.88	.19 n.s.
		residual	459	4.68	
8) $PPV = -.017(C_{p+s}) + 2.879$.00	regression	1	.48	.09 n.s.
		residual	283	5.17	

In previous research, the Self-Anchoring scale has been dichotomized or trichotomized and the measure of violence potential also often has been a dichotomized variable.³⁹ When we collapse our relative gratification variables into trichotomies by the usual procedure, and dichotomize PPV at the mean, we may test the following hypotheses of inverse monotonic relationships:

BP_1'' : The higher the congruency ranking on W_p, \dots, C_p , the lesser will be the proportion of individuals scoring high on PPV.

BP_2'' : The higher the congruency ranking on W_{p+s}, \dots, C_{p+s} , the lesser will be the proportion of individuals scoring high on PPV.

tive Gratification, then a person with a score of "0" on Achieved Relative Gratification should have a score of "10" on PPV and a person with a score of "10" on Achieved Relative Gratification should have a score of "0" on PPV. However, the equation for PPV regressed against W_p , for example, estimates that, given a score of "0" on W_p , a person will have a score of only 3.542 on PPV ($-.166 \times 0 + 3.542 = 3.542$); whereas, given a score of "10" on W_p , a person's predicted PPV score is 1.882 ($-.166 \times 10 + 3.542 = 1.882$).

³⁹ See: Bowen et al., "Deprivation, Mobility, and Orientation Toward Protest of the Urban Poor;" Thomas J. Crawford and Murray Naditch, "Relative Deprivation, Powerlessness, and Militancy: The Psychology of Social Protest," *Psychiatry* (May, 1970), 208-223.

Table 2 shows the relationships between the dichotomized PPV variable and the trichotomized relative gratification variables. For the cases of W_p and H_p , the BP_1'' hypothesis clearly is confirmed. The percentage comparisons show that the proportion scoring high on PPV does decrease consistently as congruency ranking on W_p and on H_p increases, and the chi square value for these relationships is less than .01, the alpha level we shall select for rejection of the null hypothesis. In the case of I_p , the chi square value is greater than .01, therefore, we conclude that PPV level is unrelated to I_p level and reject BP_1'' . For the relationship between PPV and C_p , the chi square value is significant at less than .01, but the percentage comparisons show that the effect of C_p on the PPV level occurs from low to medium levels of congruency, where the proportion scoring high on PPV declines fairly precipitously; but not from medium to high congruency, where the proportion high on PPV levels off. Thus, we reject BP_1'' for the case of C_p , although we note that the observed relationship is consistent with an inverse nonstrict monotonic hypothesis of the form, as C_p increases, the proportion high on PPV decreases or remains the same.

The chi square values for the relationships

between PPV and the Expected Relative Gratification variables are all greater than .01. For this sample we conclude that PPV level is unrelated to the trichotomized Expected Relative Gratification variables and reject BP_2 .⁴⁰

The relationships shown in Table 2 are similar to those found in the Cleveland study reported by Don R. Bowen, Elinor Bowen, Sheldon Gawiser, and Louis H. Masotti.⁴⁰ Percentage comparisons for the Cleveland sample were as predicted by BP_1 , although the chi square value for the relationship was significant at only the .10 level. Hypothesis BP_2 was clearly disconfirmed for the Cleveland sample. Other studies also appear to support BP_1 , but the percentage comparisons do not seem to indicate very strong monotonic relationships.⁴¹

Thus, key propositions of achievement discrepancy theory, when operationalized in a form recommended by a number of its advocates (to wit, using the Self-Anchoring scale to measure discrepancy) are not confirmed by our data. For this sample, our measure of potential for political violence clearly is not strongly correlated with degree of relative gratification in the present and is uncorrelated with degree of

relative gratification expected in the future. When we trichotomize and dichotomize our variables, our findings are quite similar to those reported in earlier studies of different communities, i.e., some—but apparently not very strong—monotonic association between potential for political violence and expected level of relative gratification. We conclude that, at least in American urban communities, it is unlikely that as level of relative gratification expected in the future increases, potential for political violence will decrease; and while it is likely that as the level of relative gratification in the present increases, potential for political violence will tend to decrease, the relationship probably will not be of the predictive strength postulated by proponents of achievement discrepancy theory.

Dynamic Relative Gratification. Given our measures of degree of perceived congruence between achievement optimum and actual achievement for the present, five years in the past, and five years in the future, we may define fourteen different patterns of relative gratification over time. These patterns are depicted graphically in Figure 1. Graphs a, b, and c are clear Rise-and-Drop or J-Curve patterns. Graphs d, e, and f all show Decreasing relative gratification from past to future. Graphs g and h show a No-Change pattern of relative deprivation and of gratification, respectively. Graphs i, j, and k show Increasing relative gratification from past to future. Graphs l, m, and n are clear Drop-and-Rise or Reverse J-Curve patterns.

Table 3 shows the mean PPV scores for each pattern of relative gratification on the four welfare values. The JC_1 hypothesis predicts that individuals characterized by the Rise and Drop

Table 2. Relationships Between Potential for Political Violence (Dichotomized) and Measures of Static Relative Gratification (Trichotomized)

Achieved Relative Gratification													
		W_p			I_p			H_p			C_p		
		Low	Medium	High	Low	Medium	High	Low	Medium	High	Low	Medium	High
PPV	Low	61.1	68.8	78.8	64.2	74.0	77.4	52.8	73.1	81.0	39.1	70.6	71.1
	High	38.9	31.3	21.2	35.8	26.0	23.6	47.2	26.9	19.0	60.9	29.4	28.9
		100%	101%	100%	100%	100%	100%	100%	100%	100%	100%	100%	100%
		$\chi^2=10.61$			$\chi^2=5.26$			$\chi^2=22.63$			$\chi^2=9.93$		
		$p < .01$			n.s.			$p < .001$			$p < .01$		
Expected Relative Gratification													
		W_{p+5}			I_{p+5}			H_{p+5}			C_{p+5}		
		Low	Medium	High	Low	Medium	High	Low	Medium	High	Low	Medium	High
PPV	Low	66.1	75.4	72.5	68.2	80.2	71.1	61.2	79.8	72.4	46.7	73.8	67.3
	High	33.9	24.6	27.5	31.8	19.8	28.9	38.8	20.2	27.6	53.3	26.2	32.7
		100%	100%	100%	100%	100%	100%	100%	100%	100%	100%	100%	100%
		$\chi^2=1.47$			$\chi^2=4.05$			$\chi^2=6.34$			$\chi^2=4.17$		
		n.s.			n.s.			n.s.			n.s.		

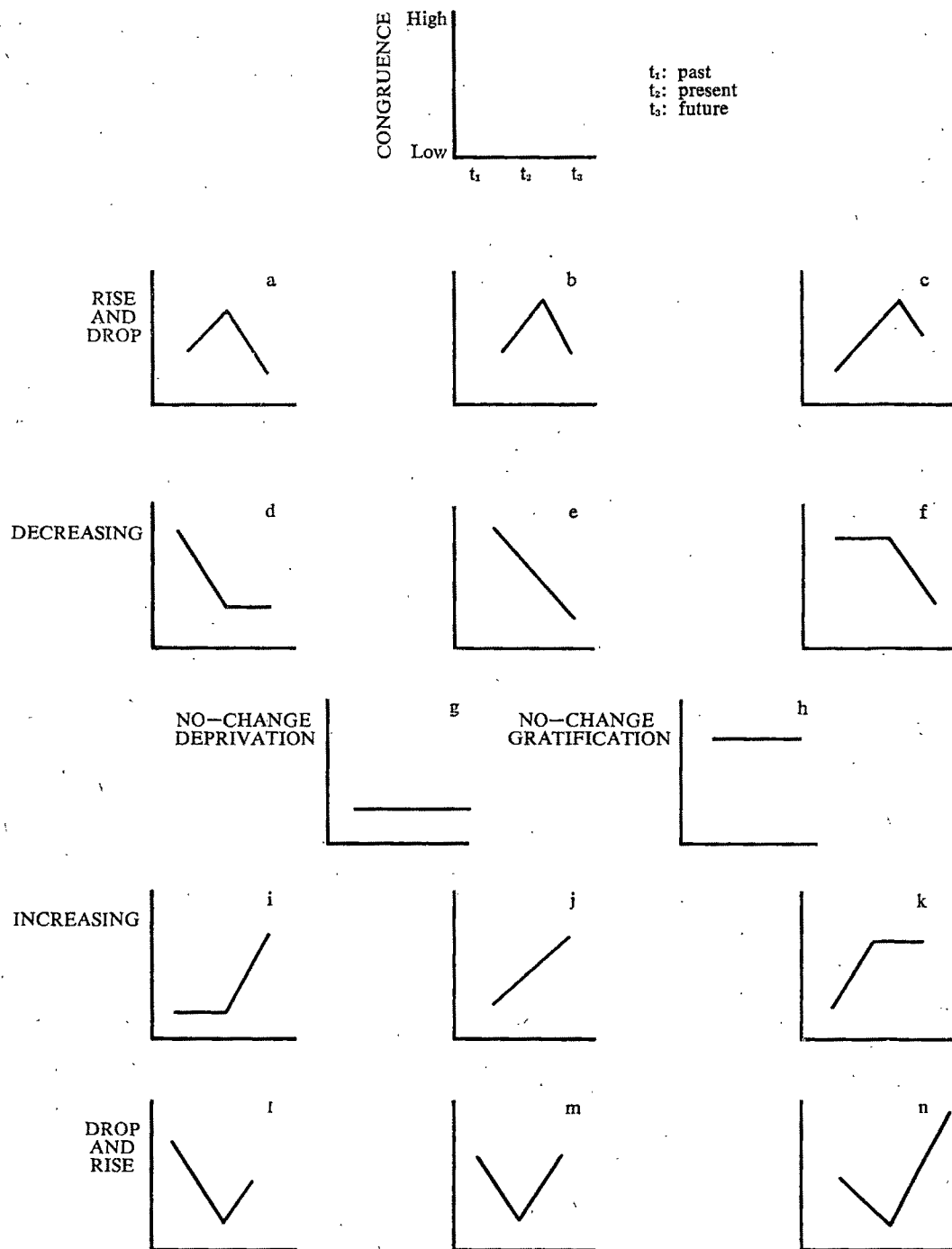


Figure 1. Patterns of shift in relative gratification over time

patterns will show a higher mean PPV than individuals characterized by any of the other patterns. The JC_2 hypothesis predicts that individuals characterized by the Rise-and-Drop patterns will show a higher mean PPV than individuals characterized by the No-Change patterns and that individuals characterized by the Drop-and-Rise patterns will show a lower mean PPV than individuals characterized by the No-Change patterns. The bottom half of Table 3 presents the relevant comparisons between means for testing JC_1 and JC_2 . Table 3 shows that our predictions are correct in only 14 of 33 comparisons for patterns on the Work variable; in only 16 of 33 comparisons on the income variables; and in none of the possible 11

comparisons on the housing variable. For JC_1 the Confirmation Index (CI) is thus .39: our prediction that the Rise-and-Drop patterns will have the greatest mean PPV is borne out by these data only 39 per cent of the time. Since Rise-and-Drop patterns *do not show* higher mean PPV than other patterns 61 per cent of the time, we interpret this as clear disconfirmation of JC_1 .

Table 3 shows that our JC_2 predictions are correct in only 5 of 12 comparisons for patterns on the Work variable, 6 to 12 comparisons on the Income variable, 0 of 8 comparisons on the Housing variable, and 0 of 2 comparisons on the Children variable. The Confirmation Index for JC_2 is .32. Our prediction that Rise-and-

Table 3. Mean Potential for Political Violence Scores for Patterns of Shift in Relative Gratification Over Time

	Pattern	Work	Income	Housing	Children
Rise-and-Drop	<i>a</i>	2.4	3.5	***	***
	<i>b</i>	1.5	1.5	1.2	***
	<i>c</i>	3.1	2.0	***	***
Decreasing	<i>d</i>	1.7	2.0	1.7	***
	<i>e</i>	3.5	3.6	3.2	4.0
	<i>f</i>	2.5	1.4	2.3	***
No-Change Deprivation	<i>g</i>	2.1	1.3	2.2	1.5
No-Change Gratification	<i>h</i>	1.6	1.3	1.8	1.6
Increasing	<i>i</i>	3.4	3.2	2.6	2.6
	<i>j</i>	2.9	2.8	3.0	3.3
	<i>k</i>	2.1	2.4	2.2	2.6
Drop-and-Rise	<i>l</i>	1.7	2.2	2.6	***
	<i>m</i>	2.3	2.9	4.3	***
	<i>n</i>	3.4	2.9	3.0	5.3

*** less than five cases

JC_1 Comparisons:

		Work	Income	Housing	Children
<i>a</i>	<i>d, e, . . . n:</i>	6 of 11	10 of 11	—	—
<i>b</i>	<i>d, e, . . . n:</i>	0 of 11	3 of 11	0 of 11	—
<i>c</i>	<i>d, e, . . . n:</i>	8 of 11	3 of 11	—	—

Confirmation Index = $30 \div 77 = .39$

JC_2 Comparisons:

		Work	Income	Housing	Children
<i>a</i>	<i>g, h:</i>	2 of 2	2 of 2	—	—
<i>b</i>	<i>g, h:</i>	0 of 2	2 of 2	0 of 2	—
<i>c</i>	<i>g, h:</i>	2 of 2	2 of 2	—	—
<i>l</i>	<i>g, h:</i>	1 of 2	0 of 2	0 of 2	—
<i>m</i>	<i>g, h:</i>	0 of 2	0 of 2	0 of 2	—
<i>n</i>	<i>g, h:</i>	0 of 2	0 of 2	0 of 2	0 of 2

Confirmation Index = $11 \div 34 = .32$

Table 4. Relationships Between Potential for Political Violence (Dichotomized) and Patterns of Shift in Relative Gratification Over Time

		Work Gratification					
		Rise-and-Drop	Decreasing	No-Change Deprivation	No-Change Gratification	Increasing	Drop-and-Rise
PPV	Low High	72.7	66.7	90.0	87.8	68.4	65.5
		27.3	33.3	10.0	12.2	31.6	34.5
		100%	100%	100%	100%	100%	100%
	N =	22	63	10	49	187	29
		$\chi^2 = 10.11$	n.s.				
		Income Gratification					
		Rise-and-Drop	Decreasing	No-Change Deprivation	No-Change Gratification	Increasing	Drop-and-Rise
PPV	Low High	64.0	75.3	100.0	91.7	68.9	65.7
		36.0	24.7	0.0	8.3	31.1	34.3
		100%	100%	100%	100%	100%	100%
	N =	25	73	20	36	225	35
		$\chi^2 = 17.84$	$p < .01$				
		Housing Gratification					
		Rise-and-Drop	Decreasing	No-Change Deprivation	No-Change Gratification	Increasing	Drop-and-Rise
PPV	Low High	81.8	70.0	78.2	89.6	67.8	58.3
		18.2	30.0	21.8	10.4	32.2	41.7
		100%	100%	100%	100%	100%	100%
	N =	11	50	55	96	180	48
		$\chi^2 = 22.72$	$p < .001$				
		Children Gratification					
		Rise-and-Drop	Decreasing	No-Change Deprivation	No-Change Gratification	Increasing	Drop-and-Rise
PPV	Low High	20.0	55.0	85.0	90.6	65.7	33.3
		80.0	45.0	15.0	9.4	34.3	66.7
		100%	100%	100%	100%	100%	100%
	N =	5	20	20	53	134	12
		$\chi^2 = 29.32$	$p < .001$				

Drop patterns will show higher mean PPV while Drop-and-Rise patterns show lower mean PPV than No-Change patterns is wrong 68 per cent of the time; therefore, we interpret this .32 index value as providing clear disconfirmation of JC_2 . In fact, if we examine the JC_2 comparisons closely, we find a con-

sistent reason for the disconfirmation of JC_2 : most of the Rise-and-Drop predictions are correct ($CI = 10 \div 14 = .72$), but almost all of the Drop-and-Rise predictions are incorrect ($CI = 1 \div 20 = .05$). If we hypothesized that both Rise-and-Drop and Drop-and-Rise patterns would show higher mean PPV than

No-Change patterns, our Confirmation Index is $29 \div 34 = .85$.

When PPV is dichotomized into low and high categories, we may test the hypothesis:

JC₃: If individuals are classified into Rise-and-Drop, Decreasing, No-Change Deprivation, No-Change Gratification, Increasing, and Drop-and-Rise patterns of relative gratification over time, those in the Rise-and-Drop and Decreasing categories will show a greater proportion high on PPV than those in the two No-Change categories while those in the Increasing and Drop-and-Rise categories will show a lesser proportion high on PPV than those in the two No-Change categories.

Table 4 shows the relationships between temporal relative gratification pattern on each welfare value and PPV. For the relationship between the Work variable and PPV we do not reject the null hypothesis, since chi square is not significant at the .01 level. But for Income, Housing, and Children, chi square is significant at well below the .01 level. However, the percentage comparisons show that in each of these instances, while those in the Rise-and-Drop and Decreasing categories do tend to score high on PPV than do those in the No-Change categories. Those in the Increasing and Drop-and-Rise categories never show a lesser tendency to score high on PPV than the No-Change categories. In fact the Increasing and Drop-and-Rise categories always yield higher scores on PPV than do the No-Change categories—as high, in fact, generally as the Rise-and-Drop and Decreasing categories! These data clearly disconfirm *JC₃*; and they equally clearly confirm (for the Income and Children variables), the alternative hypothesis:

JC₄: The proportion of individuals scoring high on PPV will decrease from Rise-and-Drop and Decreasing categories to the No-Change Deprivation category and will then increase from the No-Change Gratification category to the Increasing and Drop-and-Rise categories.

The percentage comparisons show the same thing as the comparisons between means presented in Table 3: the *J*-Curve hypothesis is not supported because individuals who perceive a drop and then a rise in congruence between achievement optimum and actual achievement have nearly the same tendency to score high on PPV as those who perceive the *J*-Curve pattern of a rise and then a drop in congruence. If we consider only patterns of relative deprivation over time—Rise-and-Drop, Decreasing, No-Change—then the *J*-Curve thesis that the Rise-and-Drop group will be the most violence-

prone is supported for two of the four welfare values (Income and Children). But in any society, many individuals will perceive patterns of relative gratification over time—No-Change, Increasing, Drop-and-Rise; and among these individuals, the Drop-and-Rise group is the most violence-prone for three of the four welfare values (Income, Housing, Children). In short, it does not seem to be a particular direction of change as exemplified by the *J*-Curve which produces the greatest tendency to violence-proneness; rather, violence-proneness seems to result from any directional change in degree of congruence between achievement optimum and achievement over time.⁴²

In contrast to the *J*-Curve hypothesis, which focuses on *change in direction* of achievement discrepancy over time, the Rate-of-Change hypothesis focuses on the *direction (and amount)* of change in achievement discrepancy over time. Given our past, present, and future Self-Anchoring scale scores for each of the four welfare values, we may construct the following Rate-of-Change variables (range: -10 to +10):

PF_{w, . . . , c}: Past to Future shift in achievement congruence; as $W_{p+5} - W_{p-5}, \dots, C_{p+5} - C_{p-5}$.

AI_{w, . . . , c}: Anticipated Increase shift in achievement congruence; defined as $W_{p+5} - W_p, \dots, C_{p+5} - C_p$.

EL_{w, . . . , c}: Experienced Increase shift in achievement congruence; defined as $W_p - W_{p-5}, \dots, C_p - C_{p-5}$.

These variables measure direction and amount of perceived change in the gap between a person's achievement optimum and actual level of achievement with respect to different temporal comparisons: past versus future; present versus future; and past versus present. A positive score indicates that the person perceives a decreasing gap; a score of zero indicates that the person perceives no change in the size of the gap; a negative score indicates that a person perceives an increasing gap.

If we assume that our variables are interval scales, we may test the Rate-of-Change hypothesis:

RC': The relationship between PPV and *PF_{w, . . . , c}*, *AI_{w, . . . , c}*, *EL_{w, . . . , c}* will show a relatively close fit to an inverse linear function.

⁴² When the Long-Term Welfare Gratification measure constructed by Muller (see appendix in "A Test of a Partial Theory of Potential for Political Violence") is trichotomized into "low" (-10 to -1), "medium" (0 to 10), and "high" (11 to 20) categories, the same kind of relationship appears: persons in the low (negative change) and high (positive change) categories consistently show higher mean PPV than persons in the medium (very little change) category.

Table 5. Correlations (*r*): Potential for Political Violence with Measures of Rate-of-Change Gratification

Welfare Value	Rate-of-Change Gratification		
	Past to Future	Anticipated Increase	Experienced Increase
Work N=	.061† (361)	.135** (394)	-.033† (418)
Income N=	.095* (414)	.146*** (438)	.028† (457)
Housing N=	.082* (440)	.225*** (461)	-.120** (465)
Children N=	.120* (244)	.160** (281)	.013† (260)

* $p < .05$

** $p < .01$

*** $p < .001$

† $p > .05$

In Table 5 we simply report the correlation coefficients, rather than the complete regression equations. As we might expect, given our findings for the tests of the *J*-Curve hypothesis, these data disconfirm *RC'* in every instance. The correlations between the Rate-of-Change variables and PPV are consistently minuscule. They indicate that PPV shows no tendency to vary linearly with the PF variables, practically no tendency to vary linearly with the EI variables, and only a slight tendency to vary linearly with the AI variables—and in the case of

the AI variables; the slight linear relationships with PPV are all in the wrong direction!⁴³

Table 6 shows why *RC'* is not confirmed. For convenience of presentation, we have trichotomized the Rate-of-Change variables into Negative Change, No-Change, and Positive Change categories. In every case, mean PPV decreases from the Negative Change group to the No-Change group, but then *increases* from the No-Change group to the Positive Change group; and in most instances, the Positive Change group shows about the same mean PPV as the Negative Change group.

For the PPV dichotomy and the Rate-of-Change trichotomies, we may test the following hypothesis of inverse monotonic association:

RC'': The more positive is the shift over time in congruence between achievement optimum and achievement, the lower the PPV.

Of course, on the basis of our examination of the *J*-Curve hypothesis and the distribution of mean PPV scores for the Rate-of-Change variables, we expect that *RC''* will be disconfirmed; and we expect that an alternative Absolute Change hypothesis will be confirmed:

AC: Individuals who perceive any shift over time in congruence between achievement optimum and actual achievement, regardless of whether the direction of shift is positive or negative, will be more likely to score high

⁴³ Our alpha level for rejection of the null hypothesis is .01.

Table 6. Mean Potential for Political Violence Scores for Measures of Rate-of-Change Gratification Trichotomized by Direction of Change^a

Welfare Value	Rate-of-Change Gratification								
	Past to Future			Anticipated Increase			Experienced Increase		
	(-)	(0)	(+)	(-)	(0)	(+)	(-)	(0)	(+)
Work N=	2.6 (79)	1.8 (76)	2.8 (206)	2.9 (67)	1.8 (138)	2.9 (189)	2.8 (96)	2.1 (134)	2.6 (188)
Income N=	2.5 (95)	1.6 (77)	2.7 (242)	2.5 (74)	1.9 (148)	2.9 (216)	2.7 (98)	1.8 (129)	2.6 (230)
Housing N=	2.6 (65)	2.1 (174)	2.7 (201)	2.6 (49)	2.0 (227)	3.0 (185)	3.1 (84)	2.0 (243)	2.6 (138)
Children's Education N=	3.4 (25)	1.8 (78)	3.0 (141)	3.8 (25)	2.2 (142)	3.3 (114)	4.1 (31)	1.7 (108)	3.1 (121)

^a (-) = -10 thru -1; (0) = 0; (+) = 1 thru 10.

on PPV than will individuals who perceive no such shift over time in congruence.

According to the Absolute Change hypothesis, the proportion scoring high on PPV in Negative Change, No-Change, and Positive Change categories should conform to a "V" pattern if we display these proportions on a graph with the percentage high on PPV shown along with ordinate.

The relationships between the Past to Future shift categories and the PPV dichotomy are depicted graphically in Figure 2. For alpha of .01 the chi square for the relationship between PF_w and PPV is not statistically significant, and we accept the null hypothesis—although we note that the chi square value approaches the .01 level of significance and that the percentage comparisons are in accord with the Absolute Change or V-Curve prediction. We reject the null hypothesis for every other relationship between a Past to

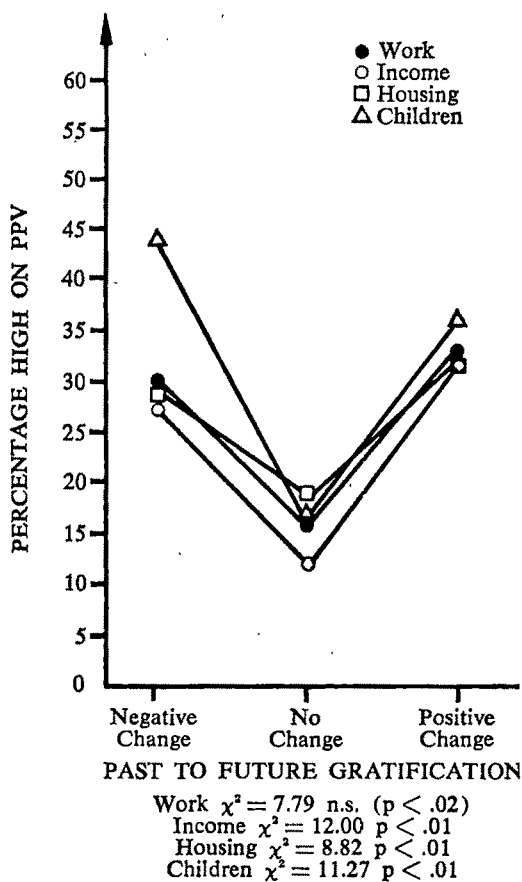


Figure 2. Percentage high on potential for political violence by direction of change category on measures of past to future gratification

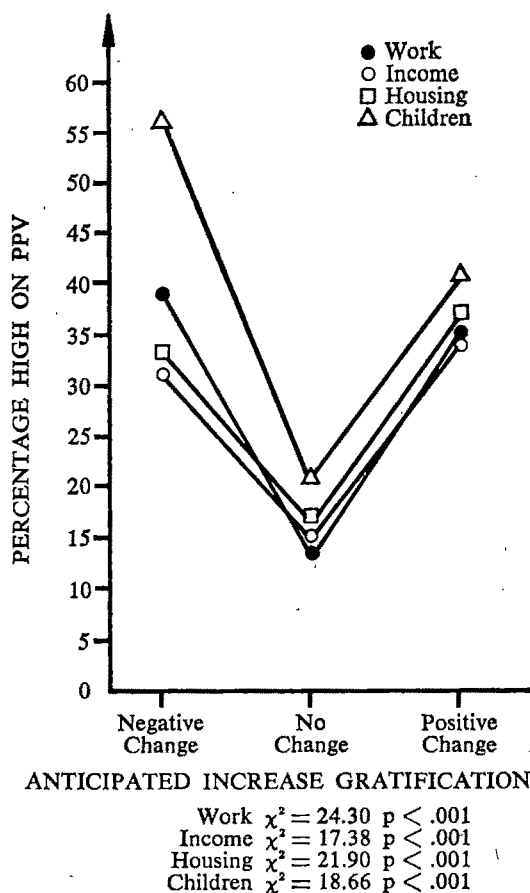


Figure 3. Percentage high on potential for political violence by direction of change category on measures of anticipated increase in gratification

Future shift variable and PPV. The percentage comparisons show that RC' is disconfirmed for the Income, Housing, and Children variables. And it is evident that the significant differences in proportions scoring high on PPV are between Negative Change as opposed to No-Change and between Positive Change as opposed to No-Change, but not between Negative Change as opposed to Positive Change. The proportions scoring high on PPV conform to the "V" pattern indicative of the Absolute Change relationship; thus, in these three instances, the AC hypothesis clearly is confirmed.

In Figure 3 we plot the proportions scoring high on PPV for the direction of shift categories on the Present to Future shift variables. Since chi square is statistically significant at well below the .01 level for each of the four relationships, we reject the null hypothesis in every instance. The proportion

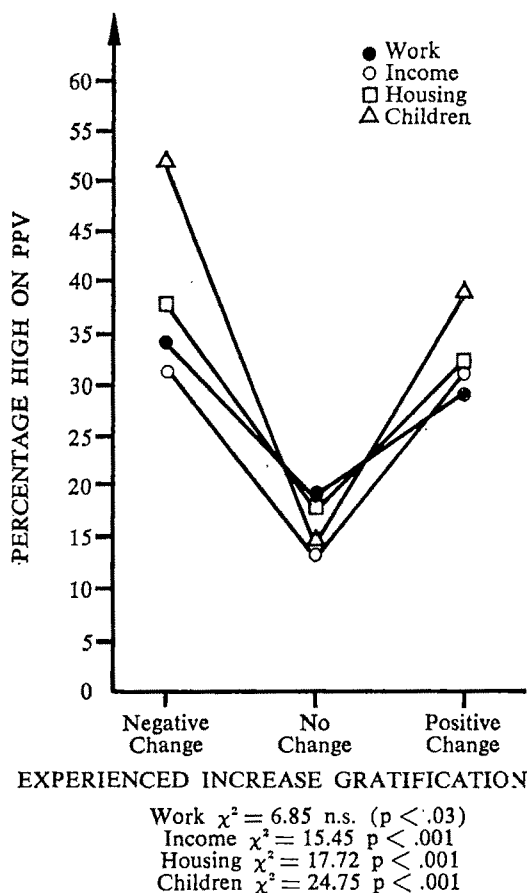


Figure 4. Percentage high on potential for political violence by direction of change category on measures of experienced increase in gratification

scoring high on PPV always decreases from perceived negative shift in relative gratification over time to no shift, but it always *increases* from no shift to positive shift. Thus, we reject RC". It is evident that significantly more of the Negative Change and Positive Change groups are high on PPV; and, excepting the Children variable, the proportion scoring high on PPV for the Negative Change groups is approximately equal to that for the Positive Change groups. Thus, each of the percentage comparisons plots as a distinct V-Curve, confirming the AC hypothesis.

Figure 4 depicts the proportions high on PPV for the Past to Present shift categories. With the exception of the Work variable, the chi-square values are well below the .01 level, and we reject the null hypothesis; also, for the relationship between EL_{10} and PPV we note that chi square is close to the .01 level

and that percentage comparisons do plot as a V-Curve. On the Income, Housing, and Children variables, the No-Change groups range from 13 to 19 per cent high on PPV, whereas the Negative Change groups range from 31 to 57 per cent high on PPV and the Positive Change groups range from 29 to 39 per cent high on PPV. These relationships clearly conform to the "V" pattern, and we again reject the RC" hypothesis, but accept the AC hypothesis.

Since the percentage comparisons are as predicted by AC for all 12 of these relationships, and since chi square is significant at or beyond the .01 level in 10 of the 12 relationships, we regard these data as providing consistent support for the Absolute Change or V-Curve hypothesis. Moreover, the only other test of a Rate-of-Change hypothesis that we know of, reported by Bowen et al. for their Cleveland sample, also disconfirmed RC" and supported AC, the Absolute Change hypothesis, in the case of the relationship between Protest Orientation and an Anticipated Increase measure built from the "best possible life" version of the Self-Anchoring scale.⁴⁴

Mean PPV by Rate-of-Change Categories and Control Variables. We have found generally consistent confirmation for the Absolute Change hypothesis across the entire sample. Even more dramatic support will be provided if it turns out that the Absolute Change predictions are borne out for various subsamples. In this section we shall examine the relationships between PPV and composite Rate-of-Change variables, controlling for levels of three variables which might produce a specification effect on the Absolute Change relationship.

We shall control for levels of race, age, and education. These variables have been selected because it seems possible that certain correlates of each of these variables might be responsible for the absolute change relationship. In the case of race, blacks, of course, have been severely discriminated against in American society. Perhaps among this group, those perceiving no change have become so inured to their condition that even if they perceive continuing relative deprivation they will be unlikely to believe that anything can be about it, whereas those who have some hope of positive change will be

⁴⁴See Table 5 at page 197 of Bowen et al., "Deprivation, Mobility, and Orientation Toward Protest of the Urban Poor," present-to-future shift on the Self-Anchoring scale is dichotomized into a change category ("Upward and Downward Mobility") and a no-change category ("No Mobility").

more violence-prone—and, indeed, as violence-prone as those who perceive negative change—because any improvement after a long period of abject subjugation will generate dissatisfaction with the *rate* of improvement. For the age variable, perhaps the absolute change relationship holds only among the old, because among this group those perceiving no change are resigned to their condition, whereas the group perceiving the possibility of any change will tend to include more activists who, regardless of the direction of perceived change, would score higher on PPV. Similarly in the case of education, perhaps among the group with only a grade school education, those perceiving no change would be particularly apathetic toward the possibility of altering their circumstances, whereas the group perceiving the possibility of any change would tend to include more activists.

To simplify our presentation we have constructed composite Rate-of-Change variables, and we examine mean scores on PPV for the various combinations of direction of shift categories and levels of control variables. Since the number of cases on the Children variables is so much smaller than the *N* on the other welfare value measures, we have excluded the Children

variables from the composite variables (range: -30 to +30), which are:

PFG: Past-to-Future Gratification. Sum of scores on PF_w , PF_i , and PF_h .

AIG: Anticipated Increase Gratification: Sum of scores on AI_w , AI_i , and AI_h .

EIG: Experienced Increase Gratification. Sum of scores on EI_w , EI_i , and EI_h .

If a respondent was missing a score for two or more of the component variables, the case was excluded; if only one score was missing, the mean of that variable was assigned. (The percentage of cases missing on PFG, AIG, and EIG was, respectively, 16 per cent, 12 per cent, and 8 per cent.)

Table 7 shows mean PPV by PFG shift category and levels of the control variables. Regardless of levels of race, age, and education, the Negative Change and Positive Change groups consistently score higher on PPV than does the No-Change group.

Table 8 shows mean PPV by AIG shift category and levels of the control variables. Again, regardless of race and education, individuals who anticipate any change in relative gratification—whether negative or positive—are more violence-prone on the average than individuals

Table 7. Mean Potential for Political Violence Scores for Direction of Rate of Change Gratification Stratified by Test Factors

Past-Future Gratification ^a		Negative Change (-)			No Change (0)			Positive Change (+)		
	<i>N</i> =	2.4 (108)			1.2 (39)			2.6 (274)		
By:	Race									
		Black			White					
		(-)	(0)	(+)		(-)	(0)	(+)		
	<i>N</i> =	3.2 (46)	1.7 (07)	3.7 (32)	1.9 (62)	1.1 (32)	1.9 (168)			
Age		18 to 33			34 to 57			58 and up		
		(-)	(0)	(+)	(-)	(0)	(+)	(-)	(0)	(+)
		4.1 (15)	1.5 (06)	3.0 (109)	2.8 (46)	1.2 (18)	2.5 (130)	1.6 (47)	1.2 (15)	1.7 (35)
Education ^b		Grade School			High School			College		
		(-)	(0)	(+)	(-)	(0)	(+)	(-)	(0)	(+)
		4.1 (41)	1.5 (10)	3.0 (48)	2.8 (49)	1.2 (23)	2.5 (179)	1.6 (18)	1.2 (06)	1.7 (47)

Table 8. Mean Potential for Political Violence Scores for Direction of Anticipated Increase Gratification Stratified by Test Factors

Anticipated Increase Gratification		Negative Change (-)			No Change (0)			Positive Change (+)		
	N=	2.2 (86)			1.6 (70)			2.8 (286)		
By:										
	Race	Black						White		
		(-)	(0)	(+)			(-)	(0)	(+)	
	N=	3.3 (36)	2.4 (18)	3.7 (108)			1.5 (50)	1.3 (18)	2.2 (178)	
	Age	18 to 33			34 to 57			58 and up		
		(-)	(0)	(+)	(-)	(0)	(+)	(-)	(0)	(+)
	N=	3.5 (10)	1.9 (07)	3.1 (135)	2.8 (42)	1.5 (43)	2.8 (109)	1.2 (34)	1.8 (20)	1.7 (42)
	Education	Grade School			High School			College		
		(-)	(0)	(+)	(-)	(0)	(+)	(-)	(0)	(+)
	N=	2.6 (30)	1.6 (21)	2.8 (48)	2.1 (46)	1.7 (41)	2.6 (182)	1.8 (10)	1.5 (08)	3.4 (56)

who do not anticipate any change in relative gratification. Also, among the young and the middle-aged, those who anticipate any change in relative gratification show higher mean PPV than those who do not anticipate any change.

Only among the old does the Absolute Change relationship disappear (whereas we thought possibly that the Absolute Change relationship might *appear* only among the old); and in this case, both the No-Change and Positive Change

Table 9. Mean Potential for Political Violence Scores for Direction of Experienced Increase Gratification by Race, Age, and Education

Experienced Increase Gratification		Negative Change (-)			No Change (0)			Positive Change (+)		
N=		2.8 (129)			1.8 (71)			2.4 (264)		
By:										
Race		Black						White		
		(-)	(0)	(+)				(-)	(0)	(+)
N=		3.2 (69)	4.6 (14)	3.5 (95)				2.3 (60)	1.1 (57)	1.8 (169)
Age		18 to 33			34 to 57			58 and up		
		(-)	(0)	(+)	(-)	(0)	(+)	(-)	(0)	(+)
N=		4.2 (34)	2.1 (07)	2.8 (94)	2.8 (50)	2.6 (27)	2.3 (129)	1.7 (45)	1.2 (37)	1.7 (41)
Education		Grade School			High School			College		
		(-)	(0)	(+)	(-)	(0)	(+)	(-)	(0)	(+)
N=		2.7 (52)	1.6 (28)	2.3 (46)	2.5 (63)	2.1 (36)	2.4 (166)	4.6 (14)	1.3 (07)	2.4 (52)

groups show slightly *higher* mean PPV than the Negative Change group.

Table 9 shows mean PPV by EIG shift category and levels of the control variables. The Absolute Change or *V*-Curve relationship holds up for whites. But for blacks, instead of the negative monotonic relationship which we thought might possibly show up, the No-Change group on EIG shows a higher mean PPV than do the Negative Change and Positive Change groups. When age is controlled for, we see that the Absolute Change relationship holds up among the young and the old; but among the middle-aged the Absolute Change relationship disappears. Mean PPV appears to decrease from Negative Change to No Change to Positive Change on EIG for persons between 34 and 57 years of age; however, the differences between the means—particularly for the Negative Change and No Change groups—is quite small. Finally, when level of education is controlled for, we observe that the Absolute Change relationship holds up among persons with only a grade school education, among those with a high school education, and among the college group.

First of all, none of our expectations about how levels of the control variables might affect the *V*-Curve are borne out. We do not find a negative monotonic relationship between mean PPV and the shift categories on the Rate-of-Change variables among blacks, or among the young and middle-aged, or among persons with high school or college training. For all the Rate-of-Change variables, at all levels of all the control variables we observe only one instance of a possible negative monotonic relationship (PPV by shift category on EIG among persons aged 34 to 57); and in this instance the differences between means are not large enough to warrant confidence in a negative monotonic relationship of real importance. Second, the Absolute Change relationship holds up in every instance for the PFG variable, and in most instances for the AIG and EIG variables, regardless of levels of race, age, and education. Controlling for these attributes generally has little or no effect on the *V*-Curve.

Absolute Change as a Predictor of PPV. If we assume that PFG and AIG are interval scales, we may test two Absolute Change hypotheses:

AC': Potential for political violence will tend to decrease at a decelerated rate as change in relative gratification decreases from high negative to zero and will tend to increase at an accelerated rate as change in relative gratification increases from zero to high positive.

AC'': Potential for political violence will tend to increase as absolute magnitude of change in relative gratification increases.

The AC' hypothesis may be tested by examining whether the relationship between PPV and the rate-of-change variables fits a quadratic function rule; the AC'' hypothesis may be tested by examining whether the relationship between PPV and the absolute magnitude of rate-of-change scores fits a linear function rule. We expected that our data would support both AC' and AC''. But in examining scatterplots, we conjectured that the relationship between PPV and the rate-of-change variables would resemble a "*V*" pattern rather than a "*U*" pattern, suggesting that the data should show a closer fit to AC' than to AC'.

Table 10 shows that AC' and AC'' are confirmed for the three composite Rate-of-Change variables. The *F* values for the quadratic regressions, and for the linear regressions with absolute magnitude PFG, AIG, and EIG scores (range—0 to 30), are statistically significant at less than .01. The *r*² values, however, show that the absolute magnitude PFG scores explain twice as much of the variance in PPV as does the quadratic function on negative to positive scores; the absolute magnitude AIG scores explain one and one-half as much of the variance in PPV as the quadratic function on negative to positive AIG scores; and the absolute magnitude EIG scores explain one and two-thirds as much of the variance in PPV as the quadratic function on negative to positive EIG scores. In fact, the absolute magnitude AIG variable ($|AIG|$) explains 9 per cent of the variance in PPV. We regard this as only a moderate degree of correlation ($r = .30$) i.e., a barely moderate degree of fit to a linear relationship. (Also, if we treat $|AIG|$ and PPV as only ordinal scales, the Spearman rank-order correlation between them is .31.) The absolute magnitude of anticipated rate-of-change in relative gratification is the only achievement discrepancy variable in this sample which shows a moderate correlation with PPV.

In an earlier report Muller proposed a partial theory of potential for political violence which postulated that potential for political violence would not vary with degree of relative gratification independent of political trust and belief that political violence had been a successful method of goal attainment in the past.⁴⁵ As predicted, it was found that a composite measure of present relative gratification indeed showed no correlation with potential for political violence independent of political trust and

⁴⁵ See Muller, "A Test of a Partial Theory of Potential for Political Violence."

Table 10. Regression of Potential for Political Violence Against Composite Measures of Dynamic Relative Gratification

Regression Equation	R^2/r^2	Analysis of Variance Mean of square F			
<i>Quadratic:</i>					
PPV = .002(PFG) + .003(PFG ²) + 2.195	.03	regression	2	30.55	6.84
		residual	415	4.47	$p < .01$
PPV = .060(AIG) + .004(AIG ²) + 2.178	.06	regression	2	66.23	14.93
		residual	438	4.44	$p < .001$
PPV = -.030(EIG) + .005(EIG ²) + 2.215	.03	regression	2	33.82	7.57
		residual	461	4.47	$p < .01$
<i>Linear (Absolute Magnitude):</i>					
PPV = .086(PFG) + 1.741	.06	regression	1	113.61	26.31
		residual	416	4.32	$p < .001$
PPV = .129(AIG) + 1.810	.09	regression	1	176.45	40.82
		residual	439	4.32	$p < .001$
PPV = .100(EIG) + 1.844	.05	regression	1	109.46	25.12
		residual	462	4.36	$p < .001$

belief in the past utility of political violence.⁴⁶ Here we have found that the variable of absolute magnitude of relative gratification correlates with PPV more strongly than do any of the straightforward relative gratification variables. Our question now is: does PPV vary linearly with |AIG| regardless of political trust and belief in the past utility of political violence?

The potential for political violence model developed earlier postulates PPV as an additive linear function of Trust in Political Authorities and Efficacy of Past Violence. The Trust in Political Authorities (TPA) scale is a variable that measures generalized affect for political authorities, i.e., belief in the degree to which government officials, the police, and the courts wield power honestly, justly, and benevolently.⁴⁷ Scores were derived from the first

component of a principal components analysis of 23 variables, and range from a low of -3.16 to a high of +1.98. The Efficacy of Past Violence (EPV) scale is a summation of scores on three items measuring belief in whether the use of political violence in the past by dissident groups has been helpful to, made no difference to or hurt the cause of the group.⁴⁸ EPV ranges from a low of 0 to a high of 6.

The earlier report showed that R^2 for PPV regressed against TPA and EPV equalled .30 for 480 cases with complete data. Table 11 shows that when we take |AIG| into account the sample is reduced to 426 cases with complete data. Model 1 is the original PPV model tested across the 426 cases with complete data on all the variables including |AIG|. The R^2 of .285 for model 1 across these 426 cases is slightly lower than R^2 for model 1 across the sample of 480 cases; however, the partial regression coefficients are virtually identical, underscoring the stability of the estimated parameters for model 1.

Model 2 includes the |AIG| term as a describing variable. The F value for |AIG| in the equation is statistically significant at less

⁴⁶ See Figure 5 in Muller, "A Test of a Partial Theory of Potential for Political Violence," p. 952.

⁴⁷ See the appendix in Muller, "A Test of a Partial Theory of Potential for Political Violence," pp. 955-959. Respondents were asked to report the degree to which they agreed or disagreed with statements such as the following: "The national government is pretty much run for the benefit of all the people instead of for a few big interests looking out for themselves;" "Most of the people running the national government are crooked;" "The national government can be trusted to do what is right just about always;" "Most policemen in the United States would be willing to take a bribe;" "On the whole, the police in the United States treat everyone equally, regardless of whether they are rich or poor, white or Negro;" "The courts in the United States give everyone a fair trial, regardless of whether they are rich or poor, white or Negro;" "If the courts in the United States find someone guilty of committing a crime, the length of his sentence will depend on whether he is rich or poor, white or Negro."

⁴⁸ See the appendix in Muller, "A Test of a Partial Theory of Potential for Political Violence," pp. 955-959. The questions referred to (1) riots that have taken place in large cities; (2) white groups that have fought with the police and destroyed public and private property in the cities in order to protest against American involvement in the war in Vietnam and other things that they dislike about American society; (3) Negro groups that have urged Negroes to arm themselves in order to be ready for shoot-outs with the police.

than .01 and the estimated $|AIG|$ slope is well above its standard error. Also, with $|AIG|$ in the equation, R^2 is raised to .32: thus $|AIG|$ increases that accuracy of prediction by $3\frac{1}{2}$ percentage points. Unlike the composite present relative gratification variable ($W_p + I_p + H_p$), which the earlier report showed had no direct effect on PPV independent of TPA and EPV, $|AIG|$ clearly has some—albeit relatively slight—direct effect on PPV independent of the other describing variables. An indication of the relative importance of $|AIG|$ can be gained from the standardized partial regression coefficients (beta weights) for the describing variables in model 2 which are $-.29$ for the TPA variable, $.30$ for the EPV variable, and $.19$ for the $|AIG|$ variable. Obviously, TPA and EPV are the most important predictors for this sample, but $|AIG|$ is by no means irrelevant.⁴⁹ On the basis of our findings for model 2 we shall also add a fourth proposition to the partial theory of potential for political violence proposed earlier:

- (4) Potential for political violence will vary directly with absolute magnitude of rate-of-change over time in relative gratification, regardless of belief that political vio-

⁴⁹ The $|PFG|$ and $|EIG|$ terms also make an independent contribution, although somewhat smaller than that made by $|AIG|$. But particularly with $|PFG|$ in the equation, the number of complete data cases is quite low.

lence has led to goal attainment and regardless of diffuse support for the political authority structure.

We do not expect that any straightforward measures of degree of relative gratification, either static or dynamic, will correlate with potential for political violence, independent of belief in the past utility of political violence and political trust sentiment; but we do conjecture that the *absolute magnitude* of shift in relative gratification—particularly shift in relative gratification from present level to anticipated future level—may be a useful predictor in its own right of potential for political violence.

Conclusion

A common explanation of men's readiness to engage in acts of dissent which constitute progressively greater challenge to the state—their potential for political violence—accords major causal significance to degree of perceived discrepancy between optimum level of achievement and actual achievement with respect to important values. Actually, this achievement discrepancy hypothesis is a series of separate hypotheses, differentiated according to the definition of the concept of optimum achievement level. Tests of the achievement discrepancy hypothesis in any of its forms at the level of individual behavior have not been prolific. Most such tests have measured achievement discrep-

Table 11. Contribution of the Absolute Magnitude of Anticipated Increase Gratification to the Potential for Political Violence Model

Variables	Mean	Standard Deviation	Correlations (r)			
			PPV	TPA	EPV	$ AIG $
PPV	2.48	2.17	1.00	-.446	.441	.306
TPA	0.01	0.99		1.00	-.380	-.232
EPV	1.18	1.46			1.00	.154
$ AIG $	5.17	4.83				1.00
Number of cases = 426						

	R^2	Describing Variables	Partial Regression Coefficients*	β_0	F
(1) $PPV = \beta_0 + \beta_1 TPA + \beta_2 EPV + \mu$.285			1.935	
		TPA	-.713 (.098)		53.40
		EPV	.472 (.066)		50.92
(2) $PPV = \beta_0 + \beta_1 TPA + \beta_2 EPV + \beta_3 AIG + \mu$.320			1.511	
		TPA	-.627 (.097)		41.75
		EPV	.450 (.065)		48.29
		$ AIG $.087 (.019)		21.80

* Standard error in parenthesis.

ancy with the Cantril Self-Anchoring scale, according to which achievement optimum is defined as a person's own perceived best possible level of achievement. In this study, achievement discrepancy with respect to four universally important welfare values—work situation, income, housing accommodations and children's education—was measured by the Self-Anchoring scale for judgments made about the present, five years in the past, and five years in the future. Our findings may be summarized as follows:

(1) *Static Relative Gratification.* As degree of relative gratification experienced in the present increases, potential for political violence shows a tendency to decrease. The relationship, however, does not appear to be of a strength warranting imputation of major causal or predictive significance to relative gratification experienced in the present. Also, as degree of relative gratification expected in the future increases, potential for political violence does not show any tendency to decrease; rather, potential for political violence appears to be unrelated to relative gratification expected in the future. Both of the above findings are consistent with those reported in other studies.

(2) *Dynamic Relative Gratification.* We tested two dynamic relative gratification hypotheses: the *J-Curve*, or Rise-and-Drop, and the Rate-of-Change.

A. *J-Curve.* Individuals were categorized by six general patterns of change in relative gratification from past to present to future: Rise-and-Drop, Decreasing, No-Change Deprivation, No-Change Gratification, Increasing, and Drop-and-Rise. There is virtually no difference between proportions of respondents in the two No-Change categories who score high on potential for political violence.⁵⁰ Individuals characterized by the Rise-and-Drop pattern of relative gratification are more likely to have a high potential for political violence than those in the No-Change Deprivation group by 36.0 and 65.0 percentage points on the Income and Children variables, respectively; but individuals characterized by the Drop-and-Rise pattern of relative gratification *similarly are more likely* to have a high potential for political violence than the No Change Gratification group by 26.0, 31.3, and 57.3 percentage points on the Income, Housing, and Children variables, respec-

tively. And on the Income and Children variables, the proportions high on potential for political violence are similar for the Rise-and-Drop and Drop-and-Rise patterns, while on the Housing variable, the proportion high on potential for political violence clearly is *greater* for the Drop-and-Rise pattern than for the Rise-and-Drop pattern. Thus, the findings tend to support an *Absolute J-Curve* hypothesis, to the effect that individuals experiencing either a Rise-and-Drop or a Drop-and-Rise pattern of relative gratification will be more likely to show high potential for political violence than individuals experiencing a No-Change pattern of relative gratification.

B. *Rate-of-Change.* Three measures of direction and degree of shift over time in relative gratification were constructed: (1) past-to-future change scores were defined by subtracting level of gratification in the past from level of gratification expected in the future; (2) present-to-future change scores were defined by subtracting level of gratification in the present from level of gratification expected in the future; (3) past-to-future change scores were defined by subtracting level of gratification in the present from level of gratification in the past. These variables range from negative change through no change to positive change (−10 to +10) depending on whether, and by how much, a person perceives his level of relative gratification to be decreasing over time, constant, or increasing.

The data provide no support for the Rate-of-Change hypothesis that as degree of relative gratification increases over time, potential for political violence will tend to decrease. However, if we convert the rate-of-change scores into absolute magnitudes, the data consistently support an *Absolute Change* hypothesis that as absolute magnitude of relative gratification increases over time, potential for political violence will show a tendency to increase. Of the rate-of-change variables, the best predictor of potential for political violence is the absolute magnitude of present to future shift.

The absolute magnitude of what men think they are going to be able to get in the future minus what they have attained in the present is a variable that shows a moderate zero-order positive correlation with poten-

⁵⁰ In this respect, the Waterloo sample differs from the Cleveland sample. See Table 6 at page 198 in Bowen et al., "Deprivation, Mobility, and Orientation Toward Protest of the Urban Poor."

tial for political violence (and a higher correlation with potential for political violence than any other relative gratification variable); it also is linearly related to potential for political violence independent of degree of political trust and belief that past political violence has led to goal attainment; and it improves prediction of potential for political violence by a slight amount over and above the more powerful political trust and past efficacy of violence predictors.

Why Absolute Magnitude? We entitled this paper, "The Strange Case of . . .," because we felt like detectives while analyzing these data. On beginning the investigation we did not expect to find the Absolute Change or V-Curve relationship between potential for political violence and shift in relative gratification over time. Our first clue occurred when we categorized relative gratification shift into the fourteen patterns depicted in Figure 1 and then computed the mean PPV scores for individuals grouped into each pattern, as reported in Table 3. Initially, we expected that the V-Curve pattern would disappear when we constructed other shift variables.

But the V-Curve persisted regardless of the kind of shift variable we used, and it continued to persist regardless of the control variables we introduced. At this point we were satisfied that we could explain the zero linear correlation between potential for political violence and the shift variable (labeled Long-Term Welfare Gratification) constructed in Muller's earlier report,⁵¹ as well as the lack of linear correlation between potential for political violence and the shift variables constructed here. Moreover, our review of the relevant literature gave us reason to believe that the V-Curve would hold for samples other than this.⁵²

It is logical to expect that persons who perceive negative change in their standard of living will be more violence-prone than persons who perceive either no change or positive change. But it is not logical to expect that persons who perceive positive change in their standard of living will be more violence-prone than persons who perceive no change, and will be as violence-prone as persons who perceive negative change. The comparatively high potential for political violence manifested by persons who perceive positive change is an example of what might be labeled the "de Tocqueville Paradox," after the French social scientist Alexis de

Tocqueville, who first called attention to this phenomenon on the basis of systematic observation. De Tocqueville noted that the French Revolution occurred after two decades of steady improvement in standard of living and that those parts of France which had experienced the greatest degree of positive change also showed the greatest popular discontent.⁵³

Two different explanations of the de Tocqueville Paradox have been advanced. The most familiar is the *Rising Expectations* thesis, predicated upon the assumption that as a person experiences positive change in his level of achievement, his achievement optimum also will rise, and rise more rapidly than his attained achievement, thus producing an increase in the amount of perceived, discrepancy between achievement optimum and achievement.⁵⁴ A second explanation, labeled *Present Value of the Past*, is predicated on the assumption that, since a person's level of achievement in the present is based in part on past costs, the degree to which past costs are perceived as having been intolerable will vary proportionately with the magnitude of perceived positive change over time, resulting in the actual devaluation of utility of present achievement by an amount proportionate to the magnitude of positive change.⁵⁵ Also, we shall advance as a third alternative a *More to Lose* thesis, predicated upon the assumption that, since a person who perceives positive change has more and more to lose (until such positive change becomes stabilized over a period of time), positive change will produce a readiness to ensure that such change is maintained, by acts of dissent against the state if necessary, among persons who believe that such change could be threatened.

1. *Rising Expectations*. According to this line of reasoning, the achievement optimum of those individuals who perceive increasing relative gratification tends to rise at a faster rate than level of actual achievement.⁵⁶ For such in-

⁵³ Alexis de Tocqueville, *The Old Regime and the French Revolution* (New York: Doubleday, 1955). The paradox observed by de Tocqueville is discussed in Charles Wolf, Jr., "The Present Value of the Past," mimeo., The Rand Corporation, Santa Monica, California, 1969. Wolf cites the work of many scholars who have elaborated upon the de Tocqueville Paradox, but notes that "Although they rarely ascribe it to 'irrationality,' . . . they usually are quite unclear . . . about a precise mechanism to account for the paradox" (p. 13).

⁵⁴ See Geschwender, "Social Structure and the Negro Revolt: an Examination of Some Hypotheses."

⁵⁵ See Wolf, "The Present Value of the Past."

⁵⁶ This explanation is offered by Bowen et al., "Deprivation, Mobility and Orientation to Protest of the Urban Poor," to account for their Self-Anchoring scale findings in the Cleveland study.

⁵¹ See Table 4 in Muller, "A Test of a Partial Theory of Potential for Political Violence," p. 938.

⁵² See Bowen et al., "Deprivation, Mobility, and Orientation Toward Protest of the Urban Poor."

dividuals, the Self-Anchoring scale would not capture this increase in achievement optimum relative to actual achievement, since it does not explicitly measure shift over time in achievement optimum. Thus, since actual achievement is rising, the Self-Anchoring scale scores would show a decrease in the gap between achievement optimum and achievement; whereas, if variation in achievement optimum were also taken into account, and if achievement optimum were rising at a faster rate than actual achievement, what shows up as a positive shift on the Self-Anchoring scale might actually be a negative shift—i.e., an actual *increase* in discrepancy—on a scale which explicitly took variation in achievement optimum into account.

The problem with this thesis, for these data, is that the top of a Self-Anchoring scale is defined by a person's own perception of his achievement optimum (defined as "best possible"). And it seems warranted to expect, say, that if an individual's achievement optimum for the future is higher than his achievement optimum in the present, he will simply take this into account when estimating degree of future discrepancy on the Self-Anchoring ladder. Furthermore, since the individual is asked to estimate his past, present, and future position on the Self-Anchoring scale with respect to his conception of his "best possible" level of achievement at the time of the interview, it seems plausible that most individuals might place themselves on the ladder with respect to a relatively constant achievement optimum. No experimental evidence, however, is available on these points.

2. *Present Value of the Past.* Charles Wolf has proposed an economic model in which valuation of the past is an important term in a person's utility function. A key premise is that the aggregate value of prior achievement levels will be mediated through a "backward-looking discount rate" or *decay rate*. Further, a decrease or an increase in this decay rate may respectively increase or decrease the effect of prior achievement levels on a person's utility function. If prior achievement levels represent costs, e.g., perceived prior inequities or discrimination, and if the decay rate decreases, then the value (disutility) of past costs may become a heavily weighted argument in a person's utility function.

Wolf discusses a utility function of the form

$$U_t = U(Y_t, \tilde{V}_t),$$

where Y_t is present achievement and \tilde{V}_t is the present value of a set of prior achievement levels

which may be either costs, if $\partial U / \partial V_t < 0$, or benefits, if $\partial U / \partial V_t > 0$. The \tilde{V}_t term is defined as:

$$\tilde{V}_t = \sum_{\tau=t-1}^{t-m} (1 - r^*)^{t-\tau} V_\tau,$$

where the r^* term refers to the decay rate and the V_τ term refers to past achievement levels over a relevant time span $t-1$ to $t-m$. Now if the V_τ values are costs, raising Y may lower r^* and thus increase \tilde{V}_t . Consequently, raising Y may have offsetting effects in the utility function U_t , since raising Y raises Y_t but also lowers \tilde{V}_t . Depending on the marginal rate of substitution $(\partial U / \partial V_t) / (\partial U / \partial Y_t)$, U_t may actually fall in proportion to the amount of increase in Y . Thus, an individual who sees himself as doing better in the present may, nevertheless, lower his present utility as a result of a heightened sense of *past* injustice. Analogously, an individual who sees himself as better off in the future may, nevertheless, lower his present utility as a result of a heightened sense of *present* injustice.

3. *More to Lose:* As an alternative to the Rising Expectations and Present Value of the Past explanations, we may postulate that perception of increasing relative gratification will produce a greater readiness for political violence than perception of no change because the person who sees himself as doing better now or in the future also will desire to insure that such increased satisfaction actually is maintained or attained. And if a person believes that his gains could turn out to be ephemeral, then the desire to insure that one gets what one expects, by those who expect to have more and more to lose, will be as much an impetus to political violence as will dissatisfaction generated by perception of decreasing relative gratification on the part of those who expect to have less and less to lose. Also, as positive shift in relative gratification increases, potential for political violence might show a tendency to increase, because the greater the positive change the more desirable insuring such change becomes, if necessary by dissent behavior that constitutes progressively greater challenge to the state.

Of course, to test the More to Lose thesis, one must devise a measure of the degree to which a person believes that his present gains or future expectation of gains reflect perception of permanent or stabilized change. Such a measure was not included in the present study. But we may conjecture that, if belief in the permanence of change were taken into account, among those who perceived positive change but were not strongly convinced of the permanence

of such change, and among those who perceived negative change and were strongly convinced of the permanence of such change, the relationships between absolute magnitude of change and potential for political violence would be even stronger than they were observed to be in this study.

Clearly, future research, in conjunction with further tests of the *V*-Curve hypothesis, must explicitly investigate the exact nature of the causal mechanism—be it Rising Expectations, Present Value of the Past, More to Lose, or something else—responsible for the *V*-Curve. Also, the findings reported here call for further testing of the relationships between potential for political violence and these Achievement Discrepancy constructs, as well as the other

variants, at the micro-level. Furthermore, the Absolute Change relationship seems to have the following public policy implications. If the state is interested only in reducing the level of potential for political violence among its members, one way is to try to ensure that people do not come to expect, at any given point in time, very marked change in their standard of living, either up or down.

However, if the state is interested in pursuing policies which improve living conditions, which lead expectations about improvement in standard of living to increase substantially among some of its members, it must expect, and be ready to tolerate, a certain increase in potential for political violence.

The Relationship between Seats and Votes in Two-Party Systems*

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Arrangements for translating votes into legislative seats almost always work to benefit the party winning the largest share of the votes. That the politically rich get richer has infuriated the partisans of minority parties, encouraged those favoring majority parliamentary rule, and, finally, bemused a variety of statisticians and political scientists who have tried to develop parsimonious descriptions and explanations of the inflation of the legislative power of the victorious party.

This paper reports tests of three different models of the relationship between seats and votes against data from 132 elections in six two-party systems. One of these models, the well-known "cube law," meets these tests less well than a rather simple rule of thumb. A more important conclusion from the analysis, however, is that electoral systems differ greatly in both their rate of translation of votes into seats (the "swing ratio") and in their partisan bias. Following the descriptions of the seats-votes relationship are explanations of the observed differences in swing ratio and partisan bias. The political consequences of these differences are assessed over the years for a variety of electoral systems—and it is clear that recent reapportionments in the United States have had dramatic and unexpected effects on the translation of votes into seats. Finally, the measures of swing ratio and partisan bias appear useful for evaluating the consequences of redistricting plans, and might well be used for that purpose by the courts.

The Data and Basic Characteristics and the Relationship

Over a series of elections, the relationship between seats and votes in most two-party systems

displays four obvious characteristics (Figure 1 shows the data¹):

(1) As the party's share of the vote increases, its share of the seats also increases in a fairly regular fashion.

(2) The party that receives a majority of the votes usually receives a majority of parliamentary seats. Such was the case in 93 per cent of the national elections and 53 per cent of the state elections examined here. The points in the upper left and lower right quadrants represent those elections in which the party winning a majority of votes failed to take a majority of seats. New Jersey, like many other states prior to redistricting (and some after redistricting), shows many markedly biased outcomes, with the Democrats often winning fully three-fifths of the votes but less than one-third of the seats.

(3) A party that wins a majority of votes generally wins an even larger majority of seats. This was found in 89 per cent of the 64 national elections in which the party with a majority of votes also won parliamentary control. For the three U.S. states, the comparable figure was 83 per cent. In Rae's study of 117 elections in 20 countries (including multi-party systems), it was 91 per cent of the elections.²

(4) In most elections (100 per cent in this series), the winning party receives less than 65 per cent of the votes (although it may receive a much larger share of seats).

Even a casual inspection of the data displayed in Figure 1 indicates that almost any curve with a slope around two or three in the region from 35 to 65 per cent of the vote for a party will fit the relationships rather well. Let us now turn to three models that seek to describe the relationships

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¹ The election tabulations were collected from state and national yearbooks. The U.S. congressional returns have been collected together in Donald Stokes and Gudmund Iversen, "National Totals of Votes Cast for Democratic and Republican Candidates for the U.S. House of Representatives, 1866-1960," July, 1962, mimeo, Survey Research Center, University of Michigan. *Congressional Directories* (Washington, D.C.: United States Government Printing Office) were used to update the Stokes-Iversen compilation and also as the source for tabulations requiring election returns in individual congressional districts. All percentages of the vote were computed from the votes received by the two major parties only.

² Douglas Rae, *The Political Consequences of Electoral Laws* (New Haven: Yale University Press, 1967), p. 73.

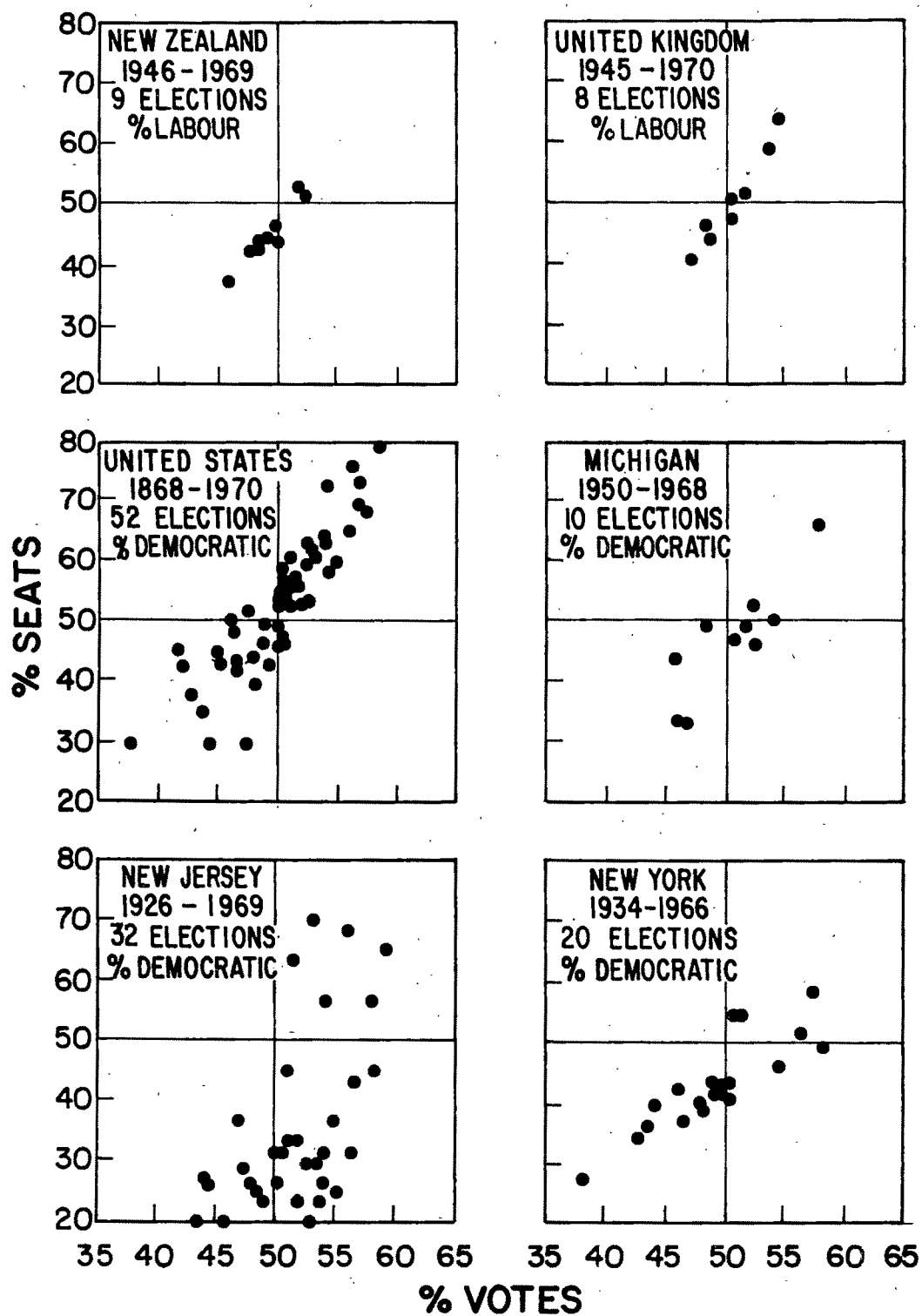


Figure 1. Seats and Votes.

more precisely: a straight line, the "cube law," and a logit fit.

The Linear Fit: Estimates of Bias and Responsiveness

The relationship between seats and votes is described most directly by a simple linear equation:

percentage seats for a particular political party
 $= b$ (percentage votes for that party) $+ c$.

The estimate of the slope, b , measures the percentage change in seats corresponding to a change of one per cent in the votes for a party.³ Thus b estimates the *swing ratio* or the *responsiveness* of the partisan composition of parliamentary bodies to changes in the partisan division of the vote in two party systems. For example, the swing ratio during the last twelve U.S. congressional elections is 1.9, indicating that a net shift of 1.0 per cent in the

³ Robert A. Dahl in *A Preface to Democratic Theory* (Chicago: University of Chicago Press, 1956), pp. 147-149, estimates the linear model for U.S. House and Senate elections.

national vote for a party has typically been associated with a net shift of 1.9 per cent in congressional seats for a party.

In addition, the fitted line provides an estimate of another important parameter of the electoral system: the bias for or against a particular party in the translation of votes into seats. Setting the percentage of seats at 50 per cent and solving for the percentage of votes in the equation of the fitted line tells one the share of the vote that a party typically needs in order to win a majority of seats in the legislative body. The difference between this number and 50 per cent is the *bias* or *party advantage*, as illustrated in Figure 2.⁴ For

⁴ Somewhat similar notions of bias have been discussed by Butler and MacRae: D. E. Butler, *The Electoral System in Britain Since 1918* (Oxford: Clarendon Press, Second ed., 1963), p. 196; and Duncan MacRae, "Models of Legislative Representation," mimeo, University of Chicago, 1969. This sort of bias can be estimated in four different ways: (1) as described in the text, where the seats-votes line is regarded as a "law"; (2) by regressing *votes on seats* and solving accordingly, (3) by the logit model described below, and (4) by manipulating the distribution of the district vote described below. A brief comparison of the four

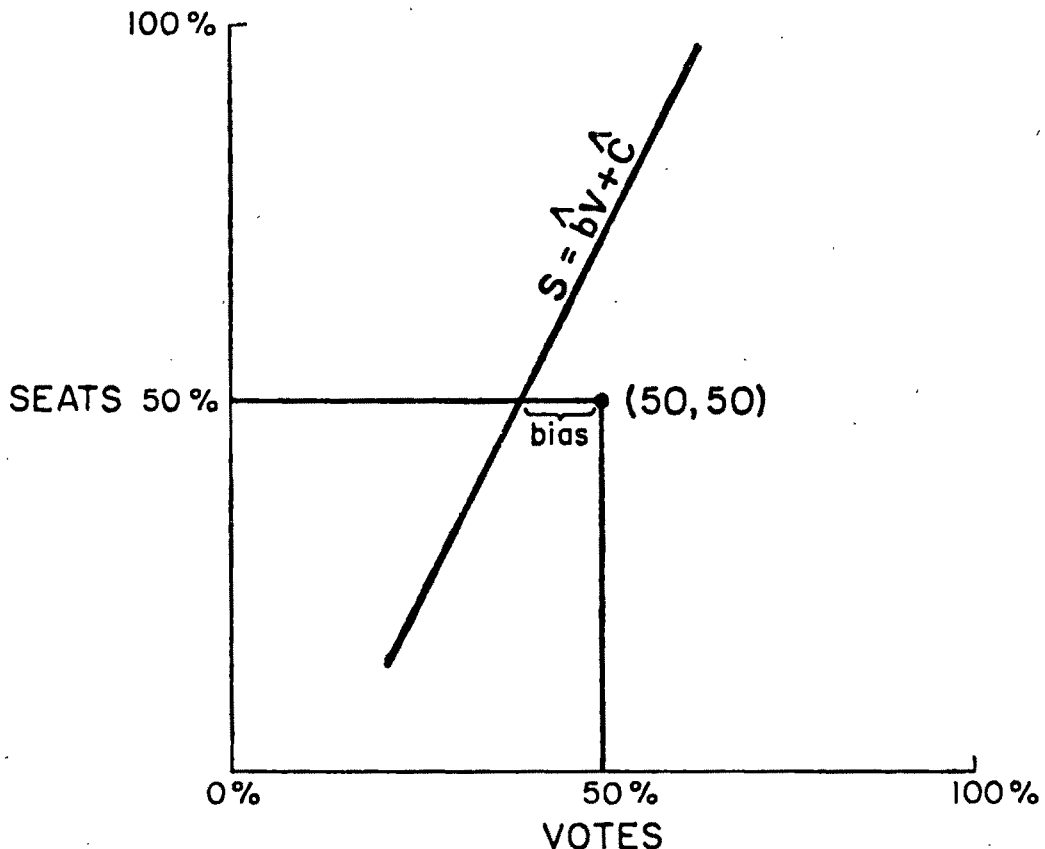


Figure 2. The Fitted Seats-Votes Line.

example, in recent congressional elections, the Democrats have typically needed only about 48 per cent of the national vote in order to win a majority of House seats; thus the bias or party advantage is about 2 per cent. Later we will explain some of the variations in the swing ratio and bias for different electoral systems over the years.

One minor defect of the linear fit is that in general the fitted line will not pass through the end points (0 per cent votes, 0 per cent seats and 100 per cent votes, 100 per cent seats), which are on the seats-votes curve by definition. Although slightly inelegant, this shortcoming is hardly troublesome—especially since parties in two party systems almost never get less than 35 per cent of the vote nor more than 65 per cent of it.

methods revealed small differences in most estimates when the bias was less than 5 per cent and the correlation between seats and votes was fairly high (usually the case); otherwise the estimates diverged. Confidence intervals for the estimated bias, if one believes they are appropriate for these data, can be constructed for the first three methods. For the complex case of method 1, see Albert H. Bowker and Gerald J. Lieberman, *Engineering Statistics* (Englewood Cliffs, New Jersey: Prentice-Hall, 1959), p. 253; for methods 2 and 3, the usual confidence intervals in regression apply. Still another interesting bias is suggested by the question: What proportion of the seats does a party receive when it wins 50% of the vote? For the fitted line, this bias is, by the geometry of the situation, the product of the slope and the bias described in Figure 2.

The clear advantage of the linear fit is that it yields two politically meaningful numbers, the swing ratio and the bias, that can be compared over time and electoral systems.

Table 1 records the fitted lines for a variety of elections. The swing ratios and the biases show considerable variation both between electoral systems and within some systems over time. Among the countries, Great Britain has the greatest swing ratio at 2.8. In the United States, the swing ratio has been about two, although, as we shall see later, there is evidence that in the last few elections, the swing ratio has decreased considerably. The U.K. electoral system shows little bias; in the U.S., a persistent bias has favored the Democratic party—partially the result of that party's victories in small congressional districts and in districts with low turnouts. In Michigan, New Jersey, and New York, there have been large biases favoring the Republicans and a great deal of variation in swing ratios. The relationship between votes and seats is weaker for the three states than for the three countries; in fact, in the states during some time periods there was virtually no correlation between the share of seats that a party won in the legislature and the share of votes it had received at the polls! In more recent elections, however, there was a fairly strong relationship between seats and votes in all three states—probably the result of new rules and practices for districting.

Table 1. Linear Fit for the Relationship Between Seats and Votes

	\hat{b} Swing ratio and (standard error)	r^2	Percentage votes required to give the indicated party a majority of seats in the legislature	Advantaged party and amount of advantage
Great Britain, 1945-1970	2.83 (0.29)	.94	50.2% Labour	Conservatives, 0.2%
New Zealand, 1946-1969	2.27 (0.27)	.91	51.4% Labour	National, 1.4%
United States, 1868-1970	2.39 (0.21)	.71	49.1% Democrats	Democrats, 0.9%
United States, 1900-1970	2.09 (0.14)	.87	48.0% Democrats	Democrats, 2.0%
United States, 1948-1970	1.93 (0.29)	.81	48.8% Democrats	Democrats, 1.2%
Michigan, 1950-1968	2.06 (0.41)	.76	52.1% Democrats	Republicans, 2.1%
New Jersey, 1926-1947	2.10 (0.44)	.53	61.3% Democrats	Republicans, 11.3%
New Jersey, 1947-1969	3.65 (0.89)	.63	52.0% Democrats	Republicans, 2.0%
New York, 1934-1966	1.28 (0.19)	.73	54.3% Democrats	Republicans, 4.3%

The fitted straight line estimates two measures of the performance of the electoral system—the responsiveness and the bias—from the outcomes of several previous elections. Confusion between the effects of swing ratio and bias sometimes occurs when only one election is considered at a time; thus one noted student of apportionment writes:

In New Jersey in 1966 the Democrats gained a 9-6 edge in congressional seats, despite a Republican plurality in the popular vote. By contrast, for the state legislature in New Jersey, using a mixture of new single- and multi-member districts, a comfortable but not overwhelming Republican plurality in popular votes in 1967 produced a sweep of two-thirds of the seats in each house. Many other examples could be given of these gross imbalances between popular votes received by a party and the number of legislators elected from that party, both before and after one man, one vote, revisions.⁵

Although there is a "gross imbalance" between votes and seats in these two cases, they differ both with respect to the causes of the imbalance and the appropriate normative evaluation of it. In the case of New Jersey congressional elections from 1964 to 1970, there was a bias of 7 per cent; the swing ratio was 1.6. In elections to the New Jersey Assembly from 1965 to 1969, there was a bias of less than one per cent but a swing ratio of 3.6. Thus, the imbalance between seats and votes in congressional elections was due to bias in the districting arrangements; in elections for the Assembly, in contrast, it was due to a steep swing ratio (a consequence of multimember districts) in what has recently become a relatively unbiased system. The two situations, then, differ: electoral systems biased toward a particular party are hardly defensible, at least in relatively democratic systems; very different swing ratios can be justified, however, depending on the objectives sought in constructing an electoral system.

Before examining the consequences and causes of differences in swing ratios and biases in detail, let us consider some alternatives to the straight line—the famous "cube law," and a logit model.

Disposing of the Cube Law

One well-known description of the relationship between votes and seats in two party systems is the "cube law," which has even become part of the political folklore of Great Britain. Although the law was originally proposed by Edgeworth and Smith near the turn of the century and then frequently discussed by David Butler in the Nuffield

series of election studies, the source most widely cited in support of the cubic relationship between seats and votes is the paper by Kendall and Stuart.⁶ The most economical statement of the law is that the cube of the vote odds equals the seat odds, where the vote odds are the ratio of the share of the votes received by one party divided by the share of the votes received by the competing party. For example, if both parties win 50 per cent of the votes, then the odds are one to one. Figure 3 shows the line traced out by the cube law.

Since Kendall and Stuart wrote, quite a number of papers have touched upon the law and, in the last few years, the law has enjoyed a certain vogue and has been fitted to electoral outcomes in England, the United States, New Zealand, and, in a modified form, in Canada.⁷ With one or two exceptions, discussions of the law are quite sympathetic, suggesting that it is a useful and accurate description of electoral realities. Most studies consider no more than a few data points and conclude that the law fits rather well—although the quality of fit is usually assessed informally and no alternative fits are tried. Let us consider a direct test of the predictions of the cube law. The law is

$$\frac{S}{1-S} = \left(\frac{V}{1-V} \right)^3$$

The ratio of shares of seats and votes won by the two parties represents the odds that a party will win a seat or a vote. Taking logarithms yields

$$\log_e \frac{S}{1-S} = 3 \log_e \frac{V}{1-V},$$

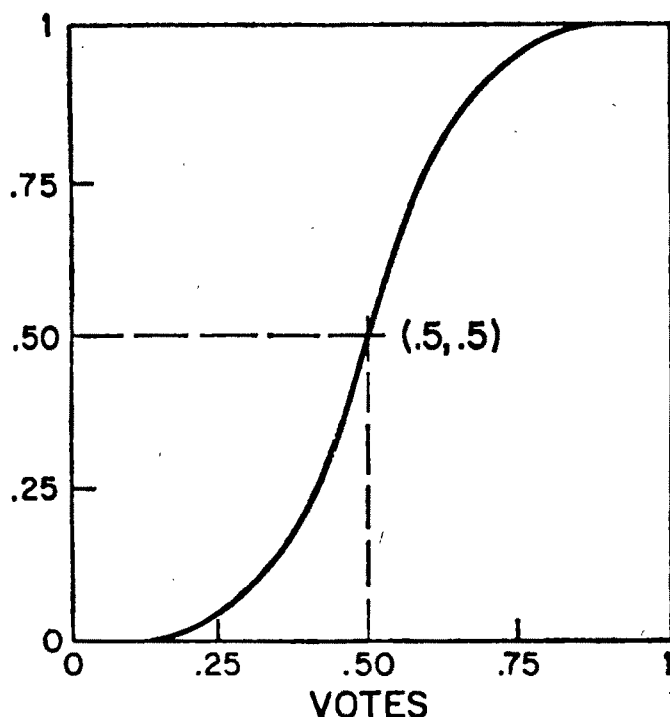
and therefore in the regression of log-odds on seats against log-odds on votes,

⁶ M. G. Kendall and A. Stuart, "The Law of Cubic Proportions in Electoral Results," *British Journal of Sociology*, 1 (September, 1950), 183-197; Kendall and Stuart, "La Loi du Cube dans les Elections Britanniques," *Revue Française de Science Politique*, 2 (April-June, 1952), 270-276.

⁷ D. E. Butler, *The British General Election of 1951* (London: Macmillan, 1952), pp. 275-276 and the other Nuffield College election studies; James G. March, "Party Legislative Representation as a Function of Election Results," *Public Opinion Quarterly*, 11 (Winter, 1957-58), 521-542; "Electoral Facts," *The Economist*, January 7, 1950, 5-7; Terrence H. Qualter, "Seats and Votes: An Application of the Cube Law to the Canadian Electoral System," *Canadian Journal of Political Science*, 1 (September, 1968), 336-344. Doubts about the cube law are expressed by Samuel J. Eldersveld, "Polling Results and Prediction Techniques in the British General Election of 1950," in James K. Pollock, *British Election Studies* (Ann Arbor: George Wahr Publishing Co., 1951), pp. 75-78; and in Ralph H. Brookes, "Legislative Representation and Party Vote in New Zealand: Reflections on the March Analysis," *Public Opinion Quarterly*, 23 (Summer, 1954), 288-291.

⁵ Robert G. Dixon, Jr., "The Court, The People, and 'One Man, One Vote,'" in *Reapportionment in the 1970s*, ed. Nelson W. Polsby (Berkeley: University of California Press, 1971), p. 13.

SEATS



$$\frac{S}{1-S} = \left(\frac{V}{1-V}\right)^3$$

$$S = \frac{V^3}{1-3V+3V^2}$$

S = proportion of seats for one party

$1-S$ = proportion of seats for the other party in a two party system

V = proportion of votes for one party

$1-V$ = proportion of votes for other party

Figure follows James G. March, "Party Representation as a Function of Election Results," *Public Opinion Quarterly*, 11 (Winter, 1957-58), 524.

Figure 3. The Cube Law.

$$\log_e \frac{S}{1-S} = \beta_0 + \beta_1 \log_e \frac{V}{1-V},$$

the cube law makes the simultaneous joint prediction that $\beta_0 = 0$ and $\beta_1 = 3$. Table 2 reports the results of tests of these predictions.

The table indicates that the cube law fits poorly in six of the seven trials. It fits quite well for the last eight elections in Great Britain, but otherwise its predictions are not confirmed. In short, it is not a "law." Since previous studies have not tested the exact joint predictions of the cube law (that is, $\beta_0 = 0$ and $\beta_1 = 3$) or used as extensive a collection of data, these results should be decisive in evaluating the empirical merits of the cube law. Even in the case of Great Britain, the linear model (and the rule of thumb replacing the cube law proposed below) fits just as well as the cube law, is simpler in form, and is politically more informative.

"But," a defender of the cube law might reply at this point, "aren't there some sound theoretical underpinnings of the cube law—and even though

it may not fit very well statistically, doesn't the law retain some merits at least as approximate quantitative theory?" First, it is not clear from any of the literature that there actually *is* a theory behind the cube law. If there is, what is it? One recent justification begins with a seemingly plausible logit model based on the notion that district swings are "harder" in legislative districts going from, say, 75 to 80 per cent (from one election to the next) than from 55 to 60 per cent.⁸ While such a logit percentage transform seems statistically natural, it is counterfactual. Percentage swings are relatively independent of the starting point and are therefore best assessed in terms of untransformed percentage differences—the so-called "paradox of swing."⁹ Other theoretical justifications do not

⁸ Henri Thiel, "The Cube Law Revisited," *Journal of the American Statistical Association*, 65 (September, 1970), 1213-1219.

⁹ David Butler and Donald Stokes discuss the issue in detail in *Political Change in Britain* (London: Macmillan, 1969), pp. 303-312.

Table 2. Testing the Predictions of the Cube Law (and Simultaneously Estimating the Logit Model)

	$\hat{\beta}_0$	$\hat{\beta}_1$	Standard error of slope	r^2	Does $\beta_0=0$ and $\beta_1=3$ as cube law predicts?	Is $\beta_0 \neq 0$; that is, is there a significant bias?
Great Britain	-.02	2.88	.30	.94	yes	no bias
New Zealand	-.12	2.31	.27	.91	no	yes, there is a bias
United States, 1868-1970	.09	2.52	.24	.68	no	yes'
United States, 1900-1970	.17	2.20	.15	.86	no	yes
Michigan	-.17	2.19	.43	.76	no	yes
New Jersey	-.77	2.09	.59	.29	no	yes
New York	-.23	1.33	.19	.74	no	yes

Note: The simultaneous joint hypothesis of the cube law was tested with the appropriate F-ratio; even the most relaxed standards ($p = .50$) did not lead to acceptance of the predictions of the cube law (with the exception of Great Britain). The statistical significance of the bias was tested by computing the t -value on the hypothesis that the intercept of the fitted line does not equal zero. A significant bias (at the .05 level or better) was found in all cases except Great Britain. It is perhaps surprising that the results of the tests are so crisp, given the relatively small number of cases. However, since the data points are rather tightly clustered about the fitted curves, the error variance is small and the results show a perhaps unexpected stability.

lead to specific derivations of the swing ratio predicted by the cube law.¹⁰

The real theoretical defect of the cube law, however, is that it hides important political issues. The law implies that the translation of votes into seats is

- (1) unvarying over place and time, and
- (2) always "fair," in the sense that the curve traced out by the law passes through the point (50 per cent votes, 50 per cent seats), and the bias is zero.

As we have seen, these implications are not true. The rate of translation of votes into seats differs greatly across political systems, ranging between gains of 1.3 to 3.7 per cent in seats for each 1.0 per cent gain in votes. Also the results in Table 1 indicate that some electoral systems persistently favor a particular party; the votes-seats curve traced out by the data does not inevitably pass close by the point (50 per cent votes, 50 per cent seats).

It might finally be argued that the cube law still has some faint merit because it is a simple, universal summary of how votes are translated into seats. It is not particularly convenient: how many students of politics can recall that the "cube of vote odds predicts the seat odds" and quickly perform the considerable mental arithmetic required to extract the number of seats predicted by the law? The following rule is a more convenient and more accurate summary of the data than the cube law:

¹⁰ David Sankoff and Koula Mellos, "The Swing Ratio and Game Theory," *American Political Science Review*, 66 (June, 1972), 551-554. For a formal model taking into account some of the findings of the present paper, see Richard E. Quandt, "A Stochastic Model of Elections in Two-Party Systems," unpublished manuscript, Princeton, 1972.

A 1.0 per cent change in votes for a party leads to a change of 2.5 per cent in seats for a party.

In other words, a rough guess of the swing ratio in two-party systems is about 2.5. This rule generates more accurate predictions for our data sets than the cube law. If one wants (perhaps for teaching purposes) a very crude rule of thumb summarizing the history of votes-seats relationship in two-party systems, then the 2.5 rule is preferable to the cube law.

An Alternative Fit: The Logit Model

A third approach to fitting a curve to the relationship between seats and votes is a logit model, which is fully as effective as the linear model and statistically more graceful. Define the odds in favor of a party's winning a seat as $S/(1-S)$ and the vote odds as $V/(1-V)$. The logit model is the regression of the logarithm of seat odds against the logarithm of vote odds (a regression used earlier to test the specific predictions of the cube law):

$$\log_e \frac{S}{1-S} = \beta_0 + \beta_1 \log_e \frac{V}{1-V}$$

Since both variables are logged, the estimate of the slope, $\hat{\beta}_1$, is the estimated elasticity of seat odds with respect to vote odds; that is, a change of one per cent in the vote odds is associated with a change of $\hat{\beta}_1$ per cent in seat odds.¹¹

The logit model has the advantage over the linear fit of producing a reasonable predicted value for the share of seats for all logically possible

¹¹ Logit analysis is described in Henri Thiel, *Principles of Econometrics* (New York: John Wiley, 1971), pp. 632-636.

values of the share of votes; the predicted values stay between 0 and 100 per cent seats for any percentage share of votes. As noted earlier, this is only a theoretical virtue since the more extreme values do not occur empirically. The logit model also provides a direct test of the hypothesis that an electoral system is unbiased, since $\beta_0 = 0$ in an unbiased system. As shown in Table 2, there is a statistically significant bias in all cases except Great Britain.

The logit model is statistically more satisfactory than the linear fit; but its coefficients are not as readily interpretable from a political point of view as are those of the linear model. Thus the straight-line fit was chosen as best suited for our purposes.

Some Explanations and Consequences of Differences in Swing Ratios

Why does the swing ratio vary over time and across electoral systems? Such differences depend, of course, on how voters are distributed over electoral districts. If voters are randomly distributed over all districts—that is, if every district is effectively like every other district—then the swing ratio would be very large, since a party that won even slightly more than half the vote would win all the seats. In effect, the whole nation would be a single member district. Now consider the other extreme in the way districts might be constructed: every voter represents herself or himself in parliament in a two-party system. Such an arrangement yields an exact equivalence of votes and seats along with a swing ratio of unity. This example suggests that the more nationally oriented the politics of a county or the more nationalized the forces prevailing in a given election, the greater the swing ratio—other things being equal.

Another line of argument suggests the same conclusion. Suppose a party gains an average of X per cent of the vote from one election to the next. Consider two different ways in which that gain could be distributed over electoral districts:

(1) The party could gain exactly X per cent of the vote in all districts; that is, a *uniform swing* of X per cent in all districts.

(2) The party could gain X per cent averaged over all districts, but, in some districts, the gain would be less than X per cent and, in other districts, the gain would exceed X per cent—yielding an *averaged swing* of X per cent over all the districts.

Under which distribution of swings, the uniform or the averaged, will the party gain the most seats? In general, a uniform swing across all districts yields more seats than the averaged swing. (Of course, if the same amount of swing were properly distributed over the marginal districts, then a party could maximize the number of seats won for a given amount of swing.) For example, com-

pare the results of a uniform swing of 1 per cent in all districts with those of an averaged swing of 1 per cent that reflects a swing of 2 per cent in half the districts and of 0 per cent in the other half. Define D_{49} as the number of districts in which the division of the vote was between 49 and 50 per cent in the previous election (and thus these districts will change hands with a 1 per cent swing favoring the initially minority party). Define D_{48} similarly. The argument now hinges on the assumption that D_{49} is greater than D_{48} (or more generally that the distribution of districts tails off as the division of vote moves away from 50 per cent). With this assumption, which is generally reasonable, a uniform swing of 1 per cent shifts a total of D_{49} seats; the averaged swing of 1 per cent (half at 2 per cent, half at 0 per cent) shifts a total of $1/2 D_{49} + 1/2 D_{48}$ seats, which is less than the gain resulting from the uniform shift, D_{49} .

Once again the argument suggests that the more uniform electoral swings are across the nation, the greater will be the swing ratio. Thus, we would expect the swing ratio in Britain to be greater than that in the United States. We would also expect that, in U.S. congressional elections, the swing ratio will be greater in on-year elections with the presidential contest on the ballot than in off-year elections (when national forces are somewhat diminished). These expectations are borne out in both cases.

Stokes, in his comparison of voting for representatives in the U.K. and U.S., has clearly shown that electoral forces are far more nationalized in parliamentary elections than they are in congressional elections.¹² The standard deviation of electoral swings from the national average has been approximately 1.8 in Britain in recent years; the comparable figure for the United States is 5.4. Thus, it is reasonable to find the swing ratio (2.83) to be greater for Britain than for the United States (1.93), as was shown earlier in Table 1.

A comparison between on-year and off-year congressional elections in the United States reveals a similar, although somewhat more fragile, result: the swing ratio is usually greater for blocs of on-year elections than for blocs of off-year elections, reflecting the presumably greater nationalization of congressional elections when they are held simultaneously with presidential elections. It is also clear from Table 3 that there are other sources of variation in the swing ratio in congressional elections (at least over time). One additional source of such variation is the competi-

¹² Donald E. Stokes, "Parties and the Nationalization of Electoral Forces," in *The American Party Systems*, ed. William N. Chambers and Walter Dean Burnham (New York: Oxford University Press, 1968), 182–202.

Table 3. Swing Ratios in On-Year and Off-Year Elections, United States, 1872-1970

	Swing Ratios	
	On-years	Off-years
1952-1970	2.1	1.7
1932-1950	3.3	3.0
1912-1930	1.9	1.1
1892-1910	2.8	3.5
1872-1890	6.8	4.0

Note: Aggregated over time, the differences are significant at the .05 level. The results are not an artifact of the particular time periods chosen; other blocs of elections show the same pattern.

tiveness of districts; the presence of many closely contested districts will lead to higher swing ratios, since small shifts in the vote will change the party control in many districts. Thus, the swing ratio is the product of both the distribution of swings and the distribution of the district party share of the votes upon which the swings are operating. Some control over the distribution of the party share of the vote is provided in the test in Table 3 by breaking the elections up into 20-year blocs. The relationship between the swing ratio and turnover in seats will be examined in greater detail shortly.

The larger swing ratio in on-year elections generally benefits the President's party, since the steeper slope in the votes-seats translation associated with on-year elections turns small gains in votes into relatively large gains in seats. In the next off-year election, however, even if the vote returns to normal, the President's party will typically lose fewer seats than it gained because it is now riding down the less steep seats-votes curve associated with off-year elections.

This observation can be stated in more formal terms. Suppose the President's party increases its share of votes in the on-year election by ΔV , thereby gaining $\beta_1(\Delta V)$ seats (where β_1 is the swing ratio in on-year elections). Let β_2 be the swing ratio in off-year elections. It is observed empirically that $\beta_1 > \beta_2$. In the next off-year election, in order for the out-party to regain those seats lost to the President's party in the previous on-year election, the out-party needs to increase its share of votes up to $\Delta V + E$, yielding $\beta_2(\Delta V + E)$ seats, where E is the excess share of votes needed to overcome the reduced swing ratio in off-year elections. Regaining all the lost seats requires that

$$\beta_1(\Delta V) = \beta_2(\Delta V + E)$$

Solving for E , the excess votes, yields:

$$E = \Delta V \left[\frac{\beta_1}{\beta_2} - 1 \right]$$

For 1952-1970, $\beta_1 = 2.0$ and $\beta_2 = 1.7$; thus

$$E = .24(\Delta V)$$

In order to regain its seats lost in the previous on-year election, the out-party needs a shift almost one-fourth greater than the shift in votes which originally won those seats for the President's party. For example, if in an on-year election the President's party gained four percentage points in votes over its previous winnings (and consequently about 8.4 per cent in seats, given the swing ratio), then the out-party would need a vote shift of $1.24 \times 4\% = 5\%$ to regain the lost seats.

Sources of Bias

The party biases computed earlier result from gerrymandering, differential turnout across districts, and the different population sizes of electoral districts. The purpose of gerrymandering is to shift the seats-votes curve and thereby produce a party advantage. A party advantage may also arise when the votes of districts with widely different turnouts or sizes are aggregated, as in the case of many formerly malapportioned state legislatures. Consider, for instance, the total votes and total seats as they are aggregated over districts. For the House of Representatives, each district adds $1/435$ to the seats total; but some districts may add a much smaller share to the total votes for each party. If, in the aggregate of all districts, low turnout or small districts are aligned with a particular party, there will be a bias in the seats-votes curve since that party is winning seats with relatively small numbers of votes.

The persistent Democratic advantage in congressional races is partially the consequence of the many low turnout districts in the South which have usually added much to the Democratic seat total, but little to the Democratic vote total. An additional source of Democratic advantage, prior to the equalization of population size of congressional districts, was the tendency for Democrats to come from smaller districts than Republicans, both in and outside the South. For the 1962 congressional elections, Table 4 indicates that Republican candidates tended to win in the larger districts; thus Republican seats were more expensive in terms of votes than Democratic seats.

The relationship between the district population size and party vote has, surprisingly, persisted even after the extensive redistricting of recent years. Although the Republicans now waste somewhat fewer votes in large districts (because of the reduction of variation in district size), there

remains, as Table 5 shows, a strong link between oversized districts and Republican dominance.

Variations in swing ratio and bias can be studied much more deeply with data from individual election districts. With disaggregated data, many different seats-votes curves can be fitted for various periods and states in the country. Indeed, for the states with more than a handful of congressional districts, any districting arrangement (proposed or actually realized) can be evaluated for responsiveness and bias.

Why the Swing Ratio Has Declined in Recent Congressional Elections: The Fruits of Redistricting

We now examine changes in the swing ratio in elections for the U.S. House of Representatives. Table 6 shows estimates of swing ratio and bias for congressional elections for the last hundred years. It appears that a shift—in fact, a rather striking shift—in the relationship between seats and votes has taken place in the last decade. The 1966–1970 triplet displays the second lowest swing ratio of the 17 election triplets since 1870. No doubt the recent elections provide a somewhat narrow range of electoral experience; the Democrats won with votes between 50.9 and 54.3

Table 4. Population Size of Congressional Districts:
1962 Congressional Elections

Population of Districts	Percentage of seats won by Republicans	Number of Districts
More than 500,000	53%	66
400,000–500,000	44%	152
300,000–400,000	40%	151
Less than 300,000	27%	32

District Size	Percentage of seats won by Republicans	Number of Districts
Outside the South		
Oversized Districts (15% or more above norm of equal population)	61%	59
Undersized Districts (15% or more below norm of equal population)	40%	53
The South		
Oversized Districts	23%	26
Undersized Districts	0%	32

Source: William B. Prendergast, "Memorandum on Congressional Districting," in *Reapportionment*, ed. Glendon Schubert (New York: Charles Scribner's Sons, 1965), p. 202.

Table 5. Population of Congressional Districts by Party Control, 1970

Population of Districts	Percentage of seats won by Republicans	Number of Districts
More than 550,000	58%	57
500,000–550,000	48%	58
450,000–500,000	47%	114
400,000–450,000	38%	133
Less than 400,000	21%	43

Source: Computed from corrected tabulations in Michael Barone, Grant Ujifusa, and Douglas Matthews, *The Almanac of American Politics* (Gambit, 1972).

per cent (a range in votes that is the fifth smallest of the 17 triplets). Until the Republicans control Congress or the Democrats win more decisively, the "new" swing ratio and bias will not be well estimated. The bias is a spectacular 7.9 per cent, reflecting the two close votes that yielded the Democrats a substantial party majority in the House. The estimate of the bias for the 1966–1970 election triplet is, however, somewhat more insecure than for previous blocs of elections because the error of the estimated bias is proportional to the reciprocal of the swing ratio—and in this case the swing ratio is moderately small.

An alternative method of determining the seats-votes curve, used by David Butler in studies of recent British elections, provides confirmation of the relatively large bias and small swing ratio. Figure 4 shows a seats-votes curve for the 1968 election constructed by taking the distribution of the vote by congressional district and watching what happens when the vote changes by ± 1 per cent, then ± 2 per cent, and so on in each district. For example, if the Democratic vote in 1968 had increased by 1 per cent across all districts, the Democrats would have gained an additional four seats; if it had decreased by 1 per cent, the party would have lost two seats. Similarly, a uniform Democratic gain of 2 per cent across all districts would be worth a total of nine seats; a loss of 2 per cent would yield a loss of six seats. (Note the low swing ratio implied by these results.) The assumption involved in the construction of this type of seats-votes curve is that there is a uniform and universal swing from the current returns—an assumption somewhat more appropriate to the U.K. than the U.S. Figure 4 shows, in the region of the actual electoral result, a relatively flat slope with a swing ratio around 1.0. And there is a relatively large bias of approximately 3 per cent. Therefore, although lacking in complete statistical security, the estimates of swing ratio and bias in

Table 6. Three Elections at a Time: Estimates of Swing Ratio and Bias

Years of elections	Swing ratio	Standard error of swing ratio	Percentage of votes to elect 50% seats for Democrats	Size of Democratic party advantage
1870-74	6.01	0.99	51.4%	-1.4%
1876-80	1.48	3.21	50.0%	0.0%
1882-86	3.30	2.83	50.8%	-0.8%
1888-92	6.01	1.36	50.9%	-0.9%
1894-98	2.82	0.09	51.7%	-1.7%
1900-04	2.23	1.09	50.1%	-0.1%
1906-10	4.21	1.38	48.8%	1.2%
1912-16	2.39	0.20	48.8%	1.2%
1918-22	1.96	0.08	47.6%	2.4%
1924-28*	-5.75*	0.36*	40.8%*	9.2%*
1930-34	2.28	0.37	45.9%	4.1%
1936-40	2.50	0.52	47.1%	2.9%
1942-46	1.90	0.60	48.1%	1.9%
1948-52	2.82	1.25	49.5%	0.5%
1954-58	2.35	0.83	50.1%	-0.1%
1960-64	1.65	0.84	47.4%	2.6%
1966-70	0.71	0.23	42.1%	7.9%

* The figures estimated for the 1924-1928 election triplet are peculiar because of the extremely narrow range of variation in the share of the vote (42.1%, 41.6%, and 42.8%) during that period. The average range within an election triplet is about 6%.

recent elections certainly seem worth taking seriously.

Compared with all the other performances of the electoral systems examined in this paper, a system with a swing ratio of 0.7 and a bias of 7.9 per cent, describes a set of electoral arrangements that is both quite unresponsive to shifts in the preferences of voters (as expressed in their party votes for their representatives) and, at the same time, badly biased. How did the low value of the swing ratio for 1966-1970 come about? Certainly the Democratic party, after their substantial gain in votes (3.4 per cent) and relatively tiny gain—given the “normal” swing ratio exceeding 2.0—in seats (3.2 per cent) would like to know what happened in 1970. And for Republicans, 1966 and 1968 need explanation: after all, they managed to make national division of the vote very close but

in neither year were they able to win even 45 per cent of the House seats.

The swing ratio indicates the potential for turnover in representation. The smaller the swing ratio, the less responsive the party distribution of seats is to shifts in the preferences of voters. The extreme case is a swing ratio near zero; such a flat seats-votes curve means that the distribution of seats does not change with the distribution of votes. Figure 5 shows the strong relationship between the swing ratio and the turnover in the House of Representatives for election triplets since 1870. Note the steady drift downward over the years in both the swing ratio and the turnover. Since 1948, the swing ratio has shifted from 2.8 to 2.4 to 1.7, and, most recently, to 0.7. Similarly the turnover in the House has declined, reflecting the long-run decrease in the intensity of competition for congressional seats. Table 7 shows the decreasing number of marginal congressional districts since 1956. In the late 1950s, about 21 per cent of House districts were relatively competitive; in 1970, 13 per cent. (Note the unchanged proportion of marginal Senate seats since 1956.) Several students of Congress have documented the situation in greater detail, and there clearly has been a large decline in competition with a concomitant increase in the tenure of incumbents in the House in recent years.¹³

Table 7. Marginal Seats: House and Senate, 1956-1970

	Proportion of seats in which the margin of victory was less than 5%	
	House	Senate
1956	20%	13%
1958	22%	11%
1966	16%	9%
1968	14%	15%
1970	13%	13%

Source: computed from vote totals in *Congressional Directories*.

¹³ See: Stokes; Nelson W. Polsby, “The Institutionalization of the U.S. House of Representatives,” *American Political Science Review*, 62 (March, 1968), 144-168; and, for a detailed discussion of congressional competition which was particularly helpful

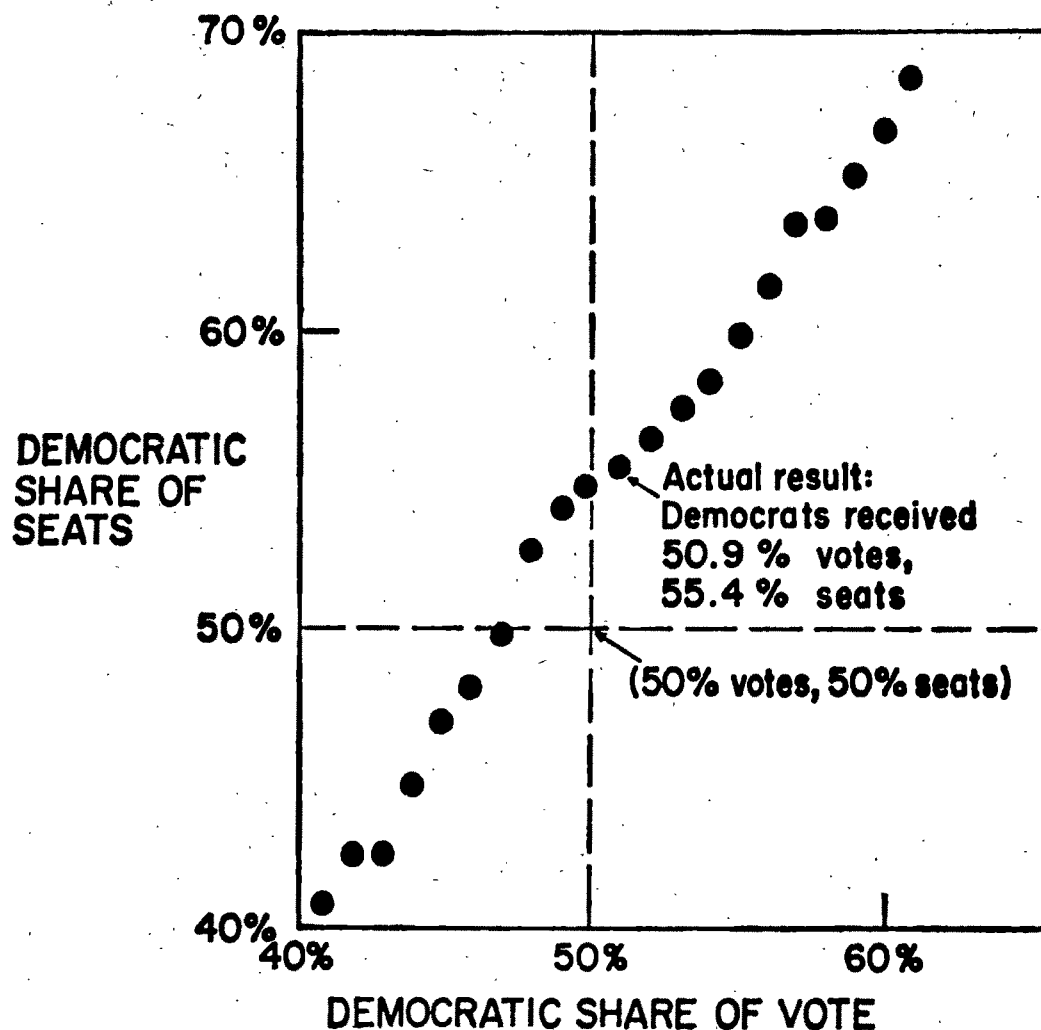


Figure 4. Seats and Votes in 1968.

Our data indicate that a major element in the job security of incumbents is their ability to exert significant control over the drawing of district boundaries; indeed, some recent redistricting laws have been described as the Incumbent Survival Acts of 1972. It is hardly surprising that legislators, like businessmen, collaborate with their nominal adversaries to eliminate dangerous competition. Ironically, reapportionment rulings have given incumbents new opportunities to construct secure districts for themselves, leading to a reduction in turnover that is, in turn, reflected in the sharply reduced swing ratio of the last few elections. One apparent consequence of reapportion-

ment is the remarkable change in the shape of the distribution of congressional votes in recent elections. Prior to 1964, the congressional vote by district was distributed the way everyone expects votes to be distributed: a big clump of relatively competitive districts in the middle, tailing off away from 50% with some peaks at the ends of the distribution for districts without an opposition candidate.

In recent elections the shape of the distribution of the vote by district has changed; Figure 7 shows the movement of district outcomes away from the danger area of 50 per cent in recent years. Note the development of bimodality in the 1968 and 1970 district vote compared to previous years (the left peak contains the Republican safe seats; the right peak contains the Democratic safe seats). Perhaps the best way to see how this pattern de-

in the present analysis, David R. Mayhew, "Congressional Representation: Theory and Practice in Drawing the Districts," in *Reapportionment in the 1970s*, ed. Nelson W. Polsby, pp. 249-290.

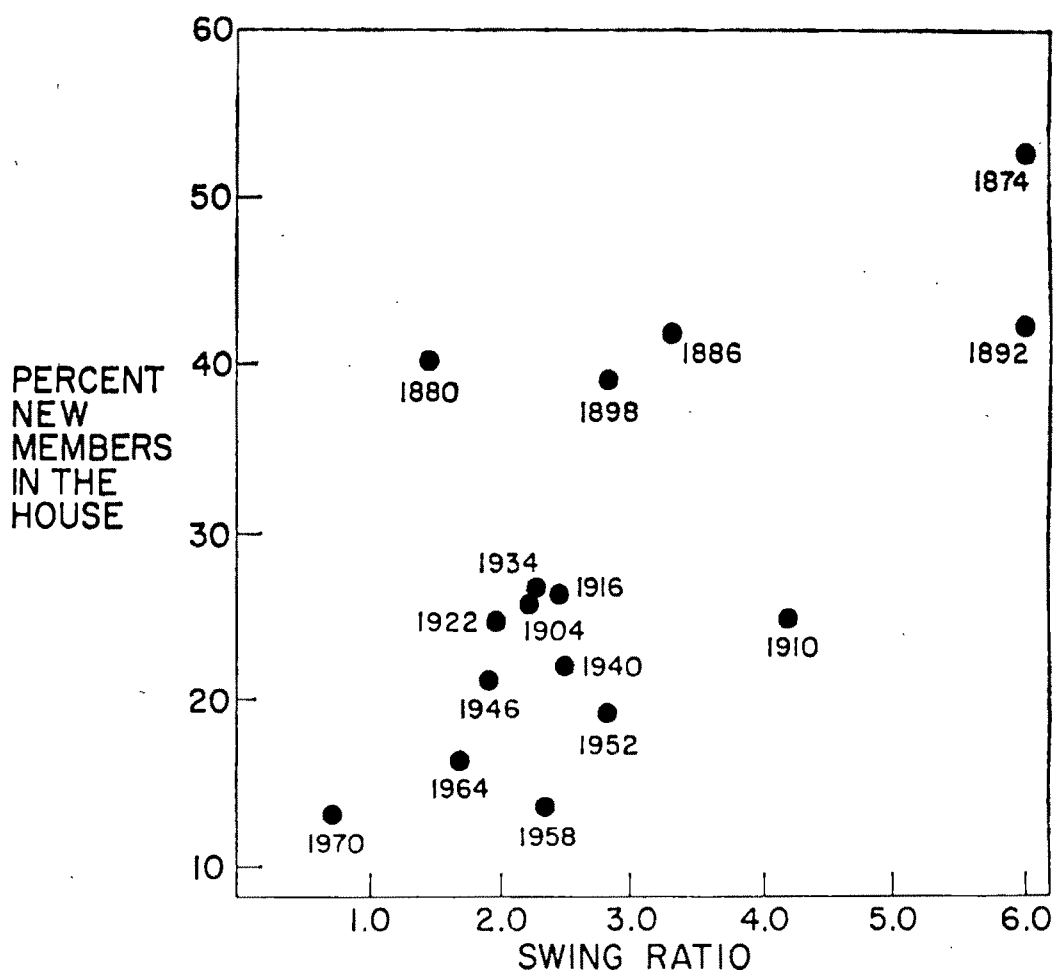


Figure 5. Turnover and Swing Ratio.

Note: The election triplets from Table 6 are used here; the date indicates the last year in the triplet. The percentage of new members is averaged over the three elections.

veloped over time is to array the vote distributions over the years and riffle through them—like an old-time peep show—and watch the middle of the distribution sag and the areas of incumbent safety bulge in the more recent elections.

Many states, through recent reapportionments,

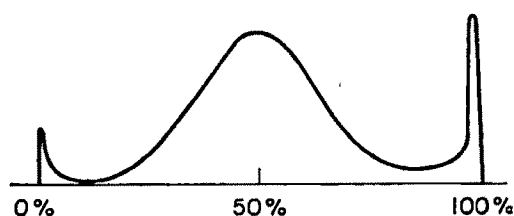


Figure 6. Democratic Share of Vote by Congressional District.

have practically eliminated political competition for congressional seats—even compared to the relatively small proportion of competitive seats in the past. In the 1970 elections in Michigan, for example, not one of the 19 districts was a close contest; the *most* marginal Republican victor won 56% of the vote and the *most* marginal Democrat won fully 70% of the vote in his district. In Illinois, the most closely contested race in all 24 congressional districts in 1970 was a 54-46 division of the vote; in contrast, in 1960, seven districts had closer races than that. The closest 1970 race in Pennsylvania was 55-45; in Ohio, 53-47.

It might be suggested that reapportionment has little to do with changes in competition; after all, the “bipartisan gerrymander,” as Mayhew aptly called it, is hardly a recent contrivance growing out of *Baker v. Carr*. Perhaps all we are seeing

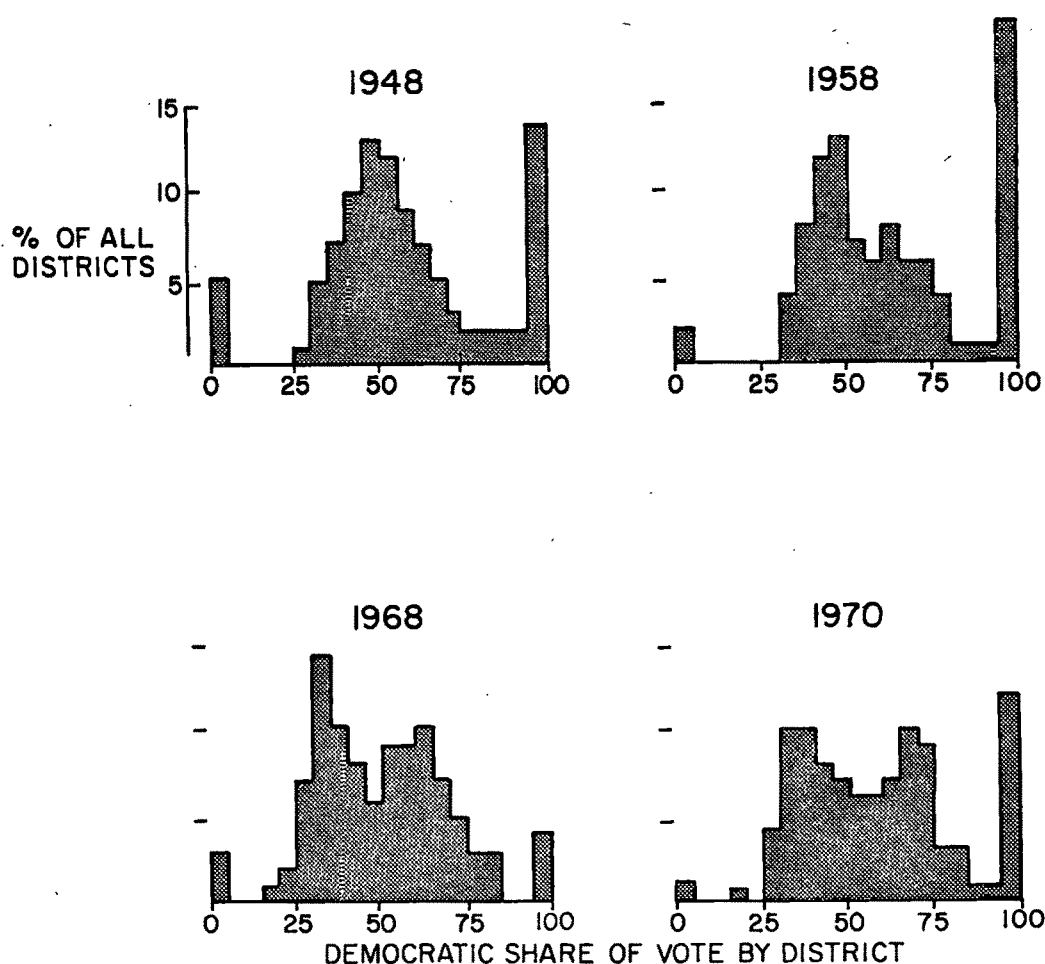


Figure 7. Distribution of Congressional Vote by District: 1948, 1958, 1968, 1970.

here is the continuation of the very long-run decline in competition for congressional seats. The evidence, however, tends rather to indicate that reapportionment has made its own independent contribution to the incumbency of incumbents. The decline in competition is not present in some contests in which the outcome is unaffected by redistricting: Table 7 shows that the marginality of Senate seats has not declined since reapportionment started. The independent contribution of reapportionment to the job security of incumbents can also be seen directly in the elections immediately following reapportionment in a state: there is an immediate decline in the competitiveness of the races in the first election after the new districting.

Concluding Practical Implications: Using the Bias and Swing Ratio to Evaluate the Fairness of Redistricting Plans

Ever since Governor Elbridge Gerry of Massachusetts designed a salamander-shaped district

some 160 years ago, politicians have closely attended to the minutiae of political cartography. Control of districting by one political party can mean many additional congressional seats in the larger states or the difference between majority and minority status in a state legislature—often remarkably independent of voters' preferences. And a "bipartisan" redistricting, as we have seen, can virtually end competition for congressional seats within a state. Redistricting should do more than fill the immediate needs of incumbents; such arrangements should pass at least two tests if an electoral system is to be minimally democratic:

(1) The districting should yield an electoral system that is *responsive* to changes in votes. If many citizens shift their votes from one party to another, then the advantaged party should win an increased share of legislative seats.

(2) The districting should be relatively *unbiased* with respect to political party; the electoral system should treat Democrats and Republicans alike.

For a state with more than a few congressional districts and for all state legislatures, it is possible, by constructing seats-votes curves (using the two methods of the previous section), to assess the bias and the responsiveness of any and all redistricting plans—and thereby judge how well a particular plan meets the two tests.

Although reapportionment eliminated some of the cruder distortions in the translations of votes into seats, we now face the problems arising from partisan and bipartisan gerrymandering. The advertised purpose of districting rules such as equal size, compactness, and contiguity was to produce “fair and effective representation.” The rules did not achieve these goals. Many redistrictings, although perfectly satisfactory by current legal standards, have produced quite biased and unresponsive electoral systems.

The conventional wisdom views reapportionment as a *district* problem; if we only make

enough rules about the construction of *individual* districts, then we will have fair and effective representation. This view is false in logic and in experience. Districting, as every politician knows, is a statewide problem; the lines of one district affect the lines of other districts. Rules specifying the desired virtues of individual districts merely add a few steps to a computer program that sorts through hundreds of statewide districting combinations; such rules cannot prevent the construction of highly biased and unresponsive electoral systems. Redistricting computer programs now seek to maximize incumbency within the constraints placed on individual districts; limits can be placed on such techniques by specifying minimum standards for the responsiveness and bias of the congressional districting in a state. Thus the achievement of an unbiased and responsive electoral system will require action based on the aggregate assessment of districting arrangements.

Regression Analysis and Discriminant Analysis: an Application of R. A. Fisher's Theorem to Data in Political Science

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Multiple regression analysis and discriminant analysis have been frequently used in political science in recent years. In accordance with the respective underlying assumptions, multiple regression analysis has been properly employed where the dependent variable can be regarded as continuous, and discriminant analysis has been appropriately used where the dependent variable can be considered dichotomous. In view of the relatively numerous variables which are involved, the applications of multiple regression analysis and discriminant analysis in political science have made the use of computers imperative. Many computer programs for multiple regression analysis are available. Of course, computer programs which independently perform discriminant analysis also are available, and—in any case—a person with experience in computer programming can write his own program for discriminant analysis. Since, however, many people find it convenient to use the readily available regression programs at computer centers, they can easily convert the results obtained under specified conditions from regression analysis to discriminant analysis. The underlying theoretical relationship and its application will be shown by presenting an example.

Assume that one wishes to examine whether or not the Supreme Court's acceptance of the fact that the defendant was not advised of his right to counsel in an involuntary confession case depends on the appearance of the fact in lower court records and appellate briefs. A similar inquiry can be made, of course, with regard to any other fact. For that purpose, the following function can be formulated:

$$Y_i = a + b_1X_{i1} + b_2X_{i2} + \dots + b_jX_{ij} + \dots + b_nX_{in} + e_i.$$

This function has to be interpreted in conjunction with Table 1. The table shows the lower court records and appellate briefs which are submitted to the Supreme Court and in which the fact under investigation may appear. If the existence of the fact is acknowledged in a record or brief j ($j = 1, 2, \dots, n$) in an involuntary confession case i ($i = 1, 2, \dots, N$) $X_{ij} = 1$, if the fact is not mentioned in that record or brief $X_{ij} = 0$, and if its existence is denied $X_{ij} = -1$. Y_i indicates the acceptance or

rejection of the fact for consideration by the Supreme Court in the particular case; its value is 1 if the fact is accepted by the Court and 0 if it is rejected. The values of the X 's and the values of Y are reported in Table 2 for 32 involuntary confession cases decided by the Supreme Court prior to *Miranda v. Arizona*.¹ From Tables 1 and 2 it can be seen that in this example $n = 6$ and $N = 32$. The values of the coefficients b as weights of the appearance, nonappearance, or denial of the fact in the various records and briefs are unknown. These values are obtained by employing multiple regression analysis. The equation, stated above as a function of Y , will be restated below as equation (1), the regression equation. The term a is a constant the value of which also is determined by the regression analysis, and e_i is an error term which measures the difference between the observed values of Y and its estimated values, obtained from the regression analysis.

The proposed model is merely tentative, however. Actually, multiple regression analysis is not appropriate for the given example, because the dependent variable Y is dichotomous (acceptance or rejection of a fact by the Supreme Court). The expected value of the dependent variable in this

¹384 U.S. 436 (1966). The case is purposely excluded from this example, because the particular fact under examination (no advice of the right to counsel) gained significance in a new context as a result of this decision. The example shows the dominant position of this fact in all the involuntary confession cases, and it contributes to the explanation of the immense importance the fact gained in the *Miranda* case and in subsequent developments.

**Table 1. Lower Court Records and Briefs
Submitted to the Supreme Court in
Involuntary Confession Cases.**

1. Allegations of the Petitioner in the Transcript of the Record.
2. Allegations of the Respondent in the Transcript of the Record.
3. Opinion of the Lower Appellate Court.
4. Dissenting Opinion of the Lower Appellate Court.
5. Petitioner's Briefs and Statements to the Supreme Court of the United States.
6. Respondent's Briefs and Statements to the Supreme Court of the United States.
7. Opinion of the Supreme Court of the United States.

Table 2. Appearance, Nonappearance, Denial, and Acceptance or Rejection of the Fact
 "No Advice of the Right to Counsel" in the Involuntary Confession Cases

Case	X_1	X_2	X_3	X_4	X_5	X_6	Y
1. <i>Brown v. Mississippi</i> , 297 U.S. 278 (1936)	1	0	0	0	0	0	1
2. <i>Chambers v. Florida</i> , 309 U.S. 227 (1940)	1	1	0	0	1	0	1
3. <i>White v. Texas</i> , 310 U.S. 530 (1940)	0	0	0	0	1	0	1
4. <i>Lisenba v. California</i> , 314 U.S. 219 (1941)	0	0	-1	1	-1	-1	0
5. <i>Ward v. Texas</i> , 316 U.S. 547 (1942)	0	0	0	0	1	0	1
6. <i>Ashcraft v. Tennessee</i> , 322 U.S. 143 (1944)	1	0	0	0	1	1	1
7. <i>Lyons v. Oklahoma</i> , 322 U.S. 596 (1944)	0	0	0	0	0	0	0
8. <i>Malinski v. New York</i> , 324 U.S. 401 (1945)	0	0	0	1	0	1	1
9. <i>Haley v. Ohio</i> , 332 U.S. 596 (1948)	1	1	0	0	1	0	1
10. <i>Watts v. Indiana</i> , 338 U.S. 49 (1949)	1	0	0	0	1	0	1
11. <i>Turner v. Pennsylvania</i> , 338 U.S. 62 (1949)	0	1	0	1	1	0	1
12. <i>Harris v. South Carolina</i> , 338 U.S. 68 (1949)	1	0	0	0	1	0	1
13. <i>Gallegos v. Nebraska</i> , 342 U.S. 55 (1951)	1	1	0	0	1	0	1
14. <i>Stroble v. California</i> , 343 U.S. 181 (1952)	1	1	1	0	1	0	1
15. <i>Brown v. Allen</i> , 344 U.S. 443 (1953)	1	-1	0	0	1	0	0
16. <i>Stein v. New York</i> , 346 U.S. 156 (1953)	0	0	0	0	0	0	0
17. <i>Cooper v. New York</i> , 346 U.S. 156 (1953)	0	0	0	0	0	0	0
18. <i>Leyra v. Denno</i> , 347 U.S. 556 (1954)	0	0	0	0	0	0	0
19. <i>Fikes v. Alabama</i> , 352 U.S. 191 (1957)	0	-1	0	0	1	-1	1
20. <i>Thomas v. Arizona</i> , 356 U.S. 390 (1958)	1	0	0	0	0	-1	0
21. <i>Payne v. Arkansas</i> , 356 U.S. 560 (1958)	0	-1	0	0	1	-1	1
22. <i>Ashdown v. Utah</i> , 357 U.S. 426 (1958)	-1	-1	-1	0	1	-1	0
23. <i>Crooker v. California</i> , 357 U.S. 433 (1958)	0	0	0	0	1	0	0
24. <i>Cicenia v. Lagay</i> , 357 U.S. 504 (1958)	0	0	0	0	1	0	1
25. <i>Spano v. New York</i> , 360 U.S. 315 (1959)	0	0	0	0	0	0	0
26. <i>Blackburn v. Alabama</i> , 361 U.S. 199 (1960)	0	0	0	0	0	0	0
27. <i>Reck v. Pate</i> , 367 U.S. 433 (1961)	0	0	1	1	1	0	1
28. <i>Culombe v. Connecticut</i> , 367 U.S. 568 (1961)	0	0	0	0	1	0	1
29. <i>Gallegos v. Colorado</i> , 370 U.S. 49 (1962)	1	-1	0	0	1	-1	0
30. <i>Lynumn v. Illinois</i> , 372 U.S. 528 (1963)	1	1	1	0	1	1	1
31. <i>Haynes v. Washington</i> , 373 U.S. 503 (1963)	0	0	0	1	1	0	1
32. <i>Escobedo v. Illinois</i> , 378 U.S. 478 (1964)	1	0	0	0	1	1	1

instance would have to be in the interval (0, 1), regardless of the values of the independent variables X . This condition is not necessarily compatible with the assumption that the expected value is a linear combination of the independent variables. Furthermore, the assumption in multiple regression analysis that the distribution of the dependent variable around its expected value is independent of the level of that value does not apply to a dichotomous dependent variable.²

In view of these considerations, a method appropriate for dichotomous dependent variables, namely, discriminant analysis, must be employed. The object of the method is to find values for a function

$$Z_i = c_1X_{i1} + c_2X_{i2} + \dots + c_jX_{ij} + \dots + c_nX_{in},$$

which—depending on whether the numerical value

of Z_i is above or below a certain point—will place each observation (case in this example) in one of two groups (acceptance or rejection of the fact under consideration by the Supreme Court). X_{ij} has the same meaning in this equation as in the regression equation. The values of the coefficients c (corresponding to the b 's in the regression equation) as weights of the appearance, nonappearance, or denial of the fact under consideration in the lower court records and appellate briefs are unknown. They are obtained as a result of discriminant analysis.³ The stated equation, which will be restated below as equation (2), is the discriminant function. It will be seen that all the statistics of discriminant analysis can be obtained from regression analysis under the specified conditions.

Before explaining and demonstrating the rela-

² See James Tobin, "The Application of Multivariate Probit Analysis to Economic Survey Data," *Cowles Foundation Discussion Paper No. 1* (July 14, 1955, as revised December 1, 1955), p. 2.

³ For a concise but complete presentation of multiple regression and discriminant analysis, with a good mathematical exposition, see Gerhard Tintner, *Econometrics* (New York: John Wiley & Sons, Inc., 1952), pp. 83-102.

tionship between regression analysis and discriminant analysis, a brief reference to another example is desirable. The considerations advanced in the preceding paragraphs also would apply, e.g., to examining the vote for President Nixon in the 1972 election as a function—hypothetically speaking—of such variables as age, education, income, etc. The dependent variable—voting or not voting for President Nixon—would be a dichotomous dependent variable, and consequently discriminant analysis rather than multiple regression analysis would be appropriate. Since data for such a study will be available from the Inter-University Consortium for Political Research, at the Center for Political Studies (University of Michigan), the conversion of regression analysis to discriminant analysis could be effectively demonstrated as applied to this example. With more than 2200 observations in the sample for the 1972 election, however, it would be difficult to reproduce all data in this research note, so that the reader can readily replicate the entire procedure. For this reason, a very small sample—consisting of 32 observations and 6 independent variables—purposely was chosen for the illustration presented here.

The theoretical relationship between multiple regression analysis and discriminant analysis, which provides the basis for converting the former to the latter, can be stated in the form of the following theorem: Let b_j ($j=1, 2, \dots, n$) be a coefficient in the regression equation

$$Y_i = a + b_1X_{i1} + b_2X_{i2} + \dots + b_jX_{ij} + \dots + b_nX_{in} + e_i, \quad (1)$$

$Y=0, 1$, for an observation i ($i=1, 2, \dots, N$). Let c_j ($j=1, 2, \dots, n$) be a coefficient in the discriminant function

$$Z_i = c_1X_{i1} + c_2X_{i2} + \dots + c_jX_{ij} + \dots + c_nX_{in}, \quad (2)$$

where every observation for which Y in the corresponding regression equation is 0 is placed in Group 1 and every observation for which $Y=1$ in Group 2. Then

$$c_j = b_j \frac{N}{N_1N_2}, \quad (3)$$

where N is the total number of observations, N_1 the number of observations in Group 1, and N_2 the number of observations in Group 2.

The relationship expressed by this theorem was originally stated by R. A. Fisher.⁴ An outline of

a proof of the theorem by the author of this paper, which is different from the one developed by Fisher, is given in the appendix to this presentation.

The similarity between equations (1) and (2) in the theorem can be readily noted. The right side of both equations states linear combinations of the independent variables X and their weights b and c , respectively. In the example under consideration, the following meaning can be attributed to the equations: Depending on the assertion of the presence of the fact under consideration, the absence of any reference to it, or its denial in the lower court records and appellate briefs (the values of X) and the degree of influence of such assertions on the Supreme Court (the weights b or c), the Court will accept the fact as having been present in some cases and reject it in others [the values of Y in equation (1)]. However, in equation (2), which represents the discriminant function, Z should not be regarded as a dependent variable with *observed* values like Y . It is merely an index, which—depending on whether its numerical value is above or below a certain point—places each observation (case in this example) in one of two groups (acceptance or rejection of the fact under consideration). The values of this index are obtained by substituting the values of c , obtained as a result of the discriminant analysis, in equation (2); the values of the X 's, of course, are originally known.

On the basis of the stated theorem, discriminant analysis can be performed by first employing multiple regression analysis (for which computer programs are readily available), with values of 0 for Y if an observation belongs in one group (e.g., rejection of the fact by the Supreme Court in the given example) and 1 if it belongs in the other group (e.g., acceptance of the fact by the Court). Accordingly, by applying regression analysis to the example under consideration, with the values for the independent variables X and the temporarily used dependent variable Y in Table 2, one obtains the values of the weights of the independent variables, b , which are reported in Table 3. By substituting these values and the applicable values of the X 's in equation (1), excluding the term e , one computes the *estimated* values of Y , which are presented in Table 4.

The conversion to discriminant analysis now can be easily achieved. By inspection of Table 2, it can be readily seen that $N_1=12$ and $N_2=20$. By substituting these values, together with the known

⁴See R. A. Fisher, "The Statistical Utilization of Multiple Measurements," *Annals of Eugenics*, 8 (1938), 376–386, and *Statistical Methods for Research Workers*, 13th ed. (New York: Hafner Publishing Com-

pany, Inc., 1958), p. 286. The notation used here differs from that employed by Fisher. For another study which compares regression analysis and discriminant analysis, see George W. Ladd, "Linear Probability Functions and Discriminant Functions," *Econometrica*, 34 (1966), 873–885.

Table 3. Regression Coefficients and Discriminant Function Coefficients

Record or Brief Identified by Number from Table 1	$a = .176$	
	b	c
1	.120	.016
2	.162	.022
3	.000	.000
4	.379	.051
5	.529	.071
6	.130	.017

value of $N=32$, and the values obtained from the regression analysis in equation (3), one can compute the values of c , which also are reported in Table 3. The next step is the substitution of the originally known values of the independent variables X and the computed values of their weights c in equation (2). In this fashion, the values of the discriminant function Z , which also are in Table 4, are obtained for all the observations.

The value of Z which separates the values of Z for the two groups (acceptance or rejection of the fact by the Supreme Court) is .051. It is obtained by taking the mean of the respective means of the values of Z in the two groups. In comparing the values of Z in Table 4 with the corresponding values of Y in Table 2 (0 indicating Group 1 and 1 indicating Group 2), in relation to the value of Z which separates the two groups, it will be noted that the values of Z do not place every observation in the proper group. There are a few observations with $Y=0$ which have a value of Z above .051 and a few observations with $Y=1$ which have a value of Z below .051. Nevertheless, the multiple squared correlation coefficient $R^2=.558$ and $F=5.254$ (both of which can be computed from the results of the multiple regression analysis with values of 0 and 1 for observed Y). For the applicable degrees of freedom, the obtained value of F exceeds the one which corresponds to the .01 level of significance; i.e., in terms of probability by chance alone, $P(5.254 \leq F) < .01$.⁵ The results of the discriminant analysis, therefore, can be regarded as significant. It also can be shown that the value of R^2 increases as other independent variables, such as the acceptance or rejection of other facts by the Court, are included in the analysis. This aspect of the research, however, is

⁵ The F -test was presented by R. A. Fisher as a test for significance for discriminant analysis (see note 4), with reference to Hotelling's generalized Student distribution (see Harold Hotelling, "The Generalization of Student's Ratio," *Annals of Mathematical Statistics*, 2 [1931], 360-378). In this connection, Fisher also introduced the applicability of R^2 to discriminant analysis (see the proof outlined in the Appendix).

substantive; whereas the main purpose of this presentation has been methodological.

Appendix

Outline of the proof of the theorem stating the relationship between regression analysis and discriminant analysis:

For $Y=0, 1, y=Y-\bar{Y}=-N_2/N$ if $Y=0$ and $y=N_1/N$ if $Y=1$. By substituting these values in

$$\sum_{i=1}^N x_{ij}y_i(x_{ij} = X_{ij} - \bar{X}_j, i = 1, 2, \dots,$$

$$N, j = 1, 2, \dots, n)$$

on the right side in the normal equations for regression analysis $[(x'x)b=x'y]$ in matrix and vector form], one obtains

$$\sum_{i=1}^N x_{ij}y_i = \frac{N_1N_2}{N} d_j, \quad (i)$$

Table 4. Estimated Values of Y and Discriminant Function Values Z

Cases Identified by Number from Table 2	Estimated Y	Discriminant Function Value Z
1	.296	.016
2	.988	.108
3	.706	.071
4	-.103	-.037
5	.706	.071
6	.956	.104
7	.176	.000
8	.686	.068
9	.988	.108
10	.826	.087
11	1.247	.143
12	.826	.087
13	.988	.108
14	.988	.108
15	.664	.065
16	.176	.000
17	.176	.000
18	.176	.000
19	.413	.032
20	.166	-.001
21	.413	.032
22	.294	.016
23	.706	.071
24	.706	.071
25	.176	.000
26	.176	.000
27	1.085	.121
28	.706	.071
29	.533	.048
30	1.112	.126
31	1.085	.121
32	.956	.104

where

$$d_j = \sum_{i=N_1+1}^{N_2} x_{ij}/N_2 - \sum_{i=1}^{N_1} x_{ij}/N_1.$$

The normal equations for discriminant analysis $[(x'x)c=d]$ are similar to the normal equations for multiple regression. The sums of products on the left side are identical in both systems of equations, the respective notations b and c for the unknowns are arbitrarily selected, and the right side of the normal equations in discriminant analysis consists of d_j instead of the term $\sum x_{ij}y_i$ in the multiple regression normal equations.

By Cramer's rule, the normal equations for discriminant analysis can be solved for

$$c_j = \frac{D_j}{D}, \quad (\text{ii})$$

where D is the determinant of the coefficients of the unknowns and D_j is obtained by replacing the coefficients of c_j in the applicable column of D with d_1, d_2, \dots, d_n . Correspondingly, by substituting $(N_1N_2/N)d_j$ from (i) for $\sum x_{ij}y_i$ in the normal equations for multiple regression, the solution

$$b_j = \frac{N_1N_2}{N} \frac{D_j}{D} \quad (\text{iii})$$

is obtained, since the multiplication of each element in a column of a determinant by a constant

$(d_1, d_2, \dots, d_n$ by N_1N_2/N) is equivalent to the multiplication of the whole determinant by that constant. By substituting from (iii) in (ii) one obtains

$$c_j = \frac{N}{N_1N_2} b_j. \quad (\text{iv})$$

Thus the theorem is proved.

Furthermore, it can be shown that for discriminant analysis

$$R^2 = \frac{N_1N_2}{N} (c_1d_1 + c_2d_2 + \dots + c_jd_j + \dots + c_nd_n). \quad (\text{v})$$

For multiple regression,

$$R^2 = \beta_1r_{yx_1} + \beta_2r_{yx_2} + \dots + \beta_nr_{yx_n},$$

where the β 's are the standardized regression coefficients and the r 's the product moment correlation coefficients. By substituting for the β 's the b 's and the applicable standard deviations, and for the r 's the sums of products of the applicable variables with $Y=0, 1$, by substituting then

$$\frac{N_1N_2}{N} d_j \quad \text{for} \quad \sum x_{ij}y_i$$

from (i), and by simplifying the expression, one obtains (v).

A Research Note on Machine Politics as a Model for Change in a Philippine Province*

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For several years political scientists have been concerned with the nature of the ties between leaders and subleaders on the local level. For instance, Robert Dahl noted that in New Haven "... the relations between leaders, subleaders and constituents produce in the distribution of influence a stubborn and pervasive ambiguity that permeates the entire political system."¹ This pervasive ambiguity is no less relevant on the subnational level in countries of the Third World. For instance, in the Philippines there has been a growing concern about local political leadership and government since World War II.² However, political scientists who have sought to explicate the dynamics of the leader-subleader linkage patterns in Filipino politics have been hampered due to the lack of an adequate leadership framework for analysis.³

*I am indebted to the East-West Center of the University of Hawaii which made the field research in the Philippines possible. The data used in this article were gathered in a Southern Luzon province during 1970 for my dissertation at the University of Hawaii entitled "Political Leadership Through Political Leaders: A New Approach for the Analysis of Philippine Provincial Leadership Positions."

¹Robert Dahl, *Who Governs?* (New Haven: Yale University Press, 1961), p. 102. Arthur J. Vidich and Joseph Bensman, *Small Town in Mass Society* (Princeton: Princeton University Press, 1958), devote a section of their book to the study of linkages in electoral politics from the local to the state and national levels. One of the best studies of subnational urban politics in America is Samuel Eldersveld, *Political Parties: A Behavioral Analysis* (Chicago: Rand McNally, 1964).

²In the Philippines, interest in local government has been reflected in the research done by the Community Development Research Council (CDRC) and more recently the Local Government Center. For an interesting discussion of thinking on local development see Fred W. Riggs, *Administration in Developing Countries: The Theory of Prismatic Society* (Boston: Houghton Mifflin, 1964), pp. 365-396, and Jose Abueva and Raul de Guzman, *Foundations and Dynamics of Filipino Government and Politics* (Manila: Bookmark, 1969).

³Most of the accounts of subnational politics in the Philippines have been solely descriptive, such as Jose V. Abueva, *Focus on the Barrio Citizens in Iloilo as They Affect Community Development* (Quezon City: Community Development Research Council (CDRC), University of the Philippines, 1960). A series of four monographs published in 1968 from the Local Government Center of the University of the Philippines, College of Public Administration (Tito Fimalino and Nextor Pilar, *The 1963 Local Elections in Aklan*; Gabriel Iglesia and Elena Gazboa, *The 1963 Local Elections in Lanao del Norte*; Aprodicio Laquian and Roberto Pangilian, *The 1963 Local Elections in Manila*; and

Those who have tried to apply American models have run into difficulties both conceptually and operationally. Mary Hollnsteiner, who employed a series of items from the Schulze and Blumberg community-power questionnaire, noted that she

became increasingly aware that something was wrong with the questions. The responses were so unconvincingly given, and clarification was so often sought that finally discounting that particular series of interviews seemed to be the only reasonable thing to do.⁴

Although Hollnsteiner was unsure whether it was the culture or the technique that was the source of the poor reaction, others who have studied Philippine politics warn:

... when working with the common man in the Philippines, it is not enough to take American instruments, such as attitude questionnaires, and merely translate them into the dialect. In using these instruments you are using American conceptualizations which have little relevance...⁵

And further:

However well adapted these models may be to the analysis of their own society's problems, they are ill-adapted to the needs of a prismatic, poly-communal and intelligentsia-dominated society.⁶

Recently James Scott suggested that machine politics might prove to be a useful context within which to develop a framework for the analysis of subnational politics in Third World

Romeo and Estrella Ocampo, *The 1963 Local Elections in Davao*) dealing with local elections and political leader recruitment in four localities still lacked a coherent framework since each author pursued his own interest.

⁴Mary Hollnsteiner, *The Dynamics of Power in a Philippine Municipality* (Quezon City: CDRC, University of the Philippines, 1963), p. 163. Robert Schulze and Leonard Blumberg reported their findings in Schulze and Blumberg, "The Determination of Local Power Elites," *American Journal of Sociology*, 63 (November, 1957), 290-296.

⁵Charles K. Warriner, "The Prospects for a Philippine Sociology," *Philippine Sociological Review*, 9 (January-April, 1961), 12-18; quotation on p. 17.

⁶Fred W. Riggs, "A Model for the Study of Philippine Social Structure," *Philippine Sociological Review*, 7 (December, 1959), 1-32; quotation on p. 18. Recently a similar precaution was voiced by Dante C. Simbulan, "On Models and Developing Societies," *Asian Studies*, 6 (December, 1968), 421-430.

countries.⁷ The purpose of this study is to determine whether the machine model is just another Western conceptualization, or whether it has practical value at the local level in the Philippines.⁸

The Machine Politics Model

Machine politics occurs predominantly in the early stages of an electoral system such as eighteenth-century England and the nineteenth- and early twentieth-century United States.⁹ The political bosses in the systems had to win elections in order to retain control of their machines, and they would rely primarily on particularistic rewards to ensure the loyalty of an otherwise fickle and poor electorate.¹⁰ The political setting within which machine politics occurs is also plagued by a fragmentation of power resulting in the development of multiple centers of power. The social system in which machine politics flourishes is typically in a state of flux characterized by social disorganization and poverty.¹¹

According to Scott, this phenomenon has recurred with variations in a number of developing nations, including the Philippines. Drawing on the English and American experiences, Scott suggests three preconditions for the existence of machine politics:

- (1) selection of political leaders through elections,

⁷ James Scott, "Corruption, Machine Politics, and Political Development," *American Political Science Review*, 63 (December, 1969), 1142-1158.

⁸ The research for this study was conducted in a rural Philippine province of Southern Luzon during the first half of 1970. The province had eleven municipalities and was a lone congressional district. The economy was dependent on timber and agriculture, and the literacy rate in the province was the highest outside Manila.

⁹ Machine politics in Britain has been characterized eloquently by Sir Ivor Jennings, *Party Politics, Volume I, Appeal to the People* (London: Cambridge Press, 1960); and Robert Walcott, Jr. *English Politics in the Early Eighteenth Century* (Cambridge, Mass.: Harvard University Press, 1956). Machine politics in the United States has been described by social scientists like Edward Banfield and James Q. Wilson, *City Politics* (Cambridge: Harvard and M.I.T. Press, 1963); and Harold Zink, *City Bosses in the United States* (Durham: Duke University Press, 1930); and by practitioners like Edward Flynn, *You're The Boss* (New York: Viking Press, 1947). Machine politics was noted at the local level even in Japan after the Meiji Reformation by Richard Beardsley, John Hall, and Robert Ward, *Village Japan* (Chicago: University of Chicago Press, 1960), Chapters 12 and 13.

¹⁰ Interesting and humorous examples of offering particularistic rewards such as jobs, money, and lodging can be found in Lloyd Wendt and Herman Kogan, *Bosses in Lusty Chicago: The Story of Bathhouse John and Hinky Dink* (Bloomington: Indiana University Press, 1967).

¹¹ Scott, pp. 1149-1150.

- (2) the presence of universal suffrage, and
- (3) a high degree of electoral competition over time.¹²

In Scott's framework, as an electoral political system changes, the incentives used by the political leaders to maintain a following adjust to the new conditions. Based on the components of support used by the political leaders, there are three phases through which a political system passes over time. It should be noted, however, that no one phase is exclusive of the others, and that movement from one phase to another is neither rigid nor deterministic.

During *Phase A*, a leader maintains his vertical ties with subordinates through traditional patterns of deference such as family alliances. The inducements are symbolic without material or ideological substance. In *Phase B*, vertical ties are sustained only through concrete, short-run material inducements. Machine politics based on a process of bartering for votes flourishes during Phase B. In the final stage, *Phase C*, subordinates' loyalties to leaders are based on ideological or policy concerns.¹³

Since these three phases are posited to occur simultaneously or independently, for purposes of operationalization, they might be better characterized as three independent dimensions. As such, factor analysis could provide a means by which independence between the phases could be established. Each support phase would essentially become a dimension in a three-dimensional space in which participants in the subnational political system could be located on the basis of their scores on the independent dimensions of support. In addition, the scores could also be used in comparing different systems as well as tracing changes in an electoral

¹² Scott, p. 1143. These criteria were used in the selection of the province studied. For instance in the province no governor or congressman was ever reelected except for the incumbent congressman in the 1969 election. One political subleader referred to the province as basically an oppositionist province that would not reelect office holders so as to give other people a chance to hold office.

¹³ Scott, pp. 1142-1146.

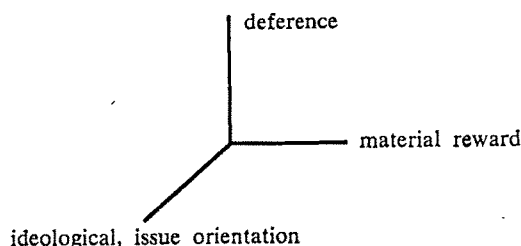


Figure 1. Dimensions of support

political system over time. Borrowing from the three-phase scheme, the dimensions of support would be labeled deference, material reward, and ideological or issue orientation.

The Political Subleader and the Machine

An important aspect of machine politics is the hierarchy of influence that exists within the machine, and in order to operationalize the machine model, one must consider the hierarchy. In most studies of machine politics the precinct captains or subleaders are shown to be liaisons between the top leaders and the constituents.¹⁴ In some cases "the function of the subleader . . . is to 'go along' loyally and thus provide a suitable facade for the actions of the party leaders."¹⁵ Others contend that the subleaders are not nearly so meek, that the subleaders are favor buyers

. . . who wish a party to act in some way which benefits them and will in return influence voters to support that party. Favor buyers claim to represent no one except themselves; they are merely engaged in trading their influence over voters for specific acts they want done.¹⁶

Favor buyers are intermediaries in peddling influence and goods (votes). They are bargainers who swap their influence over voters for preferential treatment for themselves and their constituents. Given the subleaders' importance, then, it would seem that the motivations of subleaders of the machine system who manipulate voters would be as important as studying the motivations of the individual voters.

The Philippine equivalent of the precinct captain or subleader is referred to as a "lider" (pronounced like leader).¹⁷ The *lider* is an actor in the political system who has a following and pledges himself to a candidate in return for favors which he distributes to his followers. A candidate for office calls him "his lider" referring to the subordinate position which the *lider* plays vis-à-vis the leader. Within each Philippine province there is usually a hierarchy of *liders*. At the lowest level one *lider* influences or controls ten to fifteen voters, and at the

highest level a *lider* influences several thousand voters. The Filipino *liders* maintain their ties with constituents and lower *liders* by using social and material favors that are not unlike the favors used in early England or America by subleaders operating within a machine context. These favors include being present at baptisms, weddings, and wakes, releasing constituents from jail, and arranging special treatment for constituents in their dealings with government officials.¹⁸ Supposedly, the performance of social favors is intended to reinforce strong feelings of debts of gratitude or, as known in the Philippines, *utang na loob*.¹⁹ In addition to social favors, family connections and fictive kinship or *compadre* relationships are also considered important means of mobilizing support.²⁰

Although studies have attempted to describe the motivations of the Filipino voters, the nature of the patron-client associations between the leaders and *liders* has been only marginally investigated, and even among the voter studies there are divergent interpretations of why an individual voter selects one leader rather than another.²¹ Yet, given at least the structural

¹⁴ See Walcott and Zink.

¹⁵ Charles Kaut, "Utang na Loob: A System of Contractual Obligations among the Tagalogs," *Southwestern Journal of Anthropology*, 17 (Autumn, 1961), 256-272. The concept of "face" is also involved in *utang na loob*. It is considered disgraceful not to honor one's *utang na loob*. See Hollnsteiner, p. 77 and Jean Grossholtz, *The Politics of the Philippines: A Country Study* (Boston: Little, Brown, 1964), pp. 86-91, 95-100, and 187-188. The notion of repaying personal favors is the basis of *utang na loob*, and it actually is not peculiar to the Philippines. Others have commented on the importance of debts of gratitude or reciprocity. For instance, William Gamson, *Power and Discontent* (Homewood, Ill.: The Dorsey Press, 1968), p. 78, states: "Many politicians are eager to create obligations to themselves by doing small favors for others; such obligations can be converted into specific inducements at election time or when a crisis occurs." Also Peter Blau, *Exchange and Power in Social Life* (New York: Wiley, 1964), pp. 16-21, points out that in social intercourse reciprocity is usually expected.

²⁰ Generally, fictive kinship or *compadrazgo* ties take the form of a wealthy patron's sponsoring a poor client. In the Philippines *compadre* ties are made by a wealthy individual's acting as the godparent of a client's child in a baptism or wedding. Hollnsteiner, pp. 63-67; Grossholtz; and Carl Landé, *Leaders, Factions and Parties: The Structure of Philippine Politics* (New Haven: Yale University Press, 1965), pp. 14-23 and 133-148. For a comparative discussion of *compadrazgo* ties see John Powell, "Peasant Society and Clientelist Politics," *American Political Science Review*, 64 (June, 1970), 411-425.

²¹ One group contends that it is personal loyalty which has the greatest effect: Robert Youngblood, "A Study of the 1963 Mayoralty Election in Jolo, Philippines," Unpublished Master's Thesis, Honolulu,

¹⁴ References to the intermediaries are made by authors from Geoffrey Holmes, *British Politics in the Age of Anne* (London: MacMillan, 1967), to Nancy Joan Weiss, *Charles Francis Murphy, 1858-1924: Respectability and Responsibility in Tammany Politics* (Northampton, Mass.: Smith College, 1968).

¹⁵ Dahl, *Who Governs?*, p. 108.

¹⁶ Anthony Downs, *An Economic Theory of Democracy* (New York: Harper, 1957), p. 88.

¹⁷ Hollnsteiner, p. 41. Another useful description of *liders* is made in a monograph by Buenaventura Villanueva, "Municipal Government and Politics," (Laguna, Philippines: University of the Philippines, College of Agriculture, June, 1962).

framework necessary for machine politics, it can be argued that "control rests with the local political leaders (*liders*) who can deliver the vote in the barrios."²² The Filipino voter follows the dictates of his *lider* to whom he looks for guidance and from whom, in some cases, he receives sustenance. Therefore, research evaluating the motivations of the *liders* rather than those of the voters differs from studies of voter behavior and attitudes in that the *liders* do not cast anonymous ballots: they are conspicuous in their support by actually campaigning for a leader. Since the *liders* are considered to control or at least influence a number of voters, a *lider's* support is worth considerably more than a single vote. Consequently, the subleader in the rural setting of the Philippines is instrumental in determining the type of incentive used by the leaders.

It would be logical to hypothesize that as long as the political system is dominated by boss politics the nature of the leader-*lider* (subleader) linkages would depend on particularistic rewards. According to Scott's framework, as a country develops economically and moves toward the ideological support dimension, the subleaders' concern for obtaining particularistic rewards would decrease, since these rewards would be of less value to their constituents. For instance, in the United States the role of the boss and the precinct captain as a "ward heeler" for the individual voter diminished with the advent of such programs as the social welfare system.²³ As the voters placed less emphasis on particularistic rewards, the subleaders lost control over blocs of votes, because the material rewards that they could obtain through their connections with a leader were no longer relevant to the voter.

Application of the Model

Components of Support in the Philippine Setting. On the basis of the discussion above, we might well conclude that the machine political model would be viable where the other Western models were not, but that in operationalizing

the model, special attention would have to be given to the Philippine setting. From what has been written about the nature of Philippine politics and the various appeals that are used to gain support, the general consensus is that support of a candidate is based on one or more of ten components: (1) *kinship ties*; (2) *compadrazgo ties*; (3) *utang na loob* (debts of gratitude); (4) *party ties*; offering reward in the form of (5) *positions*, (6) *projects* or *money*; (7) *making threats*; (8) *the capability* of a candidate; (9) *the issues* of a candidate; and (10) *a candidate's chance of winning*.²⁴ These components of support have additional relevance for Philippine studies. Many of the writings during the early 1960s stressed the importance of the family and *compadrazgo* ties for political leaders, but it has been suggested recently that Philippine politicians have re-evaluated their orientations, and as a result the Philippines is moving away from Phase A (authority through deference and reliance on traditional ties) to Phase B (reliance on particularistic rewards).²⁵ The components of support model was designed to test that assumption, as well as to determine the degree to which machine politics was present.

The Sample and Questionnaire. In a Philippine province the congressman and governor are considered to be the top political leaders. Beneath them is a hierarchy of *liders* reaching down into the barrios. For this study a list of high level provincial leaders (*liders*) was obtained from the Congressman, the provincial Governor, and their major opponents in the last election (1969, 1967 respectively). From the list of 187 high-level *liders* compiled from the names submitted by the four provincial leaders, 105 political *liders* were randomly selected and interviewed. For the components of support phase of the interview, the *liders* were first asked an open-ended question about why other political *liders* whom they personally

1966, pp. 87-90; and Remigio Agpalo, "The Political Elite and the People: A Study of Politics in Occidental Mindoro," Unpublished Manuscript, Quezon City, 1966. A second group places importance on capability and issues: Eufemio Patanne, "Political Opinion," Jose Abueva and Raul de Guzman, eds., *The Foundations and Dynamics of Filipino Politics*, pp. 114-118; and H. Averch, F. Denton, and J. Koehler, *A Crisis of Ambiguity: Political and Economic Development in the Philippines* (Santa Monica: Rand, 1970), pp. 60, 77.

²² Grossholtz, p. 152.

²³ Scott, pp. 1156-1157; and Banfield and Wilson, pp. 121-125.

²⁴ It is generally considered that Filipino politics is not based on ideology since politics is highly personalized. According to Agpalo and Villanueva, political support depends on family pattern, status and position, and self-interests. Landé, pp. 141-143, suggests that politics is based on dyadic relationships between patrons and clients. Other authorities such as Hollnsteiner and Grossholtz also emphasize social reciprocity.

²⁵ As an example, Jose Abueva, "Conditions of Administrative Development: Exploring Administrative Culture and Behavior in the Philippines," CAG Occasional Paper (Bloomington: December, 1966), points out that once they were away from their home province, the administrative personnel in Manila have tended to disregard their traditional family and *compadre* ties.

Table 1. Rotated Factor Matrix of Scale Values for Components of Support

Components of Support	Factors						h ²
	I	II	III	IV	V	VI	
Capability	0.93	-0.16	-0.03	0.07	-0.03	0.09	0.91
Issues	0.93	-0.01	-0.10	0.07	-0.16	0.05	0.90
Family Ties	-0.08	0.86	0.14	0.05	-0.09	0.10	0.79
<i>Compadre</i> Ties	-0.12	0.86	0.06	0.04	0.00	0.13	0.77
<i>Utang na Loob</i>	0.07	0.57	0.10	0.40	0.41	0.01	0.66
Projects and Money	0.07	0.13	0.94	-0.03	-0.05	-0.09	0.91
Positions and Jobs	-0.39	0.13	0.77	0.24	-0.06	0.17	0.84
Threats	0.10	0.11	0.09	0.95	0.06	0.10	0.93
Party	0.14	0.11	-0.03	-0.03	-0.94	0.13	0.93
Chance of Winning	0.12	0.20	0.01	0.01	-0.13	0.95	0.98
Total Variance	19.5	19.1	15.1	11.3	11.1	10.0	86.1
Common Variance	22.7	22.2	17.6	13.1	12.8	11.6	100.0

knew, would support one political candidate over another.²⁶ All of the *liders*' answers were covered by the ten components cited above. After the open-ended question, the *liders* were asked to rate each of the ten reasons why "other *liders* support a leader" on a seven point scale from "extremely important" (7) to "not important at all" (1).²⁷

Results

The Phases as Factors. The data were first used to test the three-phase model. The scores from the scale ratings were applied in a principal components factor analysis with the varimax solution. Three factors were rotated to test the hypothesis that the three phases outlined by Scott could be represented by three statistically independent dimensions. The three-factor rotation yielded a low total percentage of common variance explained; and the communalities (per cent of explained variance for a variable) for party (40 per cent), chance of winning (38 per cent), and threats (37 per cent) were so low

²⁶ The questions which yielded the most realistic responses were designed so as to have the respondents answer about other *liders*. When questions which asked the *liders* about their own views were administered, the *liders* would give what they considered "correct" or proper answers. In several instances, *liders* who were influenced by money or jobs denied that money or jobs were important to them personally. Therefore, sixty-one *liders* were asked questions about others as well as questions about themselves. The results reported here are based on responses from questions about others.

²⁷ By not using a rank ordering, the *liders* were better able to give tied scores for some variables if they chose to do so. Although the questionnaire was administered in English, those who completed the schedule demonstrated an understanding of the questions and the scale by elaborating on their numerical scoring with examples.

that it was obvious that additional factors were involved in the pattern. Another factor analysis was run using the "scree" criterion, and six factors were rotated.²⁸

For the rotated six-factor solution, the three factors which accounted for most of the variance corresponded closely to the three-factor model described above. A candidate's capability and his stand on issues loaded highly (.93 and .93) together on the first factor. This factor might be labeled the ideological or issue-oriented dimension. Obviously it matches Phase C of Scott's scheme. The second factor had high loadings for family ties (.86), *compadre* ties (.86), and *utang na loob* or debts of gratitude (.57). All of these ties suggest the traditional deference that was discussed earlier and is found in several Third World countries.²⁹ On the basis of what was said by the *liders*, however, personal loyalty, seemed a more fitting description of the dimension than did deference.³⁰ Projects, money, and jobs composed the third factor: particularistic reward. This finding was interesting since other authorities cited above assumed that personal loyalty was tied to material reward. Yet in the factor analysis, *utang na loob* was shown to be an independent factor in

²⁸ Raymond B. Cattell, "Scree Test for the Number of Factors," *Multivariate Behavior Research*, 1 (1966), 245-276.

²⁹ Powell.

³⁰ When the *liders* elaborated after giving a score for the different variables, they referred to family ties, *compadre* ties, and *utang na loob* differently than Scott's term, "deference," suggests. The *liders* defined these variables as personal bonds between themselves and others, either superordinates or subordinates. Deference implies the obedience given to only a superior. See Scott, "Corruption, Machine Politics, and Political Development," pp. 1146-1147.

spite of Western thinking that it was strictly part of a Filipino *quid pro quo*.

Although these three factors correspond to the three-dimensional model described above, the three dimensions account for only 62.5 per cent of the common variance and 54 per cent of the total variance. The rest of the common variance is taken up by threats (13.1 per cent), party loyalty (12.8 per cent), and chance of winning (11.6 per cent).

Since the results of the factor analysis verify the independence of the three dimensions, a model could be developed that would represent the three phases by three independent dimensions: personal loyalty, particularistic reward, and capability-issue orientation. In considering the three-dimensional model for analysis, however, it must be understood that in this study the three dimensions explain only a little more than half of the variance in a *lider's* motivation to support a leader. Obviously, with this kind of restriction the model would only be marginally useful even as a purely heuristic device.

Evaluation by the Liders. At the analytical, model-building level, the results of the questionnaire did not substantiate the three-dimensional model, or even the three-phase model. On simply a descriptive level, however, the questionnaire was shown to be more than just another Western instrument. The relative weight given to different bases of support used in the Philippines provided valuable insights into the *liders'* motivations and upset other commonly held assumptions.

The *liders* consistently rated the particularistic rewards above the other incentives for supporting a political leader. The offering of positions or jobs was considered first in importance by almost all of the *liders* interviewed. For example, one *lider* had been in the Liberal Party (LP) and had supported the Liberal candidate for congress, but when the Congress-

man, a Nacionalista, offered to make him the chairman of the Nacionalista Party (NP) in the *lider's* municipality, the *lider* switched parties. Another *lider*, a cousin of the Congressman's opponent, switched to support the Congressman. The *lider* brought with him the votes of approximately eleven hundred followers, and for his efforts the *lider* was appointed to a judgeship through the recommendations of the Congressman.

Rated closely behind jobs were projects and money. Since the *liders* have to sustain their own *liders* and constituents, money and projects are important in the maintenance of their political positions. One mayor showed me photographs of himself standing with Presidents Quirino (LP), Magsaysay (NP), Macapagal (LP), and Marcos (NP), as well as with three congressmen, two Liberals and one Nacionalista. He said that his party was always the same party as that of the incumbent president and congressman. Asked why, he replied that if he were not a supporter of the incumbents, he would not have been able to bring improvements to his municipality, and as a consequence, he would not have been able to serve as mayor for the past twenty years.

These attitudes are characteristic of a system of machine politics, and the *liders'* responses to the questionnaire further substantiate the hypothesis that a boss system as described earlier operates in this rural province. The more traditional elements, however, such as personal loyalty, family ties, and *utang na loob*, are also present. The relatively high scores given to family and *utang na loob* would be expected on the basis of what has been written about the nature of the Philippine social system.³¹ The real surprise was that *compadrazgo* ties were less emphasized than earlier writings suggest. As was noted above, *compadrazgo* ties were earlier said to have been essential social and political ties. Apparently extrafamilial connections have now become so extended as to be meaningless for many of the *liders*. In some cases the *liders* would have so many *compadres* and clients that they could not remember them all. In fact, one young councilman flatly rejected the *compadrazgo* system as a waste. He pointed out that he had no *compadrazgo* clients, yet he had managed to finish first in a sixteen-man field for eight council seats. Nevertheless, the other two personal loyalty elements are still considered as strong influences on a *lider* in his decision on whom to support.

The importance placed on the chance of winning of the candidates demonstrates the extent

Table 2. Mean Scores* for *Liders'* Evaluations of Components of Support

Positions and Jobs	5.98
Projects and Money	5.59
<i>Utang na Loob</i>	5.57
Party	5.48
Family	5.46
Chance of Winning	5.43
<i>Compadre</i> Ties	4.69
Capability	4.56
Issues	4.36
Threats	3.34

* Range 1-7.

³¹ See notes 19 and 24.

to which the *liders* were favor buyers and the extent to which they operated as economic decision makers.³² Although favor buyers are part of the machine system, the chance of winning was given less importance than it might have otherwise received. I found that strong endorsements and prior commitments undermined a *lider's* willingness to switch away from a losing candidate. The *liders* had a strong sense of *hiya* (face). Once a pledge was given during a campaign the *liders* would remain steadfast in their support even if their candidate was going to lose, unless sufficient money, projects, or jobs were used as incentives to switch.

Capability as a criterion for either the Governor (perceived by the *liders* to mean administrative ability, competence, and expertise) or the Congressman (speaking, legislative, and representative competence) was rejected by the *liders*. Political issues such as foreign policy stands, law and order, and coherent national planning also had little importance for the *liders*.

The adherence to the party and blind support of the party's candidate are rather rare in the Philippines where party switching is extremely common.³³ Yet in the province studied there were a few *liders* who contended that party loyalty was, in some cases, the only criterion used in determining whom they would support. Other *liders*, in discussing party loyalty, referred to party ties in personal terms, i.e., loyalty to one or another of the two top provincial leaders who headed the two parties, but these ties fell under the general heading of *utang na loob* and not party loyalty.

Summary and Conclusion

Selecting a province in the Philippines which closely matched the criteria outlined by Scott for the existence of boss politics—elected leaders, universal suffrage, and high electoral competition—made it possible to test the practicality of the machine model for interpreting the leader-subleader linkage patterns in Filipino politics. Since the political subleaders have played such a large political role in a machine

system, the local *liders* were used as the means to characterize the provincial support structure.

The *liders* were found to be an integral part of the provincial political system which operates much like the political machines of early America and early England. Discussions with the *liders* and the tabulated results from the questionnaire confirmed that the individual voter actually mattered little to the top provincial leaders since individual votes were usually bought and sold by a *lider* to whom the voter gave allegiance.

Beyond this point the model of machine politics breaks down as just another Western conceptualization. The Philippines plainly is not like the United States or Great Britain. Descriptively, the model was helpful, but for providing an analytic basis for predicting change, it is sadly lacking. The conditions which existed in Britain and the United States do not exist in the Philippines. For instance, there is not a strong party system in the Philippines to reinforce political alliances.

The most important difference between the Western states and the Philippines is that in countries like England and the United States, when the social system grew increasingly complex, the resource base and the existence of colonies or a frontier enabled their economies to develop and expand. Also, when social welfare became a government enterprise, the governments were financially sound enough to take over many of the roles previously handled by the political machines. In both cases the politics moved from particularistic reward to issue orientation. Neither of the above economic conditions exists in the Philippines, and even though the Philippine government is committed to social welfare, the government is simply not economically strong enough to cope with all the demands that have previously been met unofficially by the *liders*. As for the machine politics model, its critical fault is that it does not provide us with insights into changes that occur in the political system when the numbers of people who expect rewards from the *liders* increases and the ability of the *liders* to supply rewards decreases.

The unanswered questions now facing political scientists, and, more acutely, Philippine politicians, are (1) When will it become economically impossible for political leaders to pay off supporters with money and jobs; and (2) When it does become impossible, what will leaders use to maintain their support structures and influence patterns?

³² Downs.

³³ Landé, pp. 48–69. In Philippine politics, political leaders switch from one party to another depending on which party can accommodate them the best. For instance President Ferdinand Marcos was the leader of the Liberal Party in the Senate before Liberal President Macapagal decided to run for reelection in 1965. Marcos was invited to join the Nacionalista Party as the party's presidential nominee.

COMMUNICATIONS

TO THE EDITOR:

A colleague recently expressed to me a proposition which he felt would characterize American presidential politics in coming years. The proposition is this: "After the first term of an incumbent President, the opposition party will nominate a more extreme candidate than after the second term, when no incumbent is running." His reasoning was that no candidate with appeal to the broad center of American voters would run against an incumbent candidate and thus stand a high probability of losing, since he can run four years hence against a non-incumbent with a much higher probability of winning. He cited Goldwater's nomination as the first example consistent with his proposition, and he believed that McGovern's nomination would be the second example. (At that time, the Democratic primaries were in progress.) In the McGovern case, the obvious best example of the potential candidate with broad appeal but not motivated to run is Edward Kennedy.

More recently, political commentators have noted more than once during the Democratic convention and since that the Democratic party was not behaving like a party intent on winning. This suggests actions somewhat beyond those underlying the above proposition. That is, not only the motivations and actions of potential candidates for the presidency, but the motivations and actions of those in the party involved in shaping the platform and directions of the party—the party activists. It suggests that these motivations and actions are not so much directed to winning the election as to taking positions consistent with their beliefs. This would broaden the proposition above to include the platform, somewhat like this: "After the first term of an incumbent President, the opposition party will have a more extreme platform and will nominate a more extreme candidate than after the second term, when no incumbent is running."

This may come to be a new property of American politics which differentiates "mid-incumbency" elections from non-incumbency elections, following the two-term limitation on the presidency, just as the midterm congressional elections are different from the presidential election years. If so, it will be a particular case of a more general principle that has long been noted in politics. This principle is that minority parties with a small chance of winning are more "principled," adhering more to a fixed

set of beliefs, than are those parties in serious contention for winning the election.

In this communication, I would like to show that such behavior can be predicted from a very parsimonious rational model in which party activists, whether in a seriously contending party or in a noncontending minority party, are attempting to maximize utility. What is particularly interesting is that in terms of this model, the same motivation leads to the opposite behavior in the two cases. The model has been discussed in detail in two publications, where the interested reader may pursue the ideas further.¹ Here I will merely give a verbal discussion. The general idea is this: Those engaged in selecting a party nominee or shaping a party platform, whether voters in a primary, more active party workers, or delegates to a nominating convention, choose, if they are rational, on the basis of how that choice will affect their satisfaction with the government they can expect to have following the general election. They do not choose merely on the basis of their satisfaction with the candidates' positions; because if the candidate with those positions they most agree to has no chance of winning, then by choosing him they will end up worse off—with the opposition party candidate elected—than if they had chosen a candidate with whose positions they agreed less, but who has a high chance of winning. More precisely, they want to maximize their expected utility from the new government, and the expected utility from a particular primary candidate with a given position is the *product* of the probability that a candidate with this position will win the general election and the utility that would be experienced if he won.

Now suppose each party tentatively takes a position at the median of the position of party voters-and-activists in that party. Then, the party of the left, by moving toward the center, would take a position that is less satisfactory to the majority of its voters-and-activists than that median position. (I use "party voters-and-activists" here, rather than "party voters" to reflect the fact that party's nominee is not selected by a simple process of voting by a defined set of party members, but by a complex process in

¹James S. Coleman, "Internal Processes Governing Party Positions in Elections," *Public Choice*, II (Fall, 1971), 35-60, and James S. Coleman, "The Positions of Political Parties in Elections," in *Probability Models of Collective Decision Making*, ed. Richard G. Niemi and Herbert F. Weisberg (Columbus, Ohio: Charles E. Merrill, 1972).

which the work of the activists helps determine how many and which persons will be party voters.) But if by moving to the center it can gain enough probability of winning the general election to counterbalance the loss in utility if it wins, then all party voters-and-activists will gain in expected utility, even those to the left of the party median. Thus, if such a condition holds, the party will take a position closer to the center than its median, since that position will maximize expected utility from the general election for the majority of party voters-and-activists.

If this condition does not hold, that is, if the gain in probability of winning the general election does not counterbalance the loss in utility if it wins, the party's optimal position will be at the median of its voters-and-activists, and there will be no motivation to move toward the center. Thus depending on whether this condition holds, the party will behave very differently as a result of maximization of utility by party voters-and-activists: either moving toward the center, or taking a principled stand. The question then becomes: When can the condition be expected to hold?

Consider first the set of positions that parties can take, which may be thought of as a line (a left-right continuum of positions), a plane, or a space of higher dimensions. Then the probability of their party's winning the general election as a function of the position it takes is some probability distribution over these positions. It should take roughly the form of a cumulative normal distribution which depends upon the distribution of the population of voters in the general election, and on the positions of the other parties. We can think of this distribution in a two-party election: assume that the party's position is only one of several considerations that lead voters to vote for it. Then there will be a certain nonzero probability of winning if the party stays at the position of the median of party voters-and-activists. By moving toward the center, the party can increase that probability to some maximum, depending on the other party's position and *a priori* strength, but a maximum less than 1.

In a normal, relatively balanced two-party election, the gain in probability of winning as a function of going from the party voters-and-activists median to the position that maximizes the probability of winning, assuming that the other party goes to its optimal position, is a gain from near zero to about 0.5. In moving to that point, with each move there will be a loss in expected utility owing to reduction of utility

given a win, and a gain owing to increase in probability of winning. Assuming that the sum of the gains due to the latter outweigh at some point some of the losses due to the former, there will be an optimal position neither at maximum probability of winning, nor at maximum utility given a win (the party voters-and-activists median), but where the product of the two is maximum.

In a normal, balanced two-party election, this will produce a pair of equilibrium positions so that both parties will have about a probability of 0.5 of winning.

But in an unbalanced election such as an incumbent election, where one party has a relatively high *a priori* probability of winning, the minority party, in compromising its position to the same degree, goes from something near zero to something considerably less than 0.5, given the stronger party's optimal position. Thus the party voters-and-activists have lost the same amount in potential utility, if elected, as in the balanced case, but have gained much less in probability of winning. It is quite possible, in fact, that there may be no position, as it moves toward the center, in which the gains in expected utility owing to increase in probability of winning exceed the losses owing to decreased utility given a win, compared to the original point, the median. If that is so, then there is no incentive to move toward the center. This lack of incentive derives from the small potential gain in probability of winning, owing to the other party's *a priori* strong position.

An examination of the strong party's incentives shows an analogous situation. Because its probability of winning is high at the outset, it cannot gain much in the probability of winning by moving toward the center, and thus there may be no point other than its original point at which the gains in expected utility owing to increased probability of winning outweigh the losses owing to decreased utility given a win.

Thus, the result is this: that as the *a priori* difference in strength of the parties is greater, whether because of incumbency of a candidate or for another reason, there is less likely to be an incentive on the part of voters-and-activists in either the strong or the weak party to shift the party's position from the median toward the center. What this implies for elections in which there is an incumbent, assuming that incumbency gives *a priori* strength, is that in those elections neither the incumbent party's candidate and platform nor the opposition party's candidate and platform are as likely to show movement toward the center, as are both parties'

candidates and platforms in an election without an incumbent.

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TO THE EDITOR:

The following was originally sent as a memorandum to members of the M.I.T. community in October, 1972.

The Gamalian plurality in the late plebiscite was so huge that contemplation of it has distracted the public attention from all subsidiary phenomena. One gapes at it as a yokel gapes at a blood-sweating hippopotamus; its astounding vastness makes it seem somehow indecent, as a very fat man always seems somehow indecent.

—H. L. Mencken, "Optimistic Note,"
Nov. 29, 1920

Being the careful sort, I have been unwilling up to now to predict the outcome of the 1972 presidential election. To add my voice to the universal agreement that we will indeed have "four more years" seems almost a waste of your time and mine. If the unanimous testimony of the polls is even remotely in the right end of the ballpark, President Nixon will win a popular majority which—if it is comparable to anything at all—equals only such immensities as 1920, 1924, and 1936: it is, in fact, possible to make projections from some of the *northern* state polls which, linked to voting behavior in 1968 and earlier, yield literally incredible national estimates. It is always possible, of course, to construct a hypergeometric curve on time from now until the election which would make this election reasonably close. But this seems pure wish-fulfillment at this stage; and I, for one am now willing to throw in the towel.

At the same time, it is at least a useful exercise in self-control to analyze this "blood-sweating hippopotamus" in its context, and make a first crude attempt to unravel its meaning. There are a variety of expository modes available to those who write about American elections. This time I shall do the job in the mode of "axiomatic" statements about American politics generally and as applied to this case (of course, without "hard data").

1. The average American voter is neither a saint nor a devil; he is, quite literally, *l'homme moyen sensuel*.

2. He is accordingly very unlikely to feel personal guilt for general social conditions at home or abroad which he did not have a hand in creating.

3. Any effort by the intelligentsia or by po-

litical candidates to create such feelings of guilt will normally be deeply resented by many voters.

4. Even now, under conditions of unprecedented cultural turbulence in American society, a majority of the electorate (and an even larger majority of those who will in fact vote this year) still believe in the traditional or "conventional" political morality of this country. This morality includes such items as the following:

(a) This country has a free, democratic political system.

(b) This country has a private-enterprise economy which has produced the greatest good for the greater number of people in world history.

(c) This country's political and social system is based upon "careers open to all talents"; therefore, if I am not that well off, my children can hope for winning their place in the sun through their own achievements and hard work.

(d) The poor and the rich alike get pretty much what they deserve.

(e) Politically created threats to my own economic well-being and especially to the prospects of success for my children are an intolerable usurpation, which interferes with the right order of things. This is particularly intolerable when, as a result of pressure-group activities and support from bleeding-heart intellectual elites, I am expected to consent to this jeopardy for the sake of people who are unwilling or incompetent to "make it on their own," as I did.

(f) In the international world of threat, the U.S. must always be No. 1 in military power.

(g) As long as Americans are not doing very much of the dying, we will accept the President's definition of "national interest" in places like Vietnam. And we will accept as well his definition of the means to be used to protect and promote that interest. In any case, the world's greatest country cannot accept anything which looks like abject surrender.

5. George McGovern has become identified as being on the "wrong" side of all these propositions. Richard Nixon is identified with the "right" side—not only because of McGovern's diffuse negative image, but because (as Garry Wills and others have pointed out) Nixon is virtually the embodiment of this conventional political morality.

I have argued more than once that the United States is passing through one of the deepest transitional crises in its history; and that the crisis has increasingly assumed the form of a crisis of political legitimacy. I retain

this view. In one sense, the "emerging Nixon majority" may well be viewed as a counterrevolution of an "older" America against a "newer" one. But a "counter-revolution" cannot exist without a prior "revolutionary challenge. We can very briefly sketch some of this here.

1. The underlying crises are to be found in the following areas:

(a) Economic pressure, associated with the phenomenon of "stagflation." Among other things, it appears that the real income of the average American worker has not significantly increased since 1965. A very much larger part of this chronic (not yet acute) economic problem that most Americans realize is the result of a long-term deterioration in the *international* competitive position of the American economy, a deterioration which has been chronicled by the never-ending gold-currency crisis.

(b) Profound modification of the class structure under conditions of technological transformation and affluence. Very generally, the modifications parallel those suggested some years ago by David Apter, with an emergent stratification of technologically-competent, technologically-obsolete and technologically-superfluous being superimposed upon the older industrial stratification pattern.

(c) Massive *locational* transformations since 1945. For the shorter term, the most politically sensitive of these population exchanges affects the metropolitan region. These have had the effect of creating an ever-more explicit functional differentiation along sharply-marked political boundaries: the herding of the "technologically superfluous" strata into the center cities, the moving out to suburbs of the employed (and employable) white population. For the longer term, the continuing movement of the population to the South and West, and particularly the ongoing industrialization of the South, also will have cumulatively important effects on the structure of politics in the United States.

(d) Major *cultural* ruptures along generational, occupational and education-income dimensions.

2. With the nomination of George McGovern, cleavages within the population have been explicitly organized at the national level around these changes in *society* which have undermined the basis of the old New Deal alignment.

(a) This candidacy tends to pit the "technologically competent" and "technologically superfluous" against the "technologically obsolescent"—in short, what Levitin and Miller call the "liberal coalition" against "middle

America." For a number of reasons, this is a losing coalition—only partly because the lowest class of the technologically competent" have abysmal turnout rates.

(b) This candidacy tends to pit the "counterculture," the people who repudiate the conventional morality discussed above, against the large majority of the population which clings to old ways, and which fears that further politically directed change of the sort pioneered by the Great Society will hurt them.

(c) While economic problems are very large, this candidacy has been unable to capitalize on them. In considerable part, this is because McGovern's original economic package could be seen as threatening to transfer income away from about 48 per cent of the population (and quite a bit more than 48 per cent of the active electorate). In part, it is because McGovern seems to be widely regarded as not "safe" so far as conventional free-enterprise rhetoric is concerned.

3. The very nomination of George McGovern follows the classical critical-realignment scenario very closely. The party most vulnerable in its pre-existing coalitional structure to capture by large-scale dissent from things as they are has been duly captured. Much has been made of the McGovern rules in explaining this revolution within the Democratic party. More should be made of the point that the nomination would have been impossible under *any* rule structure unless there was a very broad and intense demand within the party for drastic change.

4. As Theodore Lowi and others have pointed out, the *political* (or legitimacy) crisis of the present is centered on "the end of liberalism." In particular this is the end of that kind of interest-group liberalism which, in ideology, is reflected in the writings of conventional American pluralist political science; and which, in operation, achieved its apogee in the programs of the Kennedy and Johnson administrations. *This* form of the "American ideology" has now achieved bankruptcy. As is well known, it is under devastating attack from at least two very discrete quarters.

(a) It is attacked by those who, literally revolted by the foreign policies of the 1960s, wish to achieve a moral regeneration. Many of these people want what amounts to a general confession of sins. They are of course concentrated in the McGovern campaign. Curiously, however, a very great deal of the McGovern domestic program calls for much more of the same—for a major expansion along "Great Society" lines without any very clear innova-

tions. Or at least it is widely *seen* to call for more of the same—and what is seen in politics is all-important.

(b) It is attacked by those who see runaway taxation, inflation, and expansion of programs to help the poor who, by definition, do not deserve such help. The latter are now clearly in the ascendancy.

5. As yet, the crisis of legitimacy has not crystallized around anything remotely like a rejection of the conventional American political morality (or the "American dream"), except among a very limited stratum of disaffected intelligentsia and a substantial part of the black community. Both of the latter are in fact, more or less pervasively identified in the public mind with all the social and political forces at work which threaten realization of "the dream." Nixon, on the other hand, virtually embodies it.

6. Time does not allow us to probe deeply into whether the time will come when more than the legitimacy of interest-group liberalism (or political-science pluralism) is at stake. A few points can be made here.

(a) It is my view that the crisis is stimulated by (you'll pardon the expression) "objective" transformations in the social and economic structure which are producing acute conflicts in belief systems.

(b) It remains *politically* uncrystallized in large part because no huge systemwide malfunction or trauma similar to those of the past has yet occurred. Perhaps no such trauma will occur in the foreseeable future.

(c) It would be very unlikely to be effectively crystallized even then unless exceptionally skilled political leadership were available. Thus, for example, even in the 1930s FDR was careful to attack "moneychangers in the temple," "economic royalists," and "malefactors of great wealth" rather than capitalism as such. Who knows? While some of us find it psychologically difficult to accept even the possibility, it may be that Nixon and his Republican successors may provide this "skilled political leadership." If so, and if *fortuna* is on their side, they stand to inherit the earth politically for the next generation.

(d) It is very possible that the crisis will continue indefinitely in a chronic rather than an acute state. If so, the leading characteristics of American electoral politics in the next decade or longer will be:

(i) Extreme volatility of voting behavior at all levels; (or continuing "electoral disaggregation," as I have called it);

(ii) Mediocre to poor levels of voting participation;

(iii) A broad "pattern" in which the candidate of either party who can most credibly promise limitation of the anxieties associated with demographic, social, and political change will win the election;

(iv) An extreme resistance (from mass referendum behavior to elite actions in Congress, the Executive and elsewhere) to *any* further policy innovations of the "Great Society" type. Such innovations as do occur (and there will be quite a few) will be legitimated in terms of conventional (or "conservative") values and will avoid any flavor of the redistributive.

To sum up: I regard both the nomination of McGovern and the emergent Nixon landslide as evidence for the existence of a deepseated crisis in belief system and underlying social structure. When large numbers of people find themselves in such crisis, several well-known paths for reducing "cognitive dissonance" and anxiety are open to them. One of these paths is well-trodden indeed; it is escape from the anxious present into an hypostatized past, an intense reaffirmation of the antecedent, of the most traditional core social values. Such a reaffirmation may seem to be grotesquely inappropriate to the intellectual. But all that the latter can do when living in such a period is to remind himself and others of what *that* can lead to, and to remind himself and others that such a flight from change can be stable in a relatively free society *only* if the pressure for change itself ceases or alters direction.

Of course, it may be wholly unnecessary to reach such Olympian heights in order to arrive at the "real" cause of the Nixon landslide. For it is very clear that the Democratic candidate himself has been made the "issue" in this campaign. The Republicans have simply picked up the pieces. There have been three major disasters in the campaign so far: the first is in the structure of the McGovern Commission party reforms, the second in the handling of the Eagleton affair, and the third in the effects of McGovern's inability to define and adhere to tenable policy positions.

1. The effect of the McGovern rules. The crucial issue here is the age-sex-race quota system which these rules, as executed by the Democratic convention, specified. Such quotas, in the first place, are in flat contradiction to the achievement or "merit" ethic which is deeply imbedded in American political culture. Secondly, it does no good *politically* to point to the conditions of factual inequality affecting what Justice Stone used to call "discrete and insular minorities" against which majority prejudice is directed. What people see is the existence of

quotas. To a very considerable extent, both Catholic "ethnics" and Jewish voters see this quota arrangement as part of a broader hyperliberal "conspiracy" against their welfare. It is obvious that many traditionally Democratic voters have found the 1972 Democratic convention to be deeply alien to them in symbolism and in personnel.

2. The Eagleton affair. Very probably, McGovern would have been better off keeping Eagleton rather than dropping him. There were three crucial mistakes in handling this affair—and it should be stressed that it has been of decisive importance in expanding the dimensions of the Nixon landslide.

(a) As one who was until very recently "from Missouri," I can testify that all sorts of background noise existed about Eagleton's "condition" ever since at least the 1968 election. Competent staff work could readily have seen this smoke, penetrated it, and examined the fire underneath. No such competent staff work existed.

(b) It was a crucial mistake for McGovern to react at once by "1000 per cent support for Eagleton," unless he was willing to ride out the storm. He was not.

(c) A situation like this requires *instant* evaluation and decision by the man in charge. It was a major error to let matters hang fire for a week, permitting Eagleton, in effect, to run a personal campaign against the staff and against McGovern.

3. Inability to define and adhere to tenable policy positions. The situation here is sufficiently well known to require little additional comment. The crucial point so far as public opinion is concerned is this: people neither know nor care very much about the details of policy, as a rule; but they are sensitive to wavering or unsteadiness in a candidate's positions, especially when the candidate's stock in trade is moralism.

The upshot of all this, very simply, was that McGovern created a large credibility gap around himself, and in two dimensions. First, the handling of the Eagleton affair, as well as his very publicized staff troubles, suggested doubts about the man's competence to make good decisions and control subordinates as president. Second, his efforts to modify his positions—no less than his courting of such people as Lyndon Johnson—have merely persuaded many voters that he's "just another politician," that winning for him is more important than principle. This I would regard as the supreme irony of the 1972 campaign, and the supreme disaster for McGovern personally.

Two things further should be stressed. First, a significant minority of people, when they vote for president, are well aware of the immense imperial scope of the office; there is, as it were, a "statesmanlike" attitude toward the electoral act among such voters. Before they feel free to vote according to party identification, liking of the incumbent, or perceived self-interest, they must be reassured about the basic competence of the candidate, his "capacity to govern." There is some reason for thinking that Stevenson had an "image problem" here; but one must go back to Horace Greeley in 1872, a full century ago, to find a candidate about whom a more pervasive doubt on this score has developed. Second, the level of popular discontent with the existing order of things is, as I indicated before, extremely high. People appear to be correspondingly sensitive to "opportunism" among political leaders. This is especially so when a leader who claims to be above it comes to be viewed as being as opportunistic as all the rest. Whether such a view is "right" or "wrong" is immaterial. Finally, to the extent that the discontent is in fact particularly directed against "interest-group liberalism," a quota system may be widely seen as its logical—and *illegitimate*—fruit.

But it seems to me very easy—too easy, in fact—to dwell upon these manifest inadequacies in the McGovern campaign. After all, only a member of President Nixon's immediate entourage would be likely seriously to believe that his leadership is inspired or inspiring. This incumbent administration is literally full of holes. The emergent FBI disclosures, going vastly beyond that "tip of the iceberg," the Watergate incident, go far toward demonstrating the literal truth of Senator McGovern's charges about the moral obliquity of this administration. Yet the overwhelming mood is one of apathy verging on narcolepsy, if not catatonia. Why is this? I believe that the general answer to the question lies somewhere in the direction of the following propositions:

(a) McGovern as a candidate has so thoroughly stultified himself that a very large part of the American electorate has literally "tuned him out."

(b) This tuning out also has a very great deal to do with popular identification of McGovern with the bankrupt liberalism of the 1960s.

(c) People in the main do not care very much about political corruption even when proved: the elections of 1872 and 1924 form good historical examples of this proposition. They come to care about it only when some systemwide malfunction directs their hostility

against the incumbent party *for other reasons* (as, e.g., in the election of 1876), usually economic ones.

(d) The crucial, central point as I read it, is this: Because of the structural crises alluded to here, a popular settling of accounts with interest-group liberalism is still going on: it is still unfinished business. Here, I believe, is the crux of the matter. Nixon is the incumbent; but Nixon and his administration have credibly presented themselves as antagonists to this liberalism. Until very recently, it had the allegiance of a wide array of policy and academic elites, it thus becomes uniquely possible for an incumbent administration to project itself *as opposed to an establishment*, to project itself virtually as the representative of the alienated (white) underdog.

(e) If Nixon were another Theodore Roosevelt, he could fashion a landslide against this "establishment" out of positive support for himself and his persona. Since he is not, the landslide becomes almost wholly negatively based; consequently, it becomes manifest by political narcosis, and by the most conspicuous noncampaign in the history of American electoral politics.

(f) If one seeks some historical parallel for this, crude as it is, we may find the most relevant to be the elections of 1920 and 1924; for the latter, in my view, reflected a continuation of the popular settling of accounts with Wilsonian liberalism which led to the Harding landslide of 1920. Not that this parallel is very exact, to be sure, but it is there nevertheless.

This overlong discourse is a "working paper" which—for whatever it may be worth—I am glad to share with my friends, students, and colleagues. Its basic theme may seem similar at first blush to Scammon and Wattenberg's "real majority" or even to Kevin Phillips's "emerging Republican majority." But it really isn't. As I read the evidence (and my hunches and "informed suspicions"), we are witnessing *the progressive radicalization of American politics*. But this turns out to be something very different indeed from the visions of further "break-through" reforms of society-through-politics which the McGovern coalition had.

On the surface, it could plausibly be argued that American politics is still dominated by a "vital center" and that "extremist" candidacies, whether of the Goldwater 1964 or the McGovern 1972 variety, are rejected by that center. But this view fails to deal with two issues. The first is that of explaining the conditions under which "radicals" like Goldwater and McGovern could win control of major parties at

all. And it is by no means enough simply to argue that primary- and general-election clienteles are of very different orders of magnitude. Secondly, such a view fails to take into account the movement of that "center" *across time*, or to contemplate the possibility that a counter-revolution of the great middle against the purveyors of a now-bankrupt interest-group liberalism is what the election is really about this year.

In any case, there are excellent reasons for supposing that George Wallace represents as much of the "wave of the future" in American politics as does George McGovern, and maybe more. We may conclude with two brief and very shrewd passages by H. L. Mencken on the 1920 election. The first has to do with the bankruptcy of an earlier, Wilsonian, liberalism—like its remote successor, extinguished in the ashes of war:

Americans tire, after twenty years, of a steady diet of white protestations and black acts; they are weary of hearing highfalutin and meaningless words; they sicken of an idealism that is oblique, confusing, dishonest and ferocious. . . . Today no sane American believes in any official statement of national policy, whether foreign or domestic. He has been fooled too often, and too callously and impudently. Every idea that has aroused him to sentimental enthusiasm and filled his breast with the holiest of passions has been dragged down into the mud by its propounders, and made to seem evil and disgusting. . . . He wants a renaissance of honesty—even of ordinary, celluloid politician's honesty. Tired to death of intellectual charlatanry, he turns despairingly to honest imbecility.

—H. L. Mencken, "In Praise of Gamaliel,"
Oct. 18, 1920

The second of Mencken's comments has to do with the subjective political realities of the American class structure of 1920. There has been less change since then than many of us may have supposed.

The United States has never developed a true proletariat. . . . Instead, it has simply developed two bourgeoisies, an upper and a lower. Both are narrow, selfish, corrupt, timorous, docile and ignoble; both fear ideas as they fear the plague; both are in favor of 'law and order,' i.e., harsh laws, unintelligently administered. . . .

The boobs, in fact, were against political radicalism as violently as the trembling captains. They always are, and for a reason lately plainly stated by Secretary Colby: they hope to rise and believe that they *will* rise—they want the loot protected so that it will be still there when they come to collect their share of it. Until this universal belief in prosperity around the corner dies out in the American people, there can be no serious radical movement in the republic. Now and then—as after 1893—there may

be a few growls, but that is as far as the thing will go. Nor can there ever be any genuine passion for liberty, or any organized movement against harsh laws, or any effective punishment of profiteers. Such things, to the bourgeoisie, are not evils; they are goods; upon them the whole structure of bourgeois society rests.

Only one thing will ever seriously damage that structure: unsuccessful war. The day the United States is beaten on land and sea, and the unbroken hope of 144 years suddenly blows up—that day it will be high time to look for the birth of radicalism. Until then, let us snooze in peace. We are all safe. All we have gobbled we may keep.

—H. L. Mencken, "Optimistic Note,"
Nov. 29, 1920

One may discount all of the elitism in Mencken's comments and still see the validity of both points in his argument—and what is more, their relevance to the 1972 election. Whether in fact crisis is once more to be resolved in "normalcy" remains to be seen. It seems to me that one very important truth of American politics is that liberalism repeatedly crashes against the outer limits of what the system can provide without *fundamentally* changing its character or beneficiaries, and so regularly comes to grief. Another equally important truth of American politics is that the political system normally works in a multitude of ways to scale mass demand down to the point where it can be accommodated without undue strain. This worked brilliantly during the Normalcy era. Mr. Nixon's first administration has been dedicated to scaling down again in ways relevant to this particular era in American history. "Four more years" will tell the tale as to whether this is still a viable strategy.

In the meantime, we see the extraordinary spectacle of an incumbent running for re-election and being re-elected in a time of vast, pervasive discontent: an incumbent running as a kind of underdog against an "establishment" which he somehow—despite his incumbency—is able to identify with his opponent. There has been nothing like it in the history of American politics. As to the aftermath, I am more than inclined to suspect that a decade hence we will come back to Peterkin's question about the Battle of Blenheim: "But what good came of it at last?" "That I cannot tell, but it was a famous victory." In the meantime, and for the present, let's leave "four more years" in the hands of the Providence which has always watched over the destiny of these United States.

WALTER DEAN BURNHAM
Massachusetts Institute of Technology

TO THE EDITOR:

Jeffrey L. Pressman's recent article "Preconditions of Mayoral Leadership" (*APSR*, 66 [June, 1972], 511–524), presents an interesting case study of mayoral behavior as figureheadship. In evaluating Mayor John Reading's performance, Mr. Pressman constructs a model of an "ideal" mayor and lists a set of preconditions for mayoral leadership. Unfortunately, Pressman's model is based mainly on research in communities interpreted in the literature as pluralistic and overlooks the effect of variations in community power and conflict structures on mayoral leadership styles, strategies, and performances. By basing his model on findings from only one type of situational context, Pressman underemphasized some important constraints on contemporary mayoral leadership and may have overestimated the potential of anyone, no matter what his personality, skills, or institutional resources, to attain the "ideal" mayor's goals in many American cities. Further, by using a rarely attained model as an evaluative standard, Pressman might have been too harsh on Mayor Reading and too hopeful that someone else could be more effective in Oakland.

In every community there is some conflict associated with the management of local government. However, the structure of conflict—its participants, stakes, intensity, focus, and forms of expression—differ across communities and over time. In studies of mayoral leadership there has been too little attention given to these variations and the impact of different configurations of groups and cleavages on political institutions and leadership activities. Since political structures vary, the pluralist model of mayoral leadership must be only one of a potential set of models of mayoral behavior. Its value as an explanatory tool, an evaluative standard, and a strategic device depends on its fit with a complex, variegated reality. How well does Pressman's model fit contemporary urban realities?

In the earlier pluralist studies of mayoral leadership, communities were interpreted to be multipolar, having low or moderate degrees of conflict. Mayoral performance was assessed on four criteria: the mayor's ability to mobilize community involvement, to integrate diverse interests, to activate innovative programs, and to convert the focus of community politics from a system of multilateral competition to a system of mayor-dominated politics, an executive-centered coalition. Because of the dispersion of power and veto privileges, innovation-

minded mayors built coalitions in which they served as brokers among the competing groups and which they used to mobilize support behind broad, overarching programs. The search for feasible innovative programs—usually couched in the rhetoric of collective goods—became the major entrepreneurial task for mayors in these pluralistic communities.

In Pressman's analysis, Mayor Reading's ineffectiveness is primarily explained by the absence of certain institutional and organizational preconditions for civic entrepreneurship. Pressman's analysis is consistent with his model. However, mayoral ineffectiveness can also be explained by factors Pressman's model underemphasized. Executive ineffectiveness can also be explained by changes in the structural preconditions of pluralist politics that affect the prospects for coalition building and civic entrepreneurship.

In many American cities increases in black populations have combined with rising black militancy and the uneasiness and hostility of white working-class residents to produce racial cleavages that are far more severe than the cleavages discussed in earlier studies of mayoral leadership. The political structure of many cities has changed from a system of multipolar competition of a low or moderate intensity to a system of bipolar or polarized conflict. Under conditions of racial polarization, both black and white leaders seek to block new programs that benefit the other racial group. The obstructionist tactics of racial politics militate against the kinds of innovative policies Pressman's "ideal" mayor might pursue and helps to explain the political immobilism of many American cities.

Since the bipolar conflict structure represents a different political reality than the pluralist structure implicitly assumed in Pressman's model, some of the structural preconditions that underpin mayoral entrepreneurship are worth examining in the context of racial polarization. Among the conditions that are said to foster pluralist politics, three are most affected by racial polarization. It has been argued that pluralist politics are more likely to occur when there is (1) an expanding set of divisible political stakes and collective goods that can be allocated without emotionally charged after effects; (2) fundamental sociopolitical agreement among a substantial majority of citizens; and (3) a commitment by officials and interest group leaders to a set of rules or bargaining conventions that lend stability and legitimacy to the policy-making process. As many cities en-

tered the last half of the 1960s, racial polarization produced changes in each of these preconditions affecting the feasibility of entrepreneurial strategies.

Of those substantive stakes usually associated with urban politics—public office or employment, money, and governmental services—even the casual observer notices their increased scarcity in most major cities. Even when some slack resources can be found, or when symbolic rewards can be substituted for substantive programs, mutual suspicion between white and black leaders often makes any form of brokering or innovation impossible. Under conditions of racial polarization, issues that might be considered public or collective goods and conform to the model of the overarching policy—like urban renewal, pollution control, recreation programs, and law-and-order policies—are often interpreted for their distributional impacts, seen as favoring one racial group at the expense of the other. In communities beset by racial polarization, "community wide" programs are interpreted in terms of their secondary impacts and in terms of their symbolic significance. Even broker-oriented coalitions are extremely difficult to assemble because racial group leaders are continually on their guard against the threat of co-optation. In short, under conditions of racial polarization: (1) programs are interpreted for their psychic effects on the self-image of the contesting groups; (2) the distributional impacts of new proposals come under close scrutiny; (3) proposals are evaluated for their opportunity costs (i.e., What other program might better benefit either the black or white communities?); (4) the long-run political consequences of an innovation are carefully analyzed (i.e., Will a new program ultimately strengthen one side at the expense of the other?).

The well-publicized frustrations of big-city mayors who possess Pressman's preconditions and are committed to civic entrepreneurship pose some serious questions about the utility of the pluralist leadership model as an explanatory tool and as a strategic device. Constraints rooted in the social structure of American cities inhibit mayoral leadership, and a mayor's strategy of leadership must take into account the degree and form of community conflict. Mr. Pressman's model of mayoral leadership has implicitly assumed a low or moderate degree of conflict, and he fails to emphasize community conflict patterns as a crucial contextual variable or constraint on mayoral leadership. In polarized communities a mayor's pursuit of the

liberal program goals Mr. Pressman's "ideal" mayor might promulgate would hardly "maintain within the political system a process of constructive dialogue between diverse groups which would contribute to harmony in the city" (p. 512).

CHARLES H. LEVINE

University of Maryland

TO THE EDITOR:

Professor Levine is quite right to emphasize urban social structure as a factor which can influence the exercise of mayoral leadership. Indeed, I made exactly that point in my discussion of the difficulties posed for a mayor by Oakland's lack of politically-interested groups. And far from ignoring racial polarization, I sought to demonstrate how black-white confrontations in Oakland served to depress and immobilize the mayor.

My purpose in creating a model of mayoral leadership was not to initiate yet another round of debate about community power. Rather, I wished to construct a standard which might be used to evaluate the exercise of leadership in a variety of social and political settings. Levine argues that I may have set the standard too high, but an evaluative model which corresponded with the experiences of numerous mayors would not be capable of distinguishing effective from ineffective performance.

In any event, I did not say that shortcomings of mayoral leadership in Oakland were solely due to Mayor Reading's personality. I argued that social structure, governmental institutions, and personality factors were all important in limiting political leadership in the city. Instead of debating whether or not the evaluative standard is too high or too low, we ought to concentrate on identifying and studying those elements which enhance or inhibit mayoral leadership. And in this regard, Professor Levine offers a number of helpful insights.

JEFFREY L. PRESSMAN

Dartmouth College

TO THE EDITOR:

Isaac Kramnick, in his recent essay "On Anarchism and the Real World: William Godwin and Radical England," (*APSR* 66 [March, 1972], 114-128), missed and distorted the political essence of at least one anarchist theoretician and perhaps of anarchism itself. Moreover, he indicted a complex and diverse political tradition on the basis of the ostensible shortcomings of one thinker and a few contemporary actors, the latter whose connection to

the rich tradition of anarchist theory is at best superficial.

While anarchists opt for a nonpolitical society as the only society in which people can achieve freedom and human fulfillment, most anarchists agree that such a society can only be achieved by decisively political means, namely, a social revolution. To be sure, education is important. The people must be shown, as serious revolutionaries have discovered, that their misery is neither a necessary nor an inevitable condition of social life. But education can only raise the consciousness of oppressed people who must themselves revolt to achieve human freedom.

One anarchist thinker, Peter Kropotkin, observed that a social revolution would be necessary to facilitate an anarchist society, a society marked by the right to well-being for *all*. He was obviously optimistic in his belief that the revolution was at hand and utopian in his faith that people can abruptly reorient their lives, let alone societies. But he was quite realistic in his understanding of the political qualities of a social revolution and the inherent bias of parliamentary regimes against fundamental social and political changes. For Kropotkin a revolution is an abrupt, violent, and destructive political activity.

Revolution . . . is not a simple change of governors. It is the taking of possession by the people of all social wealth. It is the abolition of all the forces which have so long hampered the development of humanity.

("Revolutionary Government," in Roger Nash Baldwin, *Kropotkin's Revolutionary Pamphlets*, N.Y.: The Vanguard Press, 1927, pp. 247-248.)

A revolution is a swift overthrow, in a few years, of institutions which have taken centuries to root in the soil, and seem as fixed and immovable that even the most ardent reformers hardly dare to attack them in their writings.

(*The Great French Revolution*, N.Y.: G. P. Putnam's and Sons, 1909, pp. 2-3.)

. . . a revolution [means] the demolition by violence of the established forms of property, the destruction of castes, [and] the rapid transformation of received ideas about morality . . .

("Revolutionary Government," in Baldwin, *op. cit.*, p. 238.)

The means of revolution are forceful expropriation, and Kropotkin claimed that this expropriation must be total:

It must apply to everything that enables any man—be he a financier, millowner, or landlord—to appropriate the products of others' toil. Our formula is simple and comprehensive.

We do not want to rob anyone of his coat, but

we wish to give the workers all those things the lack of which makes them fall easy prey to the exploiter. . . .

(*The Conquest of Bread*, N.Y.: The Vanguard Press, 1926, p. 41.)

While a minority, presumably some anarchist group, ". . . is to prepare men's minds for the revolution," the people are the authentic revolutionaries. Kropotkin asserted that

A handful of conspirators cannot fight an army . . . To an army must be opposed an army, and, failing an army—the people, the whole people, the hundreds of thousands of men, women, and children of a city. They alone can be victorious, they alone have conquered armies by demoralising them, by paralyzing their brute force.

(*The Great French Revolution*, p. 154.)

Now it seems that Mr. Kramnick overlooked the writings of Peter Kropotkin, an anarchist theorist who clearly follows in the tradition begun by Godwin, but who does not fit Kramnick's alleged stamp of elitism and nonpolitical means of education and enlightenment.

Kropotkin was an educator of the people, but he did not snobbishly pretend superiority to nor was he disdainful of them. He did not advocate "wasteful" confrontations, and he was, as I have indicated, an advocate of a social revolution as the only means by which human well-being could be established. If that is not political, what is?

BRUCE GILLESPIE

City University of New York

TO THE EDITOR:

One can seldom find in a responsible journal a more grossly unfair book review than that by Franklin W. Houn of Alan Liu's *Communications and National Integration in Communist China*, *APSR*, 66 (September, 1972) 1064–1065.

The validity of Houn's general criticisms can be judged only by reading the book. The specific examples which he cites to support his judgment, however, are a string of misrepresentations or unwarranted inferences. I will briefly discuss them all. His example of "glaring factual errors" is Liu's statement that the content of newspapers in years before the Cultural Revolution "showed no dominant theme." Houn proceeds to substantiate Liu's statement by listing no less than nine diverse themes. The example of contradictions on the building up of Mao's prestige by or in the army (p. 99) is no contradiction at all if one reads with reasonable care. Likewise reading the text shows no inconsistency (pp. 53 and 110–111) concerning reliance on the mass campaign. That Mao de-

stroyed the printed media during the early part of the Cultural Revolution because of their ineffectiveness (Liu's interpretation) is certainly quite as sustainable as Houn's view that it was because the persons controlling them were "taking the capitalist road"; the latter idea smacks of naive propaganda. Finally, the allegation that Liu charged that "Mao has never been willing to 'improve the material conditions of peasant's (sic) life,'" verges on falsification. The words of Liu are misquoted (the book, incidentally, has "peasants"). What he says is that without a major change of resource allocation, "to couple political penetration with a change in the material conditions of peasants' life, the prospect that the Chinese peasantry will be fully integrated with the rest of the nation is quite dim" (p. 173).

Further criticisms are of sources and detail. Anyone glancing at the footnotes can observe that Liu uses very many, varied, and frequently original sources. The specific complaint of basing references to the Cultural Revolution "almost exclusively" on Chao Tsung's three articles, cited allegedly 22 times, is a weak effort to find a flaw. There are 84 footnotes to the chapter, of which 20 refer to thirteen articles by Chao Tsung in a reputable and recognized journal.

When a reviewer doesn't like a book, he can usually find much fairly plausibly to criticize. That Houn's effort to damn this work falls so short is a tribute to it as a thoroughly sound treatment of its subject.

ROBERT G. WESSON

University of California, Santa Barbara

TO THE EDITOR:

Wesson seems to have accepted the correctness of my overall appraisal of Alan Liu's book by his complete silence on my primary criticisms of that book in the first two paragraphs of the review and by his failure to cite any specific merits as a positive proof of the latter's claimed excellence. Instead, Wesson attempts to discredit my review by challenging some examples of the book's other flaws. All his assertions are untenable, however. First, Liu himself speaks of the absence of "dominant themes" (p. 138), not "dominant theme" as Wesson says. Thus my effort to correct Liu's error by listing a number of dominant themes in the Chinese press in the period under discussion does not "substantiate" Liu's judgement. The contradiction between Liu's two statements (p. 99) on Lin Piao's campaign to exalt Mao stems from the fact that the endeavor to build up Mao's image in both the army and the country

at large began in 1960 and that the campaign to emulate the army that got under way in 1963 was not directly related to the former effort and certainly did not mark the army's first involvement in that undertaking. Despite Wesson's denial, Liu does praise anti-Mao elements' use of mass campaigns while chiding Mao for employing the same tactic (pp. 53, 86, 110-111). Nor am I prepared to alter my view that the suspension of large segments of the printed media (incidentally, it is inaccurate to say that Mao "destroyed the printed press") in 1966-67 (p. 33) was mainly because they had been under the control of persons "taking the capitalist road." If Mao's action had been indeed prompted by a low estimate of the utility of those media, one must ask why, since then he has reactivated more and more of them. If there is no explicit reference on p. 173 to Mao's alleged indifference to rural welfare,

statements on p. 178 do expressly attribute Peking's problems in the countryside to Mao's "unwillingness to improve the material conditions of peasant's [sic] life." Liu's heavy reliance on secondary sources can be seen by the fact that there are altogether 269 footnotes to the book, of which 183 cite sources of that category exclusively. In discussing the Cultural Revolution, I must repeat, Liu makes undue use of three articles on the subject itself and some specific aspects of it by Chao Tsung (transliterated as Chao Chung in some footnotes) in the avowed anti-Communist *Tsu Kuo* magazine. In fact, he cites two of them once each and the third 22 times. The last article is a serialized one which Wesson apparently has mistaken for separate essays.

FRANKLIN W. HOUN

University of Massachusetts

EDITORIAL COMMENT

The Backlog. A glance a little further along in this section will disclose that our backlog is truly enormous. This means that once an article has been accepted for publication it must ordinarily queue up for about a year before it sees print. Many of us believe that this is too long for maximally effective scholarly communication. Although the *Review* has had this year-long glut of coming attractions for quite a long time, and seems to have survived pretty well, nobody knows how many discouraged authors have taken their articles elsewhere rather than face the delays attendant not merely upon evaluation, or rejection by the *Review*, but acceptance.

A number of suggestions have been addressed to this problem. Least attractive to the management have been proposals to create of the *Review* a procrustean bed, where certain kinds of political science are ruled out *a priori*. This in our opinion defeats the purposes of a scholarly journal, which are to present work that contributes to the growing edge of knowledge. If people knew where that edge was *a priori*, it would be a rather dull edge.

Another perennial solution is to rob Peter to pay Paul. By amputating book reviews from the *Review*, goes this reasoning, more space is released for the publication of articles. Another journal is necessarily then created to publish book reviews, which of course is paid for through the same Association budget that presently pays for the *Review*. If that budget is not augmented, for example by charging an extra fee for subscriptions to the journal of book reviews, there is nothing to be gained from splitting reviews and the *Review* asunder. Many statesmen of the profession have assumed that the imposition of what amounts to a book review tax on top of our recently renovated dues structure would at a minimum provide unpleasant news about the elasticity of the market for learned writing in political science and at worst set off an effete intellectual version of the Boston Tea Party—perhaps a Cambridge Sherry party.

Some of those who have shrunk from heroic solutions have sought to pass the buck to the managing editor, urging him to raise his standards and get tough, as well as giving other household hints. This is all very well, but an acceptance rate that is already as low as nine per cent does not leave much room for maneuver. We have, nevertheless, found ourselves demanding an ever higher pitch of enthusiasm from referees before accepting articles outright,

and making revised articles jump hurdles about as high as newly submitted ones.

There is a point, of course, where this sort of thing begins to put authors off in large numbers. A scholarly journal can afford this only if the offended and discouraged are not very good scholars. If on the other hand, at least some of the articles we turn down should appear in the *APSR*, and an accelerating number of such articles are not even proffered to the *APSR* for evaluation, we shall have severely damaged the capacity of the *Review* to do its job for the growth of learning in political science.

This is a nice dilemma, and one we do not enjoy contemplating. One way to take the heat off is somehow to find the money in the hard-pressed Association budget to run a couple of fat issues and knock the backlog down to manageable size. If one of these days an issue of the *Review* arrives at the door looking—and hefting—more like an oversized blue brick than like a scholarly journal, readers may deduce that something drastic at long last is being done about the backlog.

Epistles. On the whole, the *Review* communications section is taken up with complaints. It occurs to us, however, that the epistolary mode is also suited to less doleful uses. Two letters in the current issue make the point. Both articulate notions that may or may not be worth an article-length exposition at this stage of the game. Both are long on speculation, short on verification. Both tickle the mind. We think there is room for such hors d'oeuvres of the intellect in the *Review*. But not too many of them, of course, since we have no desire to lose our hard-earned reputation as perhaps the heaviest meat and potatoes place in the world of political science.

Son of Youth. We have heard it said that the *Review* does not encourage submissions from graduate students. This is not so. The *Review* as a matter of principle makes no distinction about the sources of submissions. We welcome submissions from everyone. As a matter of practice, fewer than one out of ten articles submitted survives our anonymous reviewing process and is accepted for publication. Many are galled, as George S. Kaufman undoubtedly said, and few are chosen. We have no way of knowing whether graduate students contribute disproportionately to the population of the disappointed.

We do know, however, of graduate students

whose work has appeared in these pages over the last few issues, or has been accepted for future publication. We suspect that sometimes an article will be submitted by a graduate student but by the time we get around to printing it the job market has metamorphosed another humble caterpillar into an assistant professor.

Facts such as these, if anyone cares to collect them, may or may not bear upon the question of whether graduate students ought to run their own political science journal. Law students run most law journals, and in many cases with great success. These are not uncommonly subsidized by law schools, and there are multitudes who will testify to the educational value of the experience. Students customarily contribute only unsigned notes to these journals, to be sure, and signed articles are almost always the work of professors and practitioners. So in this particular the *Review*, which does print signed work by graduate students on the same basis as material contributed by more senior scholars, may appear in a comparatively favorable light.

In a modest way, the *Review* seeks also to provide a little of the flavor of the law review educational experience to a certain number of Berkeley graduate students who serve as editorial interns. Interns do a lot of the tiresome work that goes into producing a journal, just as law students do for their journals. They do not share the joys and sorrows of making decisions about publication as law students do. Our experience with the interns to date suggests that if they did, the quality of the *Review* would in no way suffer. These decisions, however, remain with a managing editor accountable to the Council under the constitution of the association, for better, or worse, or more likely, for just about the same.

Articles Accepted for Future Publication

- Paul R. Abramson, Michigan State University, "Generational Change in American Electoral Behavior"
- Timothy Almy, University of Georgia, "Residential Location and Electoral Cohesion: The Pattern of Urban Political Conflict"
- Peter H. Aranson, University of Minnesota, Melvin J. Hinich, and Peter C. Ordeshook, Carnegie-Mellon University, "Election Goals and Strategies: Equivalent and Nonequivalent Candidate Objectives"
- Robert Axelrod, University of California, Berkeley, "Schema Theory: An Information Processing Model of Perception and Cognition"
- Harry W. Blair, Bucknell University, "Minority

- Electoral Politics in a North Indian State: Aggregate Data Analysis and the Muslim Community in Bihar, 1952-1972"
- Bernard H. Booms, Pennsylvania State University and James R. Halldorson, F. W. Woolworth Co., "The Politics of Redistribution: A Reformulation"
- Steven J. Brams, New York University and Morton D. Davis, City College of New York, "The 3/2's Rule in Presidential Campaigning"
- Christopher Bruell, Boston College, "Thucydides' View of Athenian Imperialism"
- Blair Campbell, University of California, Los Angeles, "Helvétius and the Roots of the 'Closed' Society"
- Andrew T. Cowart, University of Oslo, "Electoral Choice in the American States: Incumbency Effects, Partisan Forces and Divergent Partisan Majorities"
- Harry Eckstein, Princeton University, "Authority Patterns: A Structural Basis for Political Inquiry"
- Peter Eisinger, University of Wisconsin, Madison, "Racial Differences in Protest Participation"
- Victor Ferkiss, Georgetown University, "Man's Tools and Man's Choices: The Confrontation of Technology and Political Science"
- Virginia Gray, University of Kentucky, "Innovation in the States: A Diffusion Study"
- A. James Gregor, University of California, Berkeley, "On 'Understanding' Fascism: A Review of Some Contemporary Literature"
- Nobutaka Ike, Stanford University, "Economic Growth and Intergenerational Change in Japan"
- Donald B. Johnson and James R. Gibson, University of Iowa, "The Divisive Primary Revisited: Party Activists in Iowa"
- Richard S. Katz, Yale University, "The Attribution of Variance in Electoral Returns: An Alternative Measurement Technique"
- Chong Lim Kim and Donald P. Rachtler, University of Iowa, "Candidates' Perception of Voter Competence: A Comparison of Winning and Losing Candidates"
- Warren Lee Kostroski, Wittenberg University, "Party and Incumbency in Postwar Senate Elections: Trends, Patterns and Models"
- Eugene B. McGregor, Jr., University of Maryland, "Politics and the Career Mobility of Bureaucrats"
- James T. Murphy, Wesleyan University, "Party and Pork: Party Conflict and Cooperation in House Public Works Committee Decision Making"
- Peter B. Natchez, Brandeis University, and Ir-

- vin C. Bupp, Harvard University, "Policy and Priority in the Budgetary Process"
- David Ray, Stanford University, "Membership Stability in Three State Legislatures: 1893-1969"
- William H. Riker, University of Rochester, "The Paradox of Vote Trading"
- David W. Rohde, Michigan State University and Kenneth A. Shepsle, Washington University, "Democratic Committee Assignments in the House of Representatives: Strategic Aspects of a Social Choice Process"
- Douglas D. Rose, Tulane University, "National and Local Forces in State Politics: The Implications of Multi-Level Policy Analysis"
- Lester M. Salamon, Vanderbilt University and Stephen Van Evera, University of California, Berkeley, "Fear, Apathy, and Discrimination: A Test of Three Explanations of Political Participation Among the Poor"
- Kenneth A. Shepsle, Washington University, "On the Size of Winning Coalitions"
- Brian Silver, Florida State University, "Social Mobilization and the Russification of Soviet Nationalities"
- Walter Dean Burnham, Massachusetts Institute of Technology, "Theory and Voting Research: Some Reflections on Converse's 'Change in the American Electorate'"
- William Cavala, University of California, Berkeley, "Changing the Rules Changes the Outcome: Party Reform and the 1972 California Delegation to the Democratic National Convention"
- Ada Finifter, Michigan State University, "The Friendship Group as a Protective Environment for Political Deviants"
- Peter C. Fishburn, Pennsylvania State University, "Paradoxes of Voting"
- Kenneth Jowitt, University of California, Berkeley, "An Organizational Approach to the Study of Political Culture in Marxist-Leninist Systems"
- John C. Pierce, Washington State University and Douglas D. Rose, Tulane University, "Non Attitudes and American Public Opinion: The Examination of a Thesis"
- Stephen G. Salkever, Bryn Mawr College, "Virtue, Obligation and Politics"
- Richard Zeckhauser, Harvard University, "Voting Systems, Honest Preferences and Pareto Optimality"
- A. H. Somjee, Simon Fraser University, "Caste and the Decline of Political Homogeneity"
- Peter J. Taylor, University of Newcastle-Upon-Tyne, "A New Shape Measure for Evaluating Electoral District Patterns"
- Glenn Tinder, University of Massachusetts, Boston, "Beyond Tragedy: The Idea of Civility"
- Vernon Van Dyke, University of Iowa, "Human Rights Without Discrimination"
- Susan Welch, University of Nebraska, "The Impact of Party on Voting Behavior in a Nonpartisan Legislature"
- Eugene R. Wittkopf, University of Florida, "Foreign Aid and United Nations Votes: A Comparative Study of Aid Allocations and Voting Agreements"
- Roger E. Wyman, Rutgers University, "Middle-Class Voters and Progressive Reform: The Conflict of Class and Culture"
- William Zimmerman, University of Michigan, "Issue Area and Foreign Policy Process: A Research Note in Search of a General Theory"

Woodrow Wilson and the Study of Administration: A New Look at an Old Essay

RICHARD J. STILLMAN, II

California State College, Bakersfield

Cornell University
Ithaca, N.Y.

Oct. 13, 1886

My dear Professor Wilson:

We have recently organized a Historical and Political Science Association, and I hope we shall be able in the course of the year to have a few addresses before the Association, and I write you for the purpose of ascertaining whether you have not some address on an administrative subject or a topic in comparative political methods that would be advantageous to our students. . . .

C. K. Adams¹

News Item

Before the Historical and Political Science Association on the evening of November 3 Professor Woodrow Wilson read a very able paper on "The Study of Administration." Professor Wilson outlined the history of the study, showed how it was a comparatively new development in political science, very cogently presented the necessity and value of such study, and indicated the methods by means of which it ought to be carried on.

New York Evening Post
November 4, 1886

A surprising number of students and scholars of public administration in recent years have returned to the pages of Woodrow Wilson's essay, "The Study of Administration," presumably for a deeper understanding of the historic antecedents of American administrative thought. Few modern readers, however, seem to draw the same conclusions about the essential point of Wilson's writing. For instance, recently Frederick C. Mosher in *Democracy and the Public Service* wrote that the essay embodied one of the earliest American statements pertaining to the separation of administration from politics. Wilson, Mosher stressed, "made the most vigorous statement on this dichotomy."²

¹ Letter from Charles Kendall Adams, *The Papers of Woodrow Wilson, 1885-88*, Vol. 5, ed. Arthur S. Link (Princeton: Princeton University Press, 1968), 351.

² News item from the *New York Evening Post*, cited in *The Woodrow Wilson Papers*, 5, 358.

³ Frederick C. Mosher, *Democracy and the Public Service* (New York: Oxford University Press, 1968), p. 68. For a similar point of view on the essay, see John Porter East, *Council-Manager Government: The Political Thought of Its Founder, Richard S. Childs* (Chapel Hill: University of North Carolina Press, 1965), p. 23.

Fred Riggs, however, disagrees. In an article on development administration, Riggs sees the reverse lesson emphasized, "For Wilson not only were politics and administration closely intertwined, but administrative action was scarcely conceivable except as the implementation of general policies formulated by political means."⁴ The essay's basic truth, at least for Riggs, is that politics and administration are inseparable.

John C. Buechner in his text, *Public Administration*, offers a third perspective, specifically, the importance of Wilson's essay was his argument that the study of public administration should be akin to the central concerns of business administration, namely the values of "economy," "efficiency" and "effectiveness." As Buechner writes: "The basic premise of Wilson's arguments was that the affairs of public administration were synonymous with those of private administration."⁵

Indeed what lesson (or lessons) should be learned from reading this salient document in American administrative thought? Did it stress the dichotomy of politics and administration? Or their inseparability? Or was Wilson, as Buechner suggests, more simply an evangelist for the wholesale application of business values to the sphere of government?

Perhaps the best way to understand the significance of the essay is to begin with the origins of Wilson's academic interest in the subject.

The Intellectual Roots of Wilson's Administrative Philosophy:

Johns Hopkins, Darwin, and Reform

While Woodrow Wilson had enthusiastically studied politics as an undergraduate at Princeton (1875-79)⁶ and law at the University of Vir-

⁴ Fred W. Riggs, "Relearning Old Lessons: The Political Context of Development Administration," *Public Administration Review*, 25 (March, 1965), cited at p. 71.

⁵ John C. Buechner, *Public Administration* (Belmont: Dickenson Publishing Company, 1968), p. 6.

⁶ For a complete set of Wilson's college papers, refer to *The Papers of Woodrow Wilson, 1856-80*, Vol. 1, especially his first published article on "Cabinet Government in the United States," pp. 493-510.

ginia (1879–81),⁷ his serious study of government and administration began at Johns Hopkins University (1883–85). Johns Hopkins had opened in November, 1877, and by 1881 the University had appointed its first two full time graduate social science professors, Herbert B. Adams and Richard T. Ely.⁸

Both professors only a few years earlier had earned their Ph.D.'s at the University of Heidelberg (Germany). They were steeped in continental history, politics, and economic scholarship and were intensely committed to making Johns Hopkins a top-ranking social science research center.⁹ In Dr. Ely's classroom the young Wilson heard his first lecture on the subject of administration, for as he wrote of Ely's lecture on February 27, 1885:

... Dr. Ely read a brief paper upon Administration, prefacing the reading of it by calling attention to two points of interest in French administrative practice: namely (1) That over each administrative dept., large or small, a single officer is placed who is held strictly responsible for the workings of his dept., though he is often assisted by a council of advice. (2) Courts for the enforcement of administrative laws.

In his paper Dr. Ely dwelt upon the importance of administrative study; upon the special necessity for it in this country, in view of the fact that our administration is the worst in the world; and upon

the strange apathy which has hitherto reigned in the U.S. with regard to questions of this nature.¹⁰

Dr. Ely's course ran only a few weeks and was essentially an introductory bibliographical survey of the major European writings in the field, principally works by German authors: Rudolf von Gneist, Lorenz Joacob von Stein, Barthold Georg Niebuhr, Robert von Mohl, Ludwig von Ronne, Siegfried Isaacsohn, and John Kasper Bluntschli (Bluntschli's personal library had been donated to Johns Hopkins in 1883, making German writings readily accessible to the students).¹¹

Wilson's intellectual heroes, however, at least those most frequently cited in his student essays, were British rather than German political theorists and statesmen, especially English conservatives and Manchesterians like Bagehot, Burke, Gladstone, Cobden, and Bright. In particular, Wilson admired Walter Bagehot's philosophic insight, literary style, and understanding of politics as a process of slow organic growth. Bagehot, wrote the youthful scholar, "would much rather . . . see [society] grow than undertake to reconstruct it."¹²

Wilson's conservative evolutionary view of society was reinforced by his teacher, Herbert Adams, whose favorite hobbyhorse was the "germ theory of politics."¹³ American political institutions, according to Dr. Adams, were the result of a slow organic growth process starting with the village organization which in the fifth century A.D. the Angles and Saxons had

For several excellent accounts of Wilson's early life upon which this study is based, refer to: Henry W. Bragdon, *Woodrow Wilson: The Academic Years* (Cambridge: Harvard University Press, 1967); George C. Osborn, *Woodrow Wilson: The Early Years* (Baton Rouge: Louisiana State University Press, 1968); Ray Stannard Baker, *Woodrow Wilson: Life and Letters, 1856–90*, Vol. I (Garden City: Doubleday, Page & Co., 1927); Arthur S. Link, *Wilson: The Road to the White House* (Princeton: Princeton University Press, 1947); and Alexander L. George and Juliette L. George, *Woodrow Wilson and Colonel House: A Personality Study* (New York: Dover Publications, 1956).

⁷ Wilson studied law initially as a route to politics, as he wrote to Ellen Axson, October 30, 1883, "The profession I chose was politics; the profession I entered was the law. I entered the one because I thought it would lead to the other," in Baker, I, 109.

⁸ Herbert B. Adams: born in Shutesburg, Mass. 1850, died 1901, A.B. Amherst 1872, University of Berlin 1874–75, Ph.D. Heidelberg 1876, Johns Hopkins Faculty 1876–1900.

Richard T. Ely: born in Ripley, New York 1854, died 1933, A.B. Columbia 1876, Ph.D. Heidelberg 1879, Johns Hopkins Faculty 1881–92.

⁹ For an excellent account of the development of Johns Hopkins University and the role of Drs. Adams and Ely in building the social science department, see Hugh Hawkins, *Pioneer: A History of The Johns Hopkins University, 1874–89* (Ithaca: Cornell University Press, 1960), especially chapter ten, pp. 169–86.

¹⁰ From the Minutes of the Seminary of Historical and Political Science, Feb. 27, 1885, *The Papers of Woodrow Wilson, 1885*, Vol. 4, 303.

Dr. Ely later noted Wilson's enthusiasm for the subject he taught, "I felt that I struck a spark and kindled a fire in Wilson." Refer to Richard T. Ely, *Ground Under Our Feet* (New York: Macmillan Co., 1938), p. 114.

¹¹ A listing of Wilson's bibliographical index between 1883 and 1890 cites 26 entries on the topic of administration, mostly German works with a few French and English titles, refer to *The Papers of Woodrow Wilson, 1885*, Vol. 4, 303.

Johann Kasper Bluntschli, 1808–81, was a Professor of International Law at the University of Heidelberg and had taught both Professors Adams and Ely.

¹² As quoted in Richard Hofstadter, *The American Political Tradition* (New York: Vintage Books, 1954), p. 242.

¹³ Henry Bragdon, *Woodrow Wilson*, p. 106 and Hugh Hawkins, *Pioneer*, pp. 300–301. Wilson resented this minute style of scholarship: "... digging into the dusty records of old settlements and colonial cities . . . seems very tiresome in comparison with the grand excursions among imperial policies which I had planned for myself," Letter to Ellen Axson, October 16, 1883, in Baker, *Woodrow Wilson*, pp. 174–75.

brought to England from Germany. These "primordial cells of the body politic," Adams argued, later developed into more complex modern institutions. In keeping with the Darwinian *weltanschauung* (equally popular with contemporary intellectuals such as Herbert Spencer, Sir Henry Maine, and William Graham Sumner), Dr. Adams put his students hard at work examining the minute details of the governments of the thirteen colonies in order to discover the evolutionary process of American institutions.¹⁴

Besides an extensive exposure to Social Darwinism, Wilson was exposed at Johns Hopkins to leading American reformist doctrines. The early 1880s saw mugwump reforms precipitated by the revelations of the spoilsmen and by the assassination of President James Garfield by a disappointed office seeker. The reformist thrust culminated in the passage of the Civil Service Act of 1883 (the Pendleton Act). As Leonard D. White astutely observed of the mugwump reformers:

The demand for moral reform was most effectively urged outside the halls of Congress by a small band of citizens who were dismayed by the corruption of the postwar years, and who eventually came together in the National Civil Service Reform League. . . . [The League's] first task was to agitate for a federal civil service law and to secure corresponding legislation in the states; its second was to defend and extend the legislation that was eventually forthcoming. Around the League were concentrated the reform forces, and seldom have so few men with such meager resources produced such major results. They were, of course, carried forward by a tide of opinion which was progressively shocked by the evils of the spoils system, by a growing irritation on the part of the business community, and by the concern of thoughtful men over the prostitution of party and the weakness of executive power.¹⁵

Civil Service Reform was a lively topic of

debate at Johns Hopkins and the faculty were active proponents of such legislation.¹⁶ *Congressional Government*, Wilson's Ph.D. treatise and first book, reflects this reformist sentiment.

Congressional Government: A Prelude to "The Study of Administration"

Of the eight books Woodrow Wilson published during his academic career,¹⁷ *Congressional Government* probably remains the only one still read and cited by students of government. Written in 1884 when Wilson was 28 years old and in his last year of graduate education, the book demonstrates Wilson's attempt to emulate the writing style and point of view of Walter Bagehot.¹⁸ Bagehot's famous treatise, *The English Constitution* (1867), had sought to examine the British political system as "a living reality" and "organic whole." Similarly, Wilson's "Preface" to *Congressional Government* expressed his goal as: "My chief aim in these essays, has been, therefore an adequate illustrative contrast of these two organs of government (cabinet and congressional) with a view to making as plain as possible the actual conditions of federal administration."¹⁹

The thesis of *Congressional Government* was that the central features of the Federal Constitution with its checks and balances of the three branches of government as well as the balance between the powers of states and the federal government had fundamentally been altered. Since the Civil War, argued Wilson, the federal government had become omnipotent and Congress had emerged as the center of national power, eclipsing both the judiciary and executive branches. "The balances of the Constitution are for the most part only ideal," wrote Wilson. "For all practical purposes the national government is supreme over the state governments and Congress predominant over its so-called coordinate branches."

Primarily *Congressional Government* focuses on a description and criticism of the shortcom-

¹⁴ For a list of Wilson's readings of the contemporary Social Darwinian writings, refer to *The Papers of Woodrow Wilson*, 5, 19, note 1. Two excellent analyses of the impact of Social Darwinism on late nineteenth century America are found in Richard Hofstadter, *Social Darwinism in American Thought* (Philadelphia: University of Pennsylvania Press, 1944) and Morton G. White, *Social Thought in America* (New York: Viking Press, 1949).

¹⁵ Leonard D. White, *The Republican Era, 1869-1901* (New York: Macmillan Co., 1958), p. 297. Also, for a detailed and insightful account of civil service reform, see Paul Van Riper, *History of the United States Civil Service* (Evanston: Row Peterson and Co., 1958), pp. 60-136, and A. Bower Sageser, *The First Two Decades of the Pendleton Act* (Lincoln: University of Nebraska, 1935).

¹⁶ Henry Bragdon, *Woodrow Wilson*, p. 118, and Hugh Hawkins, *Pioneer*, pp. 178-79.

¹⁷ The eight books Wilson published during his academic years were *Congressional Government* (1885); *The State* (1889); *Division and Reunion, 1829-1889* (1893); *An Old Master and Other Political Essays* (1893); *Mere Literature and Other Essays* (1896); *George Washington* (1896); *A History of the American People* (1902); and *Constitutional Government in the United States* (1908).

¹⁸ Henry Bragdon, *Woodrow Wilson*, p. 125, and George C. Osborn, *Woodrow Wilson*, p. 117.

¹⁹ Woodrow Wilson, *Congressional Government: A Study in American Politics*, as quoted from *The Papers of Woodrow Wilson*, 1885, 4, 13-179.

ing of Congress, which Wilson saw as "a government of Standing Committees." He cited the enormous power of these committees in American Government—their fiscal irresponsibility, their secret activities, their fragmenting effect on party discipline and executive leadership. He believed that such Congressional dominance "parcels out power and confuses responsibility." "If there is one principle clearer than another," he concluded, "it is this: that in any business, whether of government or of mere merchandising, *someone must be trusted*, in order that when things go wrong it may be quite plain who should be punished. . . . *Power and strict accountability for its use* are essential constituents of good government."

Like the other mugwump reformers of the 1880s—George Curtis, Carl Schurz, Dorman Eaton, Everett Wheeler, and Charles Bonaparte—Wilson was caught up in the pressing issues of the day, namely, how to make politics less open to the influence of the spoilsmen, less prone to political abuse, and more representative of the general public interest rather than of special interests. As George Curtis, a leading mugwump orator at that time, pleaded:

What we affirm is that the theory which regards places in the public service to be distributed after an election, like plunder after a battle, the theory which perverts public trusts into party spoils . . . destroys the function of party in a republic, prostitutes elections into a desperate strife for personal profit and degrades the national character by lowering the moral tone and standards of the country.²⁰

In the pages of *Congressional Government* Wilson had likewise pointed out the political abuses and irresponsibility of the legislative branch. He sought "a strong, prompt, wieldy and efficient government" and frequently looked over his shoulder at the British Cabinet Government, the model of his mentor, Walter Bagehot, as a possible means for restoring "good" government. Yet, his first book curiously falls short of specifically recommending cabinet government as a suitable substitute for the existing American system. Indeed he fails to recommend any cure at all for the ills he enumerates in *Congressional Government*.

Among the questions Wilson leaves untouched are: Would cabinet government really serve as the best remedy for America's institutional problems? Was a foreign system the answer for our domestic troubles? Or would an-

other antidote be necessary to heal our political ills?

"The Study of Administration": Part I

In 1885, the year Wilson completed his first book and started his college teaching career at Bryn Mawr, he referred in a letter to R. L. Gould of his "work upon comparative systems of administration."²¹ Three fragments of his early writing on the topic of administration (1885–86) have recently been published for the first time and, while brief and inchoate, these pieces suggest that Wilson's mind was increasingly being attracted to the study of administration as a possible method for correcting the political abuses so painfully apparent to the reformers of his era.²²

The opportunity to address fully the subject of administration came on the evening of November 3, 1886. At the invitation of President Charles K. Adams of Cornell University, Wilson was asked to speak before the newly organized Historical and Political Science Association in Ithaca, New York (his speech was eventually published in the *Political Science Quarterly*, June 1887).²³

²¹ Letter from R. L. Gould, *The Papers of Woodrow Wilson, 1885–88*, 5, 94–95. The actual letter Wilson wrote to Gould is missing from his papers.

Curiously, Wilson found little necessity to study administration firsthand in nearby Washington, D.C. As he wrote to Ellen Axson, January 22, 1885, ". . . If I wrote *Congressional Government* without visiting Washington, much more can I write upon the science of administration without doing so." As quoted in Baker, p. 259.

²² *The Papers of Woodrow Wilson, 1885–88*, 5, 43–54. The three fragments are each only a few pages long but clearly provide the basis for Wilson's "The Study of Administration." Interestingly Wilson entitled the first fragment, "Notes on Administration," then changed the title to read, "The Art of Government," and finally to "The Study of Administration." The changes in the title reflect his own evolution of thinking on the subject. For instance, in "Notes on Administration" he wrote, "I suppose that no great discoveries of method are to be made in administration." Yet in his final draft of the article, he states ". . . administrative study and creation are imperatively necessary. . . ." Refer to the Editorial Note in *The Woodrow Wilson Papers*, 5, 43–44.

²³ Letter from Charles K. Adams, *The Woodrow Wilson Papers*, 5, 351. For correspondence concerning the publication of his lecture in the *Political Science Quarterly*, refer to pp. 386–87. For interesting comments on the essay by Almont Barnes and Wilson's reply, read pp. 518–21. Also for correspondence with Herbert B. Adams which eventually secured Wilson a part-time lectureship on administration at Johns Hopkins, refer to pp. 393–94.

Text of "The Study of Administration" is cited from *The Papers of Woodrow Wilson, 1885–88*, 5, 359–80.

The essay, "The Study of Administration," originally

²⁰ George W. Curtis, "Party and Patronage," in *Orations and Addresses of George William Curtis* (New York: Harper and Brothers, 1894), p. 502.

"The Study of Administration" begins by introducing the reader to the general field of administration. According to Wilson, the study of administration developed because society has become increasingly complex, and government, more democratic. Reflecting the dominant evolutionary themes of his period, he argues, "There is scarcely a single duty of government which was once simple which is not now complex. Government once had but a few masters, it now has scores of masters." And next echoing the reformist anxieties about the trend toward the excesses of democracy, "We have enthroned public opinion and . . . the very fact that we have realized popular rule in its fulness [sic] has made the task of organizing that rule just so much more difficult." "The reformer is bewildered by the fact that the sovereign's mind has no definite locality but is contained in a voting majority of several million heads." Wilson then implies reform is necessary, but, believing like Bagehot in gradualism, "practical reform must be slow and all reform must be full of compromise."

And what direction should the reform take? In the body of his essay, Wilson offers a solution that he had increasingly been moving toward since 1885, namely, the study of administration. According to Woodrow Wilson, the study of administration would "rescue executive methods from the confusion and costliness of empirical experiment and set them upon foundations laid deep in stable principles." Elaborating on an earlier theme in *Congressional Government*, he connects the study of administration with methods for centralizing political authority so as to provide "clearcut responsibility which would insure trustworthiness." Drawing a graphic metaphor, he explains, "Self-government does not consist in having a hand in everything, any more than housekeeping consists necessarily in cooking dinner with one's own hands. The cook must be trusted with large discretion as to the management of the fire and the ovens."

Yet, Wilson, at this point, foresees a problem that would plague administrative scholars during the next century, specifically how "to make public opinion efficient without suffering it to be meddlesome." Early in this essay he seems to agree with the German scholar Johann Kaspar Bluntschli that for the sake of efficiency

administration and democratic processes should be separated. But at another point in his essay, Wilson views administration as deeply intertwined in the political realm, "Administration philosophically viewed is closely connected with the study of the proper distribution of constitutional authority." Wilson's essay thus vacillates between the two poles of thought regarding the separability and inseparability of administration from politics (thereby providing generations of later scholars with ample footnote ammunition for both sides of the argument).

One can, of course, speculate as to the reason why Wilson exposed this vital issue, yet failed to reach a clearcut conclusion about the problem. Was he unaware of how important this issue really was (or would become)? Was he hedging because other academic opinions were equally inconclusive on the subject?

Perhaps the truth of the matter was that this issue—i.e., the proper relationship between the administrative and political realms—was simply not his major interest. Rather, like the other reformers of his time, Wilson was more concerned with restoring moral qualities to government—insuring trustworthiness, improving honesty and responsibility, and securing efficiency. His central object was moral reform and he saw this ideal as best realized through the practical study of administration and the development of a civil service. "The ideal for us," he writes, "is a civil service cultured and self-sufficient enough to act with sense and vigor."²⁴ Wilson's nineteenth century mind was still not exposed to twentieth century scientific management—a political doctrine that would substantially favor a hard-and-fast division between politics and administration.²⁵

"The Study of Administration": Part II

In the last section of the essay Wilson returns to another dilemma he had wrestled with in *Congressional Government*—the applicability of foreign experiences to American institutions. His first book had drawn frequent comparisons between the United States and British systems of government, and in the last section of the essay, he continues to find the comparative approach a useful endeavor: "If I see a

appeared in the *Political Science Quarterly*, 2 (June 1887), 197-222 and was reprinted in the same journal, 55 (December 1941), 481-506. Wilson's essay appeared six years before the first textbook on administration was published, Frank J. Goodnow's *Comparative Administrative Law*, two volumes (New York: G. P. Putnam's Sons, 1893).

²⁴ In the original draft delivered at Cornell University and deleted from the subsequent article appearing in the *Political Science Quarterly*, this point was stressed even more strongly, presumably in defense of the newly created Civil Service System, then only three years old.

²⁵ For the classic attack on the rigid separation of politics from administration in scientific management doctrine, read Dwight Waldo, *The Administrative State* (New York: Ronald Press, 1948).

murderous fellow sharpening a knife cleverly I can borrow his way of sharpening the knife without borrowing his probable intent to commit murder with it."

Though, again as in *Congressional Government*, Wilson retreats from any specific recommendations for importing foreign practices. "Our own politics," he argues, "must be the touchstone of theories." "Arrangements not only sanctioned by conclusive experiences elsewhere but also congenial to American experiences must be preferred." Ultimately Wilson seemed too imbued with the spirit of conservative Darwinism to accept the wholesale transplantation of foreign institutions upon American soil.

Curiously, though, by the end of "The Study of Administration" the reader still remains uncertain about the actual substance of the study of administration. The essay covers considerable ground, touching on several vital issues, yet always skirting the projected theme. One senses throughout an abstract enthusiasm for the subject under discussion, though Wilson meanders, never leading the reader into a step-by-step description of the flesh and bones of administrative study.²⁶ From time to time his essay likens the study of administration to "business methods," "instituting a civil service," "fixing responsibility for public action," or "a problem of distributing constitutional authority." Such a tangle of metaphors is indeed exasperating for any careful reader.

One might speculate that a clear definition was not Wilson's major intention in his essay. Rather what was more important to him and to his mugwump audience was finding an appropriate ideology to justify their efforts to strengthen the executive branch, centralize authority, and check congressional irresponsibility. And what would serve as a better rationale for their moral reforms in government than an abstract appeal to the higher law of administration? It seemed just the sort of "moral tonic" necessary to cure the frayed nerves of government, even though no one, not even Woodrow Wilson, was quite certain what the study of administration really entailed.

The Johns Hopkins Lectures: The Continuation of Wilson's Administrative Thought

"The Study of Administration" established Wilson's reputation as an administrative scholar and for the next nine years (1888-96), he was

²⁶ In a letter to Almont Barnes, Wilson himself says of the essay that it was "too broad, too general, too vague," as appearing in *The Papers of Woodrow Wilson*, 1886-88, 5, 518-21.

invited as a visiting part-time lecturer on administration at The Johns Hopkins University.²⁷ Many of the remaining lecture notes are only fragmentary and, of necessity, are frequently repetitious. His notes, however, indicate an expansion of themes first developed in his original essay. Increasingly, Wilson tended to view administration as caught up in the complex web of the political and legal fabric of society. As he stressed in 1891:

No topic in the study of government can stand by itself—least of all perhaps administration whose part it is to mirror the principles of government in operation. It is not a mere anatomy of institutions. It deals directly, indeed, and principally with the structural features and operating organs of the state life. . . . Administration cannot be divorced from its intimate connections with the other branches of Public Law without being distorted and robbed of its true significance. Its foundations are those deep and permanent principles of politics which have been quarried from history and built into constitutions. . . .²⁸

Eventually his lectures ranged over the whole field of government, in particular examining the comparative aspects of European political systems, for, as Wilson believed, "The problems of administration are . . . in a very real sense universal, international. A wide examination of governmental organs will discover, not only the differences which may exist between government and government, but likewise the general likenesses between them."

While Wilson expressed the desire to move in his lectures "from a description of the existing organs and machinery of administration to a discussion of its standing problems, its general tenets of efficiency, its essential bounda-

²⁷ Wilson's administrative lectures were given in three cycles of three years each (1888-90, 1891-93 and 1894-96). They were originally intended to develop the topic in a systematic fashion, but this approach soon proved impossible because every year new students entered the course. Wilson normally gave these graduate lectures at Johns Hopkins during February. Fragments of the lecture notes appear in *The Papers of Woodrow Wilson*, Vols. 5-8.

Concurrently, while giving the Johns Hopkins lectures on administration, Wilson during his first three years at Princeton (1890-93) taught a similar course there on administration. The course is listed in the Princeton Catalogue of 1890-91 as follows: "Administration. Lectures and collateral readings. Two hours a week . . . Senior elective and open to Graduate Students."

When Wilson taught administration at Princeton and Johns Hopkins, only two other schools in the United States (Columbia and University of Pennsylvania), were offering courses in the field. Refer to Anna Haddow, *Political Science in American Colleges and Universities, 1636-1900* (New York: D. Appleton-Century Co., 1939), pp. 180-95.

²⁸ *The Papers of Woodrow Wilson*, 1890-92, 7, 114-15.

ries," his lecture notes, perhaps because he initially staked out such extensive boundaries for the field, failed to go very far beyond description.²⁹

Conclusion

"The Study of Administration" is at best un-

²⁹ At one point in his lectures in 1890, however, he did recommend a system of local government similar to the commission form of municipal government, first instituted in Galveston, Texas in 1901. See *The Papers of Woodrow Wilson, 1888-90*, 6, 501-502.

Despite his auspicious and enthusiastic beginning in the field of administration and despite the acclaim from his own contemporaries, Wilson did not continue writing on the subject. His subsequent works such as *The State and Constitutional Government in the United States*, while touching on administrative problems, mainly were devoted to broader political subjects. As Alan Altshuler has noted: "Wilson did not consider devoting his scholarly life to the refinement of administrative techniques. He left that to more prosaic minds." Wilson did, however, later become quite a dynamic and innovative practitioner of the administrative arts as President of Princeton (1902-10), Governor of New Jersey (1910-13) and President of the United States (1913-21). For several interesting assessments of Woodrow Wilson as a practicing administrator, see Louis Brownlow, "Woodrow Wilson and Public Administration," *Public Administration Review*, 16 (Spring, 1956), 77-81; Henry A. Turner, "Woodrow Wilson as Administrator," *Public Administration Review*, 16 (Autumn, 1956), 249-57; and Henry A. Turner, "Woodrow Wilson: Exponent of Executive Leadership," *Western Political Quarterly*, 4 (March, 1951), 97-115.

derstood as a response to the issues Wilson raised in *Congressional Government*. His first book had pointed up the defects of congressional dominance of the federal government. Consequently he wrestled with the question of how to restore balanced Madisonian government, strengthen the executive branch, and check the political abuses of Congress. While he toyed with British cabinet government as a remedy for the inadequacies of the American system, his inherent Darwinian conservatism made him reluctant to propose any foreign solutions. "The Study of Administration" was therefore Wilson's pragmatic and indigenous method for restoring "good government."

"The Study of Administration," however, raises more questions than it resolves. Wilson's essay and later lecture notes fail to amplify what the study of administration actually entails, what the proper relationship should be between the administrative and political realms, and whether or not administrative study could ever become an abstract science akin to the natural sciences. Wilson remained ambivalent on so many points that ultimately a modern reader can only conclude, as did Leonard White, that "Wilson introduced the country to the idea of administration."³⁰ To argue that he did more than this would exaggerate the influence of his essay.

³⁰ Leonard D. White, p. 46.

Woodrow Wilson to 1902: A Review Essay*

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Compilation of the Wilson Papers began in 1958. Twelve volumes have thus far appeared, bringing the record to mid-1902. By that date, in his forty-sixth year, Wilson had reached the culmination of his career as professor and was about to assume the presidency of Princeton University.

The *Papers*, chronologically arranged, record little prior to 1873 when, at age seventeen, Wilson entered Davidson College. Thus, since they reflect neither boyhood nor early youth—except the daydreams of a teenager who fancied himself now admiral, then general (of the British navy and army respectively)—they yield little against which to test the supposition of a stern, if loving, father, who exacted perfection from an admiring, though resentful, son. But neither is such supposition precluded. The Georges (*Woodrow Wilson and Colonel House: A Personality Study*, 1956) may be right in ascribing to Wilson a love-hate attitude toward his father and—in terms of subsequent bridling against threats to his ego—deducing therefrom a psychological explanation of the repetitive pattern of triumph followed by fiasco in his Princeton presidency, his governorship, and, finally, in his peacemaking.

Though lacking in anecdotal accounts of early family life (for which Ray Stannard Baker's biography remains the chief source), the *Papers*, beginning with Wilson's year at Davidson, contain many letters from home, but regrettably without counterpart of Wilson's to his parents, since hardly any have survived (that of 16 December 1888 being the earliest among the very few exceptions). With Woodrow away from home, his health, comfort, and spirits were objects of a mother's solicitude, while of exceptional interest are the letters of Joseph Ruggles Wilson to his son. A minister, leader in the southern Presbyterian church, a man of intellect and wit, and of quick, forceful expression, the elder Wilson's letters portray an unusual relation between father and son. There is in these letters no suggestion of constraint or of reticence. Quite the opposite, the impression is of an unembarrassed opening of mind and heart. To be sure, the unabashed terms of affec-

tion are exceptional in today's idiom. Used by the father, however, they are hardly open to the interpretation of disguise for unacknowledged hostility, though such protestation by the son would, of course, be subject to a psychological reading. Even so, such address, it must be emphasized, was common to all the correspondence among the Wilson kith and kin, and it seems also to have reflected a wider currency in the upper middle class of the Victorian South.

In the fall of 1877, Joseph Ruggles Wilson, sending ten dollars to Woodrow at Princeton ("until the Church replenishes my own small treasury"), wondered whether a new overcoat was essential to his son's health and comfort. "If so you shall at once have it. If not—wait a little. Be perfectly candid—as I am. Our interests are *one*. I will sacrifice anything for you, as I am sure you will for me." (I,318. Italics in original.) That father and son were indeed one in mutual regard and affection is so strong an impression that the burden of proof is on him who would read the record otherwise. Nor does young Woodrow's need for frequent reassurance in his strivings, nor his dyspepsia, so severe as to cause a year to elapse between Davidson and Princeton, and as to result later in his withdrawal after a year and a half of law at the University of Virginia, necessarily argue to the contrary, even though they suggest a high degree of emotional dependence on his parents. There can be no doubt that Wilson's was a highly complex personality—a thread that destiny was to weave into the fabric of American history. Yet, it remains conjecture that unconscious reservation attended his many laudatory references to his father, as, for example, in the dedication to *Congressional Government*: "To His Father, the patient guide of his youth, the gracious companion of his manhood, his best instructor and most lenient critic, this book is affectionately dedicated by the Author." [To which Joseph Ruggles Wilson responded: "My precious Son—Your book has been received and gloated over. The 'dedication' took me by surprise, and never have I felt such a blow of love. Shall I confess it?—I wept and sobbed in the stir of the glad pain. God bless you, my noble child, for such a token of your affection" (IV, 208).]

A widower since 1888 and long in poor health, Joseph Ruggles Wilson died in 1903. In the preceding years, a querulous note now and

* Arthur S. Link and associates, eds., *The Papers of Woodrow Wilson* (Princeton, N.J.: Princeton University Press, 1966–1972, Volumes I–XII. Pp. 8171. \$240.00 [\$20.00 per volume]).

again crept into his letters. Yet neglect—if not wholly the plaint of age and illness—can readily enough be attributed to his son's exceedingly busy life, while the continued warmth and solicitude in Wilson's references to his father do not in the least suggest filial piety that sprang merely from duty.

That Wilson was frail of health and psychologically complex the *Papers* confirm. Any psychoanalytical deduction therefrom continues, however, to be highly speculative, and, with the record now as nearly complete as it will ever be, is destined so to remain—though one must summarily reject the grotesque allegations of Bullitt and Freud (*Thomas Woodrow Wilson: A Psychological Study*, 1965). Only animus can explain so improbable a contention, as is there entertained, that a defective personality—assuming the mantle of Jesus Christ—was Wilson's chief stumbling block preventing realization of his Fourteen Points at the Paris Peace Conference.

Wilson's method as political scientist was (within the confines of Western civilization) historical and comparative. His scholarly reputation has rested primarily on *Congressional Government*, although *The State*, a text treating the origins of government and law and comparing certain governments of the time, was used widely in successive editions in the United States and in four translations abroad.

Wilson had just turned twenty-eight when *Congressional Government* was published in January, 1885. Wilson was then in the second of his two years as a graduate student, but the book was quite independent of his studies at Johns Hopkins and was used only *ex post facto* for purposes of a Ph.D.—a degree that Wilson did not particularly covet, but for which he submitted to examination at the end of his first year of teaching at Bryn Mawr. [The fields were history (classical, French, and British and American constitutional development) and political economy (general principles, public finance, American political economy, and socialism) (V, 154–155).] *Congressional Government* was, of course, modeled on Walter Bagehot's *English Constitution*, a realistic account of the functioning of British government. Such an account Wilson attempted for American government, eschewing, like Bagehot, a mere "literary theory" of his subject. With forceful effect, he contrasted government by congressional committee with the ministerial direction and party responsibility of the British political system. That *Congressional Government* greatly influenced the outlook of American political science is evidenced as recently as 1950

in the report of the APSA's Committee on Political Parties: "Toward a More Responsible Two-Party System." In actual practice, however, the reforms envisaged by Wilson have not occurred. Even the erratic consequences of the congressional rule of seniority are today unabated. To be sure, there have since been notable instances of presidential leadership of the legislative branch—unaided, however, by any institutional alterations. Despite its perversities, American separation of powers persists, and, as our bicentennial approaches, the *Federalist Papers*, whose dedication to separation of powers Wilson regarded as doctrinaire, increase rather than diminish in esteem.

Continuing professional recognition is also accorded "The Study of Administration" (V, pp. 359–380), published in 1887, an essay that marks the beginning of public administration as a subject of inquiry in American political science. The *Papers* now reveal that Wilson himself—without having written a book on the subject—pioneered the field with thoroughness and originality. From 1888 through 1897, he lectured on administration four to five weeks each winter at Johns Hopkins. Extending through a three-year cycle consisting of twenty-five lectures a year, the course was comparative in approach, using the German and French administrative systems to gain perspective on the American, rendered peculiar in contrast both by its British antecedents and by its novel constitutional arrangements. The lecture notes (chronologically distributed in Volumes V through VIII), though in some years fragmentary, are on the whole sufficiently systematic and detailed (with considerable emphasis on urban government) to afford a good understanding of Wilson's treatment of the subject.

Until his graduate study, British history and biography had dominated Wilson's reading. Such, however, was American regard for German scholarship that it was incumbent on Wilson to learn the language, which he did at Hopkins on his own, though not well enough "to be emancipated from the constant use of the dictionary in reading it" (V, 385). In 1886 he laid plans to spend as much as two years at German universities, improving his facility in the language, attending lectures, and engaging in "wide observation of men and things" (V, 385); but Mrs. Wilson's second pregnancy intervened. Wilson's exploration of the field of public administration was heavily dependent on German sources, as was the content of *The State*, while his course bibliographies at Princeton were replete with German references. Given the comparative approach to law and

government typified by Wilson and considering the European-centered world of his time, one clearly sees the origin of German and French as the "tool skills" that have for so long attended advanced study of political science in the American university.

Wilson's preoccupation with literary style cannot go unremarked. It was an obsession. Combined with an incredibly facile pen, it often resulted in tiresome rhetoric: language that one reviewer of *Division and Reunion* characterized as "overwrought" (VIII, 346). Though Joseph Ruggles Wilson was Woodrow's model, the father's style was by comparison the more economical and muscular. In a fascinating letter of 12 September 1887 (V, 587-588), the father—gingerly and only at Woodrow's insistence—criticized an article of his son's. "You have too much wealth in use for the purpose in hand," he said, "and hence you have only to appear poorer in order to appear stronger. . . ." "You are not stingy enough . . . in your acts of creation." "Perfection is within your reach if only you keep well to the road your already well-groomed steed which is too much disposed to go afield."

Nor can one leave unmentioned the hundreds of love letters, each a fresh and ardent reiteration of a mutual dependence that never faltered. Ellen Louise Axson, educated largely at home, a woman of native intelligence and artistic sensitivity, was no less articulate than Woodrow. Moreover, Woodrow's letters—immune to the temptations of formal composition—are notable for their descriptive power and force of characterization, including self-analysis (see especially letters to Ellen in the year 1885). A remarkable corpus simply as love letters, they also afford intimate insight into the prevailing attitudes (on woman's status, race, the South, etc.) and conditions (salaries, prices, health, transportation, etc.).

Unlike Jefferson's, Wilson's is not the seminal mind that would in itself warrant so vast an undertaking as that involved in the assembly and publication of his papers. Compared to Lincoln's utterances, moreover, Wilson's fall short in style and poignancy. Yet the sheer ex-

istence of so extraordinary a record of a major public figure, one as talented and articulate as Wilson, invites and justifies the enterprise in which Professor Link and his associates are engaged. One is curious, of course, about the shaping of Wilson's thought; and, in fact, the *Papers* afford the basis for understanding his idea of democracy. Even more, however, one is beckoned in his reading by the human interest contained in the *Papers*, for they are a window on another era.

Thus Francis Landy Patton, the last Princeton president in an unbroken line of Presbyterian clergymen, congratulating Wilson on his election by the Trustees (February 1890) to the Chair of Political Economy and Jurisprudence, alluded to "one or two criticisms that I have heard regarding your work on the State."

. . . in your discussion of the origin of the State you minimize the supernatural, and make such unqualified application of the doctrine of naturalistic evolution and the genesis of the State as to leave the reader of your pages in a state of uncertainty as to your own position and the place you give to Divine Providence. More particularly have I been reminded of the fact that while you have devoted inordinate space to Roman law and while you credit Roman law with its full share of influence upon the regeneration of modern society, you are silent with respect to the forming and reforming influences of Christianity.

The Trustees, Patton said, "mean to keep this College on the old ground of loyalty to the Christian religion," and would "not regard with favour such a conception of academic freedom or teaching as would leave in doubt the very direct bearing of historical Christianity as a revealed religion upon the great problems of civilization" (VI, 526-527).

I wish finally to commend the meticulous job of editing: annotation of the *Papers* is complete and the index both thorough and helpful. Meanwhile, the great bulk of the *Papers* still lies ahead, for in 1902 Wilson had just begun to catch the public eye.

EDWARD H. BUEHRIG

Indiana University

BOOK REVIEWS

Critical Interruptions: New Left Perspectives on Herbert Marcuse. Edited by Paul Breines. (New York: Herder and Herder, 1971. Pp. 188. \$5.50.)

Herbert Marcuse. By Alasdair MacIntyre. (New York: Viking Press, 1970. Pp. 114. \$1.65.)

The collection of essays edited by Mr. Breines is dedicated "to Theodor W. Adorno and Ho Chi Minh." This curious juxtaposition tells one more about the American New Left than whole volumes of criticism. Adorno in his last years had become the prime target of neo-Stalinist student activists in the West German Federal Republic, primarily because he refused to subscribe to the fashionable cult of the Third World. It is arguable that on this point Marcuse has taken a different position, but the dedication permits misleading inferences to be drawn about the Frankfurt school of neo-Marxism founded by Max Horkheimer, Theodor Adorno, and Herbert Marcuse in the 1950s and currently represented by Jürgen Habermas and Alfred Schmidt. It is of the essence of the matter that all these writers rejected the Leninist interpretation of Marxism, whereas Ho Chi Minh—like other notable Third World figures—was not merely a primitive Leninist, but an active Stalinist who *inter alia* massacred all the Trotskyists he could lay hands on. The attempt to link Frankfurt with Hanoi is either naive or disingenuous. It is also at variance with the tone of Mr. Breines's introductory essay where one may read:

At present the New Left appears to have utterly and decisively freaked out—and it may have. Normal and intense factional debate has not only suddenly been replaced by a blaring carnival of fetishized and mind-clogging rhetoric, but the rhetoric itself is "new." Actions and theories are now upheld or denounced in the name of Marxism-Leninism, proletarian internationalism, revolutionary discipline, the working class, the Black Panther Party, Chairman Mao, the National Liberation Front of Vietnam, the dictatorship of the proletariat, the seizure of state power, armed struggle and, here and there, Stalin, Georgi Dimitrov, and the Peoples' Republic of Albania. At least momentarily, genuine *auto-critique* or critical self-reflection is scarce.

The title of Mr. Breines's essay is "From Guru to Spectre." That Marcuse has remained a guru even for the most sophisticated faction of the New Left may be inferred from Shierry M. Weber's essay, "Individuation as Praxis,"

which restates the basic anarcho-libertarian message of Marcuse's *One-Dimensional Man* (1964), the work that turned him from a cloistered scholar into a popular messiah. What is wrong with the world is that "at present technology is subordinated to capitalism and capitalist rationality" and this subordination breeds a type of conflict which can be discussed in Freudian or Reichian language: "Capitalism perverts modern technology by turning it to false purposes, false in terms of the basic value, the notion of man discussed at the beginning of this essay. The resulting system serves not Eros, the integrating life forces, but the death instincts" (p. 31). Soviet state ownership presumably has different consequences for the individual.

It was the peculiar achievement of Marcuse in the 1930s to amalgamate a few carefully selected Marxian concepts with an ontology derived from his teacher, Martin Heidegger. The final implication of this remarkable synthesis is now translated into New Left language by the contributors to this symposium, for example, by Russell Jacoby, who writes:

To follow Marcuse, the necessity of subjugating Nature entailed the subjugation of man himself: this historical "project" deformed from the beginning the subject/object, man/nature relation. . . . Violence against nature is sustained by violence against man. "The ego," Marcuse tells us in *Eros and Civilization*, which undertook the rational transformation of the human and natural environment revealed itself as an essentially aggressive, offensive subject, whose thoughts and actions were designed for mastering objects. It was a subject *against* an object (p. 68).

While Marcuse relied on Heidegger, Jacoby relies on Spengler. "If, as Spengler has written, imperialism is civilization unadulterated, Nature was the first colony" (p. 68). This surprising reference to the principal theorist of technocracy is followed by a quotation from Rosa Luxemburg concerning the ill-treatment of animals. Syncretism could hardly be carried further.

Where the volume under review serves a useful purpose is in acquainting the reader with Marcuse's adherence to the Frankfurt school of neo-Marxism originated by the Institute for Social Research set going by Horkheimer in 1932. Unlike Marcuse, Horkheimer and Adorno are barely known outside Germany, which is a pity, since they represented the core of the Institute. Horkheimer's essays, originally pub-

lished in the *Zeitschrift für Sozialforschung*, have now been reproduced in book form and may eventually reach the English-speaking world. Contrary to legend, the Frankfurt school was not significantly influenced by Lukács. Its interpretation of Marxism was pioneered in the 1920s by Karl Korsch, whose *Karl Marx* (1938, reprinted in 1963) is still one of the best introductions to the subject. It was Korsch who rescued the authentic Marx from the Leninists, while Lukács threw in his lot with Soviet orthodoxy. William Leiss's essay in the volume under review deals at some length with Horkheimer, but fails to bring out the insurmountable cleavage that existed from the start between the Frankfurt group and the Leninists. This cleavage was rooted in the commitment of the Frankfurt school to the heritage of classical German philosophy, whereas Russian Marxism from the start found its principal inspiration in the materialism of the French Enlightenment—a materialism which is both contained and transcended in the writings of Marx. The pre-Hegelian, even pre-Kantian, character of Soviet philosophy was duly stressed by Korsch in his influential *Marxism and Philosophy*, recently translated into English and published by the British branch of the New Left. It is the starting-point for any rational discussion of the subject.

Some of these topics are pursued with great intellectual vigor in Alasdair MacIntyre's critique of Marcuse, published in the *Modern Masters* series. MacIntyre is particularly enlightening in noting the remarkable resemblance between Marcuse and the Left Hegelians of the 1840s from whom Marx had to emancipate himself before he could become the theorist of social revolution. As MacIntyre very pertinently observes, "we ought at least to consider the hypothesis that Marcuse is not a post-Marxist thinker" (p. 22). It is indeed remarkable how extensively some of the contributors to *Critical Interruptions* reproduce the problems of the Berlin Hegelians in the 1840s, a circumstance for which Marcuse must be at least held in part responsible. MacIntyre does not pursue this theme very far, being more concerned to stress the incompatibility of Marcuse's thinking with his own rugged empiricism. But he does bring out very well a notable peculiarity of Marcuse's theorizing: his tendency to characterize the difference between Hegel and Marx in terms of his own commitment to the notion that the idea of reason has been superseded by the idea of happiness. As MacIntyre notes, Marcuse does not produce a single quotation

from Marx to support this view, for the excellent reason that Marx never adopted the idea of happiness as a human goal. The simple fact is that Marx was not a utilitarian, though some later Marxists—notably Kautsky—were. To quote MacIntyre,

... most [Marxists] have been clear that freedom is a goal which may be incompatible with the goal of happiness. Trotsky's view that the gap between aspiration and achievement will be a permanent feature of human life, so that tragedy will be permanently relevant to the contemporary human experience, seems far more faithful to Marx's view than Kautsky's was. Moreover on this point Marx and Trotsky are surely right and Marcuse surely mistaken (p. 37).

He might have added that Marcuse's chief predecessor in this domain was neither Hegel nor Marx, but Ludwig Feuerbach. To the present reviewer, at any rate, it appears fairly obvious that in Herbert Marcuse the American New Left has found its Feuerbach. It is still awaiting its Marx.

GEORGE LICHTHEIM

Late of London

Law, Order and Power. By William J. Chambliss and Robert B. Seidman. (Reading, Massachusetts: Addison-Wesley Publishing Company, 1971. Pp. 533. \$12.50.)

This is a pretentious, mediocre book. Its authors, a sociologist and a law professor, are said to have benefited by a fellowship held by the senior author at the University of Wisconsin under the auspices of the Russell Sage Foundation. I'm afraid that the authors, in this *olla podrida*, have read not wisely but too widely and too narrowly.

The book starts out, after an innocuous statement of perspective, with a part on the creation of formal rules of law. After a bow to anthropology, they focus on whether law can be value-free. Of course, this discussion was bound to be tedious. The question can be answered both ways, depending on the level chosen. The authors first take up models (one of their favorite words) denying value choice. One is quite startled to encounter natural law under this heading. One's surprise is intensified when the authors talk about positivism as a theory that asserts value-choice by lawmakers. Then they get into a real morass when they talk about legal realism. To talk about legal realism when one is still discussing positivism vs. natural law reminds me of the man who wondered whether to go to Europe next winter or by air.

Legal realism can be consistent with either natural law or positivism. Most Americans have been conditioned by their legal system to be natural lawyers. British thinkers, on the other hand, who are not accustomed to a court's denying validity to certain acts of the legislature, tend to be positivists. It is an interesting paradox that most legal realists are natural law types.

Following this unfortunate venture into jurisprudence, the authors take up law-making institutions. Here it is unfortunate that they display no familiarity with the most influential, unpublished book on what I call applied jurisprudence, *The Legal Process* by the late Henry M. Hart and Dean Albert Sacks of Harvard Law School. That work asks what functions can be undertaken by courts, what functions by legislatures, what functions by administrative agencies, and finally (but first in their presentation) by the parties to transactions themselves. This last kind of law-making Hart and Sacks call "private ordering." Their scheme is one that cannot be ignored by anyone who addresses the problems of what can be done by legal institutions. Yet the authors ignore it completely and therefore are forced into a bland rehash of what political science has done to the study of legal institutions. On courts, what political science has done is to count, but not to think. This the authors faithfully parrot. It would take more than a book review to state what the deficiencies of that approach have been.

The authors next turn their attention to the substantive criminal law. In this shortest part of their book, they succeed in making in a scant 85 pages just about every mistake that can be made. Their discussion of *mens rea* is laughable: the centrality of the idea of the mental element in the analysis of crime has entirely escaped the authors. This lacuna leads them into an uncritical acceptance of the now discredited theories of Edwin Sutherland concerning so-called "white collar" crime. In their only reference to the contemporary debate between classical criminal law theorists and advocates of sociological theory, they unhesitatingly award the prize to the latter, without so much as discussing the contrary arguments.

The remaining part of the book professes to be concerned with "the implementation of law." Actually, it is almost wholly a tired recapitulation of what has been written about the criminal process. They pay far too much attention to what I would describe as the "official" legal version, the work of the National Crime Commission. This is interlarded with strong tributes to the work of what I would call "radi-

cal" sociologists (like Jerome Skolnick) to lend an air of being up-to-date, and therefore relevant. That too would require a separate essay to criticize. In their final chapter, they deal with what I would call "disorganized crime." They submit entirely to the official theories, which have been sanctified by the National Crime Commission, by the sociologist Donald Cressey and by the political scientist, James Q. Wilson. That there are contrasting theories does not occur to them (if one excludes a reference to Morris and Hawkins).

HERBERT L. PACKER

Late of Stanford Law School

Shape of Community: Realization of Human Potential. By Serge Chermayeff and Alexander Tzonis. (Middlesex: Penguin Books, 1971. Pp. 247. \$2.95, paper.)

The authors mean to impress us with the fact that solving urban problems is a complex business. They are especially concerned that techniques employed in solving these problems are not used to define their solutions. Thus, they stress the importance of commitment as a prior condition to the use of techniques. The authors' commitment is toward humanizing life in urban areas. Man, as they see it, has become a victim of his own technology. While this victimization has occurred as a consequence of an evolutionary process, they argue that man must now plan for the uses of technology in order to attain desirable "sociological ends." Further, the aim of planning is to transform conflicting elements into complementary ones. They warn us that, "The most promising method for preventing ecological mayhem, if not the extinction of all living things on earth, undoubtedly must start with the planning for an excellent and comprehensive urban compactness" (p. 195).

The authors argue that planning for such transformations requires that all of the factors that constitute a given problem are sorted out and made explicit. They propose a "lattice-grid" approach as an aid to establishing priorities and clarifying solutions for problems. More specifically the aim is to relate human goals to technological means at different levels of interaction. The result is a "complementarity of goals, complementarity of organizing principles and complementarity of operational components irrespective of scale" (pp. 210-211).

The desirable human goal for the authors is the construction and maintenance of communities. By "community" they do not mean a geographically delimited area in which interpersonal relations are relatively stable and intense. Rather, they have in mind "places of con-

course" to offset the dehumanizing effects of urban movement and transportation flow. They believe that such places should be devised along transportation routes so that pedestrians can "mingle and thus have a sense of community." Such places, the authors believe, can be made complementary to the human need for solitude and to the technological requirements of movement. The reader thus conjures up a vision of an area where people promenade, enjoy each other's presence, and take an avid interest in the shops and displays along the concourse. The authors' motives are worthy.

What is not so worthy is that it takes 244 pages, including appendices and a glossary, to tell us about the value of concourses, why we need them, and how to plan for them. The reader must have extraordinary perseverance to discover that this is really what the book is about. He must, in the process, deal with wide-ranging, disconnected commentaries, annoying metaphors and analogies, and obscure jargon.

The book opens with an account of technologically befouled ecology of man. Then comes a treatise on commitments to human goals. This is followed by a quick overview of evolution and by the authors' judgment that the four evolutionary "clocks" of biology, methodology, sociology and technology are not sufficiently synchronized; the principal problem as they see it is that sociological understanding has failed to keep pace with technological advances. Next, the reader is served a jargon-laden discussion on models, the problems of urban planning, and the myopia of urban planners and allied professions.

The graphic presentation of the text seems needlessly confusing. The chapter and section headings are printed in the left-hand margins, where they seldom caught this reader's attention. Footnotes and inspirational quotations are also entered in the margins, perpendicular to the text. It's rather annoying to have to turn the book sideways in order to read them, but apparently this is all part of the authors' master plan. What better way to convey the complexities of urban problems than to make the text and its presentation convoluted? As the authors state in the prologue, "he [the reader] is invited to become, as it were, a functionary to help structure the complex urban order with us and in so doing help overcome the restrictions of a linear narrative system" (xxvi). The prologue itself is presented in "random assemblage form" in an attempt to help overcome the difficulties of "one-dimensional representation of a multi-dimensional order of great complexity" (xxvii). Unfortunately, both the content of the

book and its mode of presentation seem far more pretentious and complex than the issue of "concourses" warrants.

JOSEPH SMUCKER

Sir George Williams University, Montreal

The Child's Construction of Politics. By R. W. Connell. (Carlton, Victoria: Melbourne University Press, 1971. Pp. 251. \$4.95.)

When I first received this book I flipped through the pages, much in the manner of most of us when trying to make a quick judgment about a new work. My initial judgment, based on this quick sampling, was not altogether favorable. I was upset primarily by what seemed to be an inordinate quantity of verbatim interviews. In some portions these materials continued for several uninterrupted pages. Now, virtually everyone doing research in political socialization uses interviews, either of the fixed format, survey variety or the in-depth, probing type. In addition, we all know that it is fun and even enlightening to listen to children's portrayals of the political world. Still, do we want to wander through a book saturated with dialogues between an adult investigator and children ranging in age from five to sixteen? After all, it isn't too difficult to conduct such dialogues on our own; and besides, case materials can quickly lead to an obsession with the idiosyncratic—all very interesting, but where does it take us in any systematic sense?

Happily, I dutifully tried to overcome this initial impression and proceeded to give the book a "fair" trial. While I still have strong reservations about the ratio of case materials to discussion and interpretation, I am nevertheless persuaded that this is a tremendously important work. It is, in the first place, the first major report which attempts to develop a theory of political learning. True, there have been preliminary efforts and hints, but I think no other work links up data with learning theory in such a clean, articulate fashion. A second major virtue is that it shows how what are essentially case studies can be used in a rigorous, systematic fashion. Again we have some precursors, notably Greenstein in political science and Adelson et al. in psychology. But Connell, partly because he takes a full book to do it, comes closer to illustrating the rich potential of the method.

The raw materials for the undertaking come from extended interviews with 119 children living in various suburbs in Sydney, Australia. As the protocols amply reveal, a direct, nonthreatening approach was used in the tape-recorded interviews. Surprisingly, perhaps, the fact that

these are Australians rather than Americans, Germans, Japanese, etc., seldom gets in the way of the analysis. Obviously, the names and places make a difference, but not much. More significant is the geographical location of Australia and the political history born out of that location. Specifically, there is the "threat" phenomenon—the worry about being invaded and conquered by alien forces—which has conditioned Australia's children for generations. Apart from this element and the fading presence of the British crown, few culture-specific attributes limit the wider application of the work.

But the major reason topical and country-specific elements do not reduce the general usefulness of this study is that the author is primarily trying to erect a theory of preadult political learning. It is the how rather than the what of learning which forms the central focus. As with any good general theory, this one allows for substitutability of particulars. Connell is heavily influenced by the stages outlined by Piaget and by other cognitive learning theorists. To this end, and drawing directly on the case materials, he constructs a two-dimensional paradigm of political learning. The first dimension has to do with the stages of learning and is divided into four more or less age-graded categories: intuitive thinking, primitive realism, construction of the political order, and ideological thinking. Accompanying this essentially cognitive development is one more affective and evaluational in nature, what Connell calls "stances." This is the more innovative conceptualization and consists of two classes. The first is marked by a view that politics is nonproblematic, and in this stance most judgments are of a random, unqualified, *ad hoc* nature. In the second stance politics is recognized as being problematic; preferences come into being, alternatives are advanced, the interconnected nature of stances is often recognized, and the problematic nature of one's own political behavior is identified. In the nature of such a paradigm, the contribution of the self to political learning looms large. While the indebtedness of Connell to the developmental and maturational theorists is thus apparent, it would be a mistake to say that he had simply applied their precepts to political learning. He makes clear the difference between learning about the physical world versus the political world, and it is, I think, a fundamental difference.

While succeeding admirably at the micro level, Connell fares less well when turning his attention, ever so briefly, to societal matters. He shares with others the dissatisfaction with

what he calls the "... twofold preoccupation with induction into norms and the stability of systems" (p. 234) characterizing much of the political socialization literature. His proposed alternative regards the development of political belief "... as a contingent, historical process ... which stretches across historical time and which links events present with events past" (p. 235). Fine, but there is nothing in Connell's admittedly cursory explication of this idea which forges any two-way link between the individual and the political process. It might tell us how historical tradition helps shape the political orientations of the child, but it would seem to go little beyond that. Nor is Connell much more persuasive when talking about social control and freedom, seeming to imply that the Aussie children, compared with those from a number of other countries, are growing up in a much less indoctrinated fashion. I would suggest that indoctrination is far more subtle than Connell admits. What I consider to be shortcomings in these areas should not detract in the least from Connell's major achievements.

M. KENT JENNINGS

The University of Michigan

Bernard Shaw: The Road to Equality: Ten Unpublished Lectures and Essays, 1884-1918. With An Introduction by Louis Crompton. Edited by Louis Crompton with the assistance of Hilayne Cavanaugh. (Boston: Press, 1971. Pp. 348. \$9.95.)

Louis Crompton's discovery in the British Museum of ten previously unpublished essays and lectures now adds *The Road to Equality* to Shaw's *Intelligent Woman's Guide to Socialism* and *The Fabian Essays*. In the first essay, "Our Lost Honesty," written in 1884, Shaw proclaims that England is a slave state. This youthful radicalism comprehends distrust of the British party system, the Crown, the Houses of Lords and Commons, in short, the whole "establishment." But he perceived at an early date the defects in Utopian socialism and communism, which he also thought would not be accepted by the average British citizen. He advocates instead the gradual approach developed by the Fabians. "In the first place, it must be a continuous policy for developing our existing institutions into socialistic ones, and not a catastrophic policy for simultaneously destroying existing institutions and replacing them with a ready-made Utopia" (p. 31).

In "Freedom and the State" Shaw attempts a refutation of J. S. Mill's "Essay on Freedom." According to Shaw the alleged existence of

"self-regarding actions" is a "decaying superstition" since there are no human actions which do not affect other human beings. Mill's position is therefore absurd because the natural rights of individuals are subject to limitations imposed by the rights of his fellows. Shaw thought that no one should have the right to claim an exemption from all moral and legal responsibility for his actions on the grounds of asserted individual rights. "In short, everything that a citizen possesses is held by him on conditions dictated by the community" (p. 125).

According to Shaw socialism is a "state of society in which the entire income of the country is divided between all the people in exactly equal shares, without regard to their industry, their character, or any other consideration except that consideration that they are living human beings" (p. 155). Because all governments are class governments, he advocates the abolishment of class structures by removing class distinctions which stem from inequalities of income. If incomes could be equalized, he suggests, class differences would be removed and real democracy would become possible. Shaw faces the paradox that although the working class and poor constituted a majority of the population and had the right to vote, the British government remained oligarchic, not democratic, in his opinion. Socialists were confronted with the average British working man's "conservatism" and the even "reactionary" tendencies of some labor union leaders. In contemporary America a similar problem emerges when the majority of the population is rich and middle-class, so that the poor are a minority. Rule by the rich and middle-class majority produces support for the capitalistic system rather than for equalitarian socialism.

In "The Climate and Soil for Labour Culture," written in 1918, Shaw provides a solution for the socialistic problem. The most significant factor contributing to socialism's failure in Britain, he holds, is the secondary educational system which produces a gulf between the university man and the wage worker. According to Shaw this discovery of the masses' conservatism made life dangerous for "men of modern culture." "Even Queen Victoria is now too liberal for us" (p. 293). The solution Shaw suggests is the extension of Fabian control over the educational system in combination with the invention of what he calls a religion of "creative evolution," a scientific religion dedicated to carrying man to the point where he attains a power and wisdom which would "now be called divine." "Socialism must have a positive reli-

gion, characteristic of and proper to the epoch which it is to inaugurate, with articles of faith and commandments based on it and accepted as the foundations of the Socialistic State" (p. 323).

Shaw's essays are marked by intellectual consistency. He defends the collectivistic state, the expansion of governmental power, and the economic, cultural, and political advantages of socialism. His wit, his humanity, and his insight into the foibles of human nature enliven the "dismal science." Despite his well-deserved reputation as dramatist, wit, and literary critic, he is not likely by virtue of these lecture-essays to be considered also a major political-economic theorist. Although their subject is supposedly economics, these essays consist mainly of summaries and commentaries on the ideas of others. Shaw offers no original theories and no empirical references to economic conditions in his own day. He does not move to discuss the ethical-moral-philosophical basis for his conclusions. Why, for example, is it better for the working class and poor to get a larger piece of the pie than the capitalist, and x, y, or z? Why assume the ethical-moral superiority and exclusiveness of economic equality as *the* definition of social justice? Could justice mean something more than or different from equality of incomes? Shaw's work provides no answers to these questions; this, in my estimation, is its essential weakness.

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Philosophy and Political Action. Edited by Virginia Held, Kai Nielsen, Charles Parsons. (New York: Oxford University Press, 1972. Pp. 282. \$2.95, paper.)

The present collection of essays is sponsored by The Society for Philosophy and Public Affairs, organized in 1969 by a group of philosophers intent on making concern for public issues a part of the activity of their profession. As one of the editors, Professor Charles Parsons, points out, the concern expressed in the title reflects the climate of political activism that has swept the academic world in recent years. He might have been better advised to refer to the weather than to the climate; rapid changes have occurred in the academic world during the last two years and one may well wonder whether today the same sense of urgency would have inspired the editors who assembled these essays—not to mention the authors themselves.

That Noam Chomsky, author of one of the

essays, should be hailed by the editors as an "inspiring example" of what can be accomplished in the area of philosophy and political action is indicative of the orientation of the book and the organization that sponsors it, although there is no party line and at least two of the contributors, Peter Caws and Robert Simon, hardly take positions that would bring joy to an activist's heart. Clearly editors and contributors alike would reaffirm Marx's celebrated eleventh thesis on Feuerbach: "The philosophers have only *interpreted* the world . . . ; the point however is to *change* it."

The volume is divided into three parts. In the first section, dealing with the issue of peaceful or violent reconstruction of society, Kai Nielsen supplies a precise definition of the difference between reform and revolution, finds that they are on a continuum, and explores the morality of recourse to revolutionary violence. Many readers will find the debate between advocates of reform and violent change a stale one and will wonder if anything new has been added in this essay. Popper and Marcuse are enlisted, the former in opposition, the latter in support of the possibility of a moral justification of revolution. Nielsen takes the side of Marcuse. However, Nielsen repudiates, as does Caws, the infantile leftist so confident of his ability to predict the future that he welcomes the worse as a necessary prelude to the better. But the very reason Nielsen gives is one of the best arguments I know for repudiating the flirtation or infatuation—as the case may be—of many adherents of the New Left with violence: "Our ability to make long-range social predictions is so slight that it is irrational to develop any considerable confidence concerning claims about how things must be" (p. 50).

For those of Nielsen's persuasion, Joseph Margolis, in discussing the possibility of an ethical defense of violence and destruction, must be a dubious ally for the very reason that prompts Nielsen to reject Popper. Margolis, one of a declining number of noncognitivists, rejects the possibility of objective ethical appraisal. Not only are there no values that all men support, there are no values that it can be said they *ought* to support. Consequently, individuals may rationally advocate the wholesale destruction of life: ". . . their claim may be disputed by partisans of another persuasion, but their claim is not obviously self-contradictory, incoherent, or morally ineligible" (p. 69). Clearly, Margolis's metaethic is unlikely to appeal to the average revolutionary convinced of the objective rightness of his cause.

A second section of this collection is concerned with civil disobedience and other methods short of violence for defying the state. Virginia Held finds that political protest by way of the ballot has been ineffective and suggests that the technique of the strike, since its efficacy has been established as an economic weapon, might be used with equal effectiveness as a political weapon. Such "political" strikes, which Professor Held distinguishes from civil disobedience, would be designed to hamper the administration of government and would take such now familiar forms as blocking entrances to public buildings, tax strikes, etc.

Professor Hugo Bedau sticks closer to the philosopher's last. His topic is compulsory military service, particularly whether there is a moral obligation to obey the draft law. This leads him to distinguish between legal and moral obligation, between being *obliged* and being *obligated* to do something, and to conclude that there is no general moral obligation to do what the law obliges us to do; each case must be decided on its merits. Sidney Gendin's is a thoughtful discussion of civil disobedience. The major issue, he contends, is not what circumstances justify civil disobedience, but rather what kind of disobedience a government may and should tolerate. Gordon Schochet concentrates on a specific issue, challenging the generally accepted assumption that a willingness to accept the penalty is intrinsic to authentic civil disobedience.

A third section of this collection raises the sensitive issue of institutional and professional neutrality. Professor Chomsky repeats his now well-known indictment of the American social system and American foreign policy—overdrawn even for the many of us who share his sensitiveness to prevailing social abuses, his hostility to the war in Vietnam, and his condemnation of a foreign policy which too often confuses stability and totalitarianism. This essay is different in that it is addressed to philosophers. Chomsky believes they have a special role to play, suited to their peculiar competence, in working for a "reconstruction of values" and "the creation of a more healthy intellectual community." In calling on the universities to institute programs of "radical social inquiry that would examine the premises of public policy and attempt a critical analysis of the prevailing ideology" (p. 213), Professor Chomsky implies that this is not now being done, an implication that may come as a surprise to some of us who have been spending our lives at it.

If what Professor Chomsky is really calling

for is an abandonment of neutrality by universities and professional associations, he is answered at least in part by Robert Simon's essay defending institutional neutrality and denying (as many activists charge) that the universities are already captives of the "Establishment." What is at issue is not, as L. M. Schwartz seems to suggest in his discussion of the responsibilities of the universities, the propriety in classrooms of a normative appraisal of public policies, nor the impact of moral appraisal on academic detachment—I believe such issues have long been settled—what is at issue is the adoption of official, institutional policies, even on so consensual an issue as our intervention in Southeast Asia. I heartily agree with Schwartz's conclusion (p. 255) that "the overall consequences of politically neutral universities are to be preferred." That goes double for our professional associations.

Stuart Hampshire's concluding essay proposes a program for that one-third of the liberal and radical intelligentsia who, sharing Bertrand Russell's moral disgust with the post-war world (this is a reprint of Hampshire's review of the third volume of Russell's autobiography), say, in effect, "Stop the world, we want to get off." Admittedly lacking a comprehensive social theory, they can at least deprive all adversaries in the present struggle of a critical pool of talent and of the "experimental habit of thought" by which the society of which they are a part is normally invigorated. Such a "moratorium policy of disassociation" is what the student movement of recent years has given us, in Hampshire's view. Those who have not been trapped in a consequentialist (utilitarian) morality are urged to appropriate it and make it their own. Hampshire has given us a vision; perhaps like all visions it should be pondered, not analyzed.

Much as one may disagree with some of the viewpoints expressed in this book, it represents a refreshing departure from recent preoccupations of philosophy.

HARRY GIRVETZ

University of California, Santa Barbara

Paradoxes of Rationality: Theory of Metagames and Political Behavior. By Nigel Howard. (Cambridge, Mass.: M.I.T. Press, 1971. Pp. 248. \$12.95.)

Howard argues that the concept of rational behavior breaks down at three points. The first "breakdown" occurs in two-person constant-sum games lacking a saddle point in pure strategies, such as the following game:

	X	Y
A	1, 2	2, 1
B	2, 1	1, 2

GAME I

If player 1 knew that his opponent would choose strategy X, then he himself would choose strategy B. If he knew that the opponent would choose Y, then he himself would choose A. Likewise, if player 2 knew that his opponent would choose A, then he himself would choose X; and if he knew that his opponent would choose B, then he himself would choose Y. Neither player, however, can possibly know what strategy his opponent will choose if the latter plays the game with any degree of competence, because the main task of each player in this game is precisely to keep the other player completely in the dark about his intentions.

So what should the players do? Conventional game theory suggests that each player should use a mixed strategy, playing both of his pure strategies with probability $\frac{1}{2}$. Howard wants to avoid the use of mixed strategies because they would require him to introduce cardinal utilities. This is a legitimate point of view. But, then, the correct conclusion would have been that for each player it is equally rational to use *either* of his two pure strategies (so long as he takes care that the opponent cannot guess which strategy he will actually use). There is here no "breakdown" of rationality. In fact, there are two alternative concepts of rationality to handle this situation. The stronger one, used by conventional game theory, tells the players exactly what to do (viz, to use a one-half/one-half mixed strategy). The weaker one, which avoids the use of mixed strategies and of cardinal utility, tells the players to use *either* of their two pure strategies. As far as I can see, there is nothing wrong with either recommendation.

What Howard calls the second breakdown of rationality is no "breakdown," either, but it is at least a genuine paradox. It occurs in games called Prisoner's Dilemma games, such as Game II below. (The first example of such a

	X	Y
A	3, 3	1, 4
B	4, 1	2, 2

GAME II

game, devised by A. W. Tucker, was stated in terms of a story involving two prisoners. See R. D. Luce and H. Raiffa, *Games and Decisions*, Wiley, 1957; p. 95.)

If the players can trust each other to keep agreements, then they should be able to agree to use the strategy pair (A,X), which is called the *cooperative solution*. But if they feel they cannot trust each other, then there is no point in reaching such an agreement because presumably it would not be kept, anyhow. Accordingly, player 1 will use strategy B (which will yield him a higher payoff than strategy A would, regardless of whether his opponent chooses X or Y). Likewise, player 2 will use strategy Y (which will yield him a higher payoff than strategy X would, regardless of whether his opponent chooses A or B). Thus, the two players will end up with using the strategy pair (B,Y), which is called the *noncooperative solution*.

Admittedly, it is somewhat paradoxical that two rational players, if they cannot trust each other, should choose the strategy pair (B,Y), even though both of them would be better off by choosing (A,X), since the latter choice would yield a payoff of 3 units to each of them whereas the former choice yields only 2 units. But, on reflection, this is not really surprising. If two rational players cannot trust each other, then it may very well be too risky for them to cooperate very closely with each other; and if they can obtain a higher payoff only by close cooperation, then they may find it safer to forego this higher payoff rather than face this risk.

Howard proposes, however, to show that this conclusion, generally accepted by game theorists, lacks logical force. He introduces the concept of *metagames*, which are games in which the players can make their strategy choices dependent on the strategy choice(s) of the other player(s). Thus, in Game II, both players may adopt what is called a tit-for-tat policy: "I shall cooperate with you if you cooperate with me; but I shall thwart you if you thwart me." (Here "cooperating" means using strategy A or X, while "thwarting" means using strategy B or Y.) It turns out that if both players use such a tit-for-tat policy, then they will reach the cooperative solution of the game because now both of them will have an incentive to use their cooperative strategies A and X, respectively.

The only trouble is that in Game II and similar games, as they are defined, a tit-for-tat policy is not really available to the players because they simply *cannot* make their strategy choices dependent on the other player's strategy choice.

This is so because each player has to choose between using a cooperative or a noncooperative strategy *before* he knows whether the other player will use a cooperative or a noncooperative strategy. The only way the players can make their strategies mutually interdependent is to *agree* on which strategies they will use. But if they cannot trust each other to keep such an agreement, then there just is no way for them to cooperate in achieving the cooperative solution.

Howard's third "breakdown" of rationality is based on sheer misunderstanding. He proves that, in games of a certain type, if a given player has a dominant strategy, then this will induce the *best* possible equilibrium outcome for the other player (Theorem 9 on p. 180). This is the third "breakdown" of rationality. He has overlooked the fact that the theorem would be equally true if the word "best" were replaced by the word "worst": in that particular class of games it so happens that all equilibrium points will yield the *same* payoff to the other player, which is, therefore, both the "best" and the "worst" payoff he can obtain. Thus the third "breakdown" of rationality does not exist.

Howard also claims that his theory of metagames gives reasonably good predictions about people's actual behavior in experimental games. But this claim is hard to judge since his experiments were done on very small samples and the results seem to fall far short of statistical significance. (He does not provide significance levels.) But, what is more important, his theory yields only very weak predictions, so that very little would be accomplished if these predictions were in actuality confirmed by the empirical facts.

The book is well written. The author's ideas are very original and often quite ingenious. It is with regret that this reviewer expresses his disagreement with the author's main conclusions.

JOHN C. HARSANYI

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History as Social Science. Edited by David S. Landers and Charles Tilly. (Englewood Cliffs, N.J.: Prentice-Hall, 1971. Pp. 147. \$5.95.)

This book is one of a series aimed at providing a comprehensive review of the behavioral and social sciences in the USA, so as to serve as the basis for an informed national policy for their development. The inclusion of history within this scheme is courageous; and the panel of historians responsible for the volume are well aware of the difficulties and dangers of misunderstanding which their task involves.

The panels responsible for other volumes, they tell us ruefully, did not have to persuade their colleagues of the importance of their contributions to social science, nor to explain and identify what makes them social scientists. But the authors of this volume have perforce combined two very different jobs. They had to alert their fellow historians to the actual situation—a critical but by no means daunting one—of their own discipline. They have done this remarkably well in the space available, by methods well calculated to stimulate their colleagues to the kind of reflective stock-taking to which, as a species, historians are peculiarly disinclined. They have also tried, with less success, to address themselves to those in government and elsewhere whose cooperation is necessary if historical studies are to make their requisite contribution to social science. Here their suggestions seem to me, in the main, jejune when they are not positively misdirected.

How do the authors see the relation between history and the social sciences? Because they eschew arguments from first principles, and because their implicit arguments are complex and subtle, it is difficult to explain briefly their remarkably successful treatment of this question. They reject the view that history is an aggregate of inquiries having no common core or substance, and insist on the importance of two at first sight rather banal beliefs to which almost all historians would subscribe. These are the belief that no human institution or achievement can be understood except "as seen through time" (p. 17), and the belief—contrasting with the natural scientist's canon of simplicity—that in historical explanation complexity, many-sidedness and many layered-ness are the hallmarks of insight, of truth (p. 18).

These claims provide the starting-point for two highly generalised and highly persuasive descriptions, first of the traditional style, norms and methods of historical research, and secondly of the new techniques of historical analysis that have recently been suggested by, when not actually borrowed from, the more developed social sciences. The combined effect of these brilliant vignettes is to suggest, powerfully, that the hackneyed contrast between humanist and social scientist, or between the idiographic and nomothetic approaches, applies not so much to divergent intellectual aims as to changes in the character of the evidence available to historians and to the discovery of new ways and means of reaching back to the past. Thus the authors re-present the old-fashioned and easily caricatured historian—for whom originality appears to mean simply being the

first to tap new sources, and for whom the minuteness and completeness of his evidence have a near-religious value—as a researcher determined to come as nearly as possible into actual touch with his object, and to build up his total picture only from elements that have been tested by his own individual perceptions and judgments.

Similarly, at the other end of the scale, the historian who makes use of statistical methods, new demographic techniques or the abstract models of economics is shown (when he is not a merely vulgar self-booster) to be quite as good an apostle of complexity, subtlety and many-sidedness as the best of humanistic colleagues. In sum we might say: the authors have effectively reminded us that historians at either end of the methodological spectrum are at one in the pursuit of freshness—of fresh evidence, fresh types of evidence, fresh slants upon and uses of whatever evidence can secure contact between the historical writer and student now and the theme or problem of the past that is his quarry. The importance of this reminder is two-fold. It could teach social scientists that not only one *kind* of history, but that any real work of history, can be of service to them—whether as a trail blazer or provoker or corrector. And it should remind historians (who have always acknowledged their responsibility to use evidence of whatever sort is relevant to their problems) that the new situation in which they must share much of their evidence with scientists engaged in more abstract social studies, gives no cause for anxiety on their part. It simply means a notable extension of the range of their more intelligent and critical readers.

The chapters in which the authors make recommendations for the better direction, development, dissemination and financing of historical knowledge are much less happy in style and level of argument. The authors have not always avoided the temptation to argue that because certain methods of work in the social sciences have succeeded in winning more students, more posts, and more endowments, therefore history had better make at least a show of going with the popular tide. This applies especially to their recommendation of some statistics or some sociology for all undergraduate historians, and of more colloquia, more senior seminars, more travel and, of course, more conferences for their teachers. What is wrong with these proposals is their assumption that whatever is necessary for better work within the discipline can, and must, somehow, be "laid on" to order and for all comers. The truth seems rather to be that, here as in other fields of creative work,

what is most needed is more time, more liberty, more peace for the individual student and teacher to develop his own interests, ideas and techniques *as he matures*. Silence and solitude can do more for continuous learning than can the cult of incessant communication.

W. B. GALLIE

Peterhouse, Cambridge

Humanism and Politics. By Albert William Levi. (Bloomington, Ind.: Indiana University Press, 1969. Pp. 498. \$15.00.)

This book is a response to an urgent need. Professor Levi argues that our civilization is on the edge of an abyss which threatens to swallow up the works of Proust, Picasso, Wallace Stevens, and Stravinsky just as it did those of Menander. Auschwitz and Hiroshima are the warnings, and the author reminds us again and again, lest we forget, of the stakes for which he is playing. Although our situation is such as to have induced the despair and withdrawal from political involvement of Martin Heidegger and Norman Mailer, this is a temptation which must be avoided, for we can be saved, Levi says, only by the intervention of the humanistic imagination.

Science, with its technological barbarism and merely manipulative social branch, can offer us no help, says Levi, and he quickly dismisses it. Only humanism offers a resource because it is the endeavor which can be defined as "the quest for value" (p. 15). It assumes "not only the existence of standards of value which are not merely arbitrary, but the human freedom to commit oneself to values through an act of the will" (p. 17). He does not, however, give us much indication of what the nonarbitrary values are or how they are arrived at. Happily, though, one can catch glimpses of Professor Levi's views in the course of the book—man must always care for man and avoid his reduction to thingness or to brutishness as well as his transcendence of man. He steers carefully between the Scylla of behavioralism and the Charybdis of religion. Some of his imperatives might read: Thou shalt not serve the military-industrial complex (a commandment broken by Herman Kahn). Thou shalt not admire a political assassin who is not willing to die himself (a commandment obeyed by Camus).

The specific task of this book, which Professor Levi seems to understand as an act of humanistic beneficence to politics, is to investigate the relation to politics of humanists from Erasmus to Hochhuth. The fundamental distinction is between those who get involved and those

who do not. For example, Erasmus did not get involved, but Montaigne did; Goethe did not really care, but Schiller did. These essays are filled with much nice detail, summations of the spirits of ages and anecdotes about the lives of the large cast of characters. Professor Levi assures us that he could have begun with Socrates, but that would have been to overextend his "canvas," which is already of proportions that would put Veronese to shame.

These essays, however unconnected they may appear, actually convey a general impression of a subtle humanistic strategy. After the quick tour of Renaissance humanists, we are introduced to the "great refusal," the turning away from politics practiced by many great humanists, particularly German. Levi tells us they were good men because they were humanists, but they, Goethe in particular, did not fulfill their potential for good works to mankind at large. Goethe in some way is responsible for Auschwitz, for he did not engage. Thomas Mann was another who refused engagement but who later changed his mind. And we know that he gave up a rightwing sentimental cultural snobbishness for the politics of the left. As Professor Levi puts it, "... Lukács urged on the regenerate Thomas Mann that at the heart of his intellectual and moral life, he should replace the tradition which runs: Goethe-Schopenhauer-Wagner-Nietzsche with another which runs: Lessing-Goethe-Hölderlin-Büchner-Heine-Marx" (p. 418). The point is that all humanists must be engaged; the refusal leads to Nazism or the like; and when any true humanist does engage, he must do so on the side of the humanitarian left, for that is all that a humanist could do. He must also be a cosmopolitan, for nationalism, it seems, almost inevitably leads to positions like those held by Treitschke, of whom Levi presents a sinister portrait.

The left which we must join in order to be useful humanists is the Marxist left. Marx, we learn, cannot be opposed any more than can Descartes. Even in disagreeing with him, we are agreeing with him. He constitutes our historic ambience. The issue is not whether to accept Marx but how to interpret him. Therefore, it follows that the last half of the book, and more, is devoted to the postures of various humanists with regard to Communism, except for a stop to annihilate Herman Kahn who represents science which is responsible for Hiroshima and the Cold War. Picasso, Brecht, Pasternak, Camus, Sartre, Merleau Ponty, and Lukács are looked at. The existentialists mentioned are all members of Sartre's circle and

their thought treated as a footnote to Marxism.

At all of this, one might wonder whether Stalin's Russia was more open to a humanist's values than was Hitler's Germany. This is a difficult point. Professor Levi knows that Stalin was not nice, but for him the experience with Stalin does not have the same absolute value that the experience with Hitler did. Somehow, humanism is not excluded by it. Thus Goethe's stance toward politics foreshadows Hitler, but Marx's possible connection with Stalin is not even hinted at; hence, humanist activism hardly seems a danger. Herman Kahn is *prime facie* corrupt because he worked for the Rand Corporation, but Berthold Brecht who accepted Stalin Prizes and put the money in Swiss banks was merely exercising peasant canniness in preserving the freedom of the artist; those humanists who supported and praised Stalin receive no blame, or else their little failings are buried under sociological or psychological explanation.

Professor Levi is suggesting a transnational, even a transpolitical, politics, peopled by "humanists" with special privilege but committed to the proletarian cause. Regimes are indifferent; one does not have to study that sort of thing; whichever has more "humanism" is best. This allows a dialogue across the opposing regimes with Marxists from bourgeois societies politely disagreeing with Marxists from Communist societies about the interpretation of Marx. Marxism has been freed from party rigidity, and the success of this endeavor is vouchsafed by the stunning flowering of humanism in Yugoslavia. This is the perfect ideology for the cultural congresses in pleasant Eastern European resorts (p. 342 ff.), and it allows one to play the conscience to both decadent bourgeois societies and intolerant people's democracies which misinterpret Marx's humanism.

The achievement of this book, the political central segment of a monumental trilogy, is to reduce to nothingness the gap separating philosophy from journalism.

ALLAN BLOOM

University of Toronto

The Works of Joseph de Maistre. Selected, translated, and introduced by Jack Lively; foreword by Robert Nisbet. (New York: Schocken Books, 1971. Pp. 303. \$3.95.)

A few years ago, surely, no political thinker would have seemed more irrelevant to the present condition of society—in the eyes of the typical educated American—than Joseph de Maistre. Ultramontanist politics had become anti-

quarian merely; even the royal house of Savoy, which Maistre served with distinction, had gone down to dusty death. Yet Jack Lively (of the University of Sussex) took Maistre very seriously when this useful volume of selections was first published in 1965; and that influential sociologist Robert Nisbet, in his foreword to this new paperback edition, finds Maistre worth comparing with Plato and Hobbes—for good reason.

The powerful intellect of Maistre, at the beginning of the nineteenth century, stood in opposition to the Age of Reason. In the closing decades of the twentieth century, much of the rising generation sets its face against political and moral Rationalism—if more on the principles of Rousseau than on those of Maistre. Once more people seek, as Maistre sought, for an enduring principle of order: some principle quite different from the liberal order of the past two centuries. So it is that the conservative arguments of Maistre regain significance.

"Where the liberals and radicals of their day saw the *new order* rising like a phoenix from the ashes of the old regime," Professor Nisbet writes in his foreword, "the conservatives saw not the new order but the *new disorder*—one that they declared would be the permanent condition of man so long as the values of individualism, secularism, progress, and mass democracy prevailed" (pp. xv–xvi). That principle of order, Maistre (though no theocrat) found in obedience to God's design. The study of history, rather than of philosophy, is the proper discipline of politics.

Mr. Lively selects many of the more important sections and passages from Maistre's *Considerations on France*, *Study on Sovereignty*, *The Pope*, *Essay on the Generative Principle of Political Constitutions*, *The Saint Petersburg Dialogues*, and *Enlightenment on Sacrifices*. The character of these writings is so foreign to the tendency of political belief in America and Britain during the past two centuries that Maistre now has an exotic charm; some readers will be impelled to consult the sources at greater length.

In his perceptive and impartial introduction, Jack Lively traces the opposition—and the similarities—between Maistre and Rousseau, on the one hand, and between Maistre and Hume, on the other. These three political philosophers had this in common, that they denied the authority of a complacent rationalism, and discovered in intuition, moral sentiments, and custom the enduring bonds of society. For understanding in modern context such criticism of the Enlightenment's basic assumptions, it may

be useful to turn to Michael Oakeshott's *Rationalism in Politics*.

Very like Hobbes, Maistre was seeking to apprehend the principles by which men may be saved from anarchy, in a time when the fountains of the great deep are broken up. Very unlike Hobbes, Maistre asserted the primacy of divine revelation and providence. The only real authority, he argued, is God's authority: from that is derived the authority of the political sovereign. For Hobbes's notion of the social compact, and for Rousseau's, Maistre had only contempt: men never deliberately create their own social institutions. Nor does any universal "best form of government" exist, every people necessarily being bound to their own peculiar historical experience. "The question is not to know what is the best form of government, but which nation is best governed according to the principles of its government" (p. 126).

Although influenced by Edmund Burke in much, Maistre did not share Burke's concern for political forms. "At bottom," Lively writes, "he was not concerned with institutional recommendations. His real interest was not in the machinery of government, but in the individual's relations with the state, and his ultimate purpose was to destroy all individual independence by equating the state with God, and transforming a necessarily qualified political into an imperative and unlimited religious obligation" (p. 30). Individualism, indeed, Maistre looked upon as a doctrine concocted in hell: men are not autonomous atoms, but members of a timeless community of souls. He did speak for a freedom that the French Revolution had tried to efface—for the concept that the service of God is perfect freedom.

Pull down your pride: that was Maistre's injunction to the postrevolutionary generation. Overweening egoism, usurping the throne of God, delivers modern man to the devouring appetite for power. As Lively puts it, "What he found most objectionable in this most objectionable of ages was the excessive individualism lying behind its claims for the omniscience and universality of reason" (p. 5). Trusting in his petty private rationality, modern man ignores religious authority and the dictates of common sense—and so loses order in the commonwealth and order in the soul.

Burke was the defender of an order which, in considerable part, could be preserved and improved; Maistre was the spokesman of an order, medieval essentially, that had fallen before he wrote, and could not rise again within foreseeable time. Yet Maistre's mordant analysis of

fallacies in rationalism and liberalism has not lost its force in our time; and this book does something to remind us of how society, like the human body, cannot prosper unless it possesses the power of reaction as well as the power of renewal.

RUSSELL KIRK

Mecosta, Michigan

The Grundrisse. By Karl Marx. Edited and translated by David McLellan. (New York: Harper & Row, 1971. Pp. 156. \$5.95.)

The title of this volume is obviously misleading: its 132 small pages of translated text are not, of course, Marx's *Grundrisse* ("Basic Outlines"). The original work runs to more than 1,000 closely-printed pages; McLellan's volume is nothing else than a selection, covering perhaps one-fifteenth of the mammoth manuscript. Nevertheless, this is a welcome addition to the constantly growing Marxian texts now becoming available in English translation. After Joseph O'Malley's excellent translation of Marx's first theoretical work, *The Critique of Hegel's Philosophy of Right*, David McLellan provides us in this volume with snippets from Marx's most significant manuscript of his mature period.

The Grundrisse der Kritik der politischen Ökonomie is a set of manuscripts prepared by Marx during the late 1850s when he was gathering material for his *magnum opus*. As McLellan reminds us in his Introduction, Marx envisaged this life work of his to comprise six volumes. They should have dealt, respectively, with capital, landed property, wage-labor, state, foreign trade, and the world market. What we possess in the three volumes of Marx's unfinished *Das Kapital* is thus only a torso of what Marx intended to be the first part of this enormous body of work. Marx's methods of work obviously made the completion of such a task unlikely, and one sometimes wonders how Marx did not realize this himself. The economy of time certainly was not Marx's strongest side.

The *Grundrisse* are the drafts Marx prepared for this enormous work. They thus cover a much wider range than that contained in *Das Kapital*: hence it is quite misleading to describe them, as some authors did, as a mere draft of *Das Kapital*. They represent Marx's mature thoughts on a variety of subjects which he hardly had an occasion of discussing in the published writings of his later period. Because the *Grundrisse* were published for the first time in Moscow in 1939, and this German original version is quite rare even today, it is the most

neglected of all of Marx's theoretical writings. Though one may feel that McLellan overstates his case when he suggests that the *Grundrisse* are the "centerpiece" of Marx's work, no adequate understanding of the structure of his thought can be undertaken without incorporating the *Grundrisse* into this account. That some authors continue to overlook this is only a testimony to the basic conservatism of much of the writing concerned with a topic like Marxism in which orthodoxies and counterorthodoxies have become rigidified into doctrinaire beliefs over a period of many decades.

In his helpful Introduction, McLellan traces the development of Marx's studies on economic and social problems from his 1844 *Paris Manuscripts*, through the *Grundrisse* to the published portions of *Das Kapital*. McLellan is most successful in documenting his claim that since 1844 Marx was, in a way, trying to write one book; it never got finished, but the *Grundrisse* is the most complete version of it (pp. 14-15).

The question of continuity in Marx's thought thus comes to the fore in any discussion of the *Grundrisse*—especially in the case of alienation and Marx's usage of the term. McLellan justly points out that authors like Bell, Hook, and Feuer, who maintained that Marx abandoned the concept of alienation in his later writings "must now be judged to have been mistaken" (p. 13). The passages translated on pp. 59-73, 96-102, and 132-143 bear ample evidence to the centrality of the theme of alienation in Marx's mature thought. In the case of Marx's discussion on money (pp. 57-64) the parallels with the 1844 ms., "Money," are most striking and illuminating.

This issue is of extreme importance to the current debate about Althusser's contention that there is a basic "caesura" in Marx's thought and that his early writings should be dismissed as mere "socialist humanism." The neo-Stalinist implications of Althusser's thinking are obvious, and it is for this reason that his views became the new orthodoxy of the French Communist party when it was trying to resist the more humanist interpretation of Marx offered by Garaudy. What is shocking in Althusser's discussion is that he never mentions the *Grundrisse* in his *Pour Marx*. Had he considered the evidence of the *Grundrisse*, the whole argument would have fallen flat on its face—or, alternatively, Althusser would have had to dismiss the mature Marx as a mere "humanist socialist" as well. McLellan refers to this extraordinary intellectual dishonesty of Althus-

ser's (p. 2), and it must invalidate whatever other merits Althusser's treatment of *Das Kapital* may possess.

McLellan is generally a lucid and reliable translator. When one keeps in mind Marx's innate proclivity for long involved sentences, as well as the fact that this is after all a draft manuscript, McLellan's achievement is considerable. On some occasions, however, he is carried away by his zeal to render Marx into clear Anglo-Saxon prose. In the chapter on money, for example, he translates "*blosses Moment des Austausches*" as "pure element of exchange" and "*Bestimmung*" as "definition" (both on p. 59); this is highly oversimplified. On p. 60 he mistranslates "*Verwicklung*" as "implication," and on p. 94 he translates "*Eigenschaften*" as "attributes:" both are obvious mistranslations. More examples could be given of this reluctance to preserve the Hegelian flavor of Marx's language: it flattens much of Marx's argument and is hardly justifiable.

Nonetheless, the availability of this selection from the *Grundrisse*, together with an earlier selection of other parts of it edited a few years ago by Hobsbawm under the title *Pre-Capitalist Economic Formations*, makes the prospect of the forthcoming complete translation of this work by Marx into English even more welcome and eagerly awaited.

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Ideology and Moral Philosophy. By K. Bruce Miller. (New York: Humanities Press, 1971. Pp. 200. \$7.50.)

How is one to react to a book which begins by lamenting the incapacity of "intellectualism, scientism, and particularly the radical moral subjectivism of positivism" to "raise a barricade of sustained moral action against the ubiquitous forces of disintegration appearing in both totalitarian ideologies and alien ethical systems" (p. xiii)? At the very least in a polemical work such as this, one would expect an effort to describe the "moral ideology," or "vital relationship of knowing, believing, and doing," which the author considers superior to the "narrow approach of Western intellectualism" (p. 137). A chapter is devoted to the ethics of Marxism-Leninism and it is preceded by a series of brief evaluations of various figures in the history of philosophy; Hume, Kant, and Bergson are praised because they recognize the volitional elements in moral philosophy, while the Greeks and the Scholastics are guilty of "anti-practical intellectualism." No attempt,

however, is made to describe the substance of the moral ideology favored by the author. Instead he presents us with a tract on the need for a "dynamic" philosophy in the world today. But as Bentham once said, "Hunger is not bread" and a belief in belief is not an ideology. What Miller calls "experiential supernaturalism" (p. xvi) seems to involve faith in the person of Christ (p. 103) or in the efficacy of love, but then why was this book published in a series on Philosophy? (Or is the Humanities Press's attitude toward the whole enterprise indicated by the title "Ideology and Moral Philosophy" printed on the back of the dust jacket?)

Appendix 60 at the end of the book finally gives the whole show away. It begins "To date this superior ideology has appeared in the force of Moral Re-Armament" and this is followed by the favorable comments by Konrad Adenauer and denunciations by Radio Moscow which appear often in MRA literature. The author has limited himself to only one of the four MRA absolutes, absolute love, but like the moral rearmer he makes no effort to deal with the complexities, tensions, and ambiguities involved in applying this principle to personal, political, and international life.

Reportedly MRA is running into financial difficulties these days. And if its advocates do not buy this book, who will? Perhaps the publishers should try to boost sales by including with each copy a free record of a youth choir singing "Up with People." *Ideology and Moral Philosophy* is neither ideology nor moral philosophy.

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L'Obligation Politique. By Raymond Polin. (Paris: Presses Universitaires de France, 1971. Pp. 159.)

This book is first and foremost a harsh, unrelenting polemic against "*les anarchistes sorbonniques*" of 1968. The character of the polemic and the concern that fuels it are conveyed by the following passage: "*on pense à la dissolution de l'Empire Romain sous les coups des grandes invasions: il s'agissait aussi d'une civilisation qui n'avait pas pu et pas su se défendre contre les barbares. Mais, cette fois-ci, les barbares sont à l'intérieur*" (p. 62). By demonstrating the simplistic and dangerous character of the new version of anarchism that took form "*dans l'enceinte limitée et artificielle de la Sorbonne*," and by reasserting the view that acceptance of political obligation is an

essential constituent of a human community, Polin evidently hopes to provide France and perhaps contemporary civilization more generally with the understandings and the fortitude essential to defense against the new barbarians.

In its polemical character the book is of interest primarily as one more testament to *l'emprise* that *les événements* of 1968 continue to exercise over politics and much political thinking in France. (Curiously, the work contains lengthy sections consisting of what are on the whole familiar summary sketches of earlier theories concerning obligation and rights.) Owing perhaps to the author's basically polemical purposes, the more broadly theoretical position taken is presented primarily by means of dogmatic assertion rather than by carefully elaborated argumentation. Such a position does emerge, however, and deserves attention.

Professor Polin is one of the very best contemporary interpreters of the thought of Hobbes, and his own position testifies to Hobbes's influence upon him. Most generally, political society is said to be founded on an "opposition dialectique" between an absolute and unconditional obligation to obey and an absolute right or liberty of individual judgment and action. (Of course Hobbes did not speak of dialectical relationships. But there is nothing in Polin's use of this fashionable terminology that goes beyond or puts him at odds with what Hobbes said in other language.) By absolute obligation, moreover, Polin intends that the citizen, once having consented (p. 67), must obey all commands issued by the sovereign political authority, that is by an agent or agency that possesses a right of life and death over the members of society and hence the right to risk life and death in order to assure the public good (pp. 65-6). If it should be thought that having such an obligation negates or destroys liberty, Polin reminds us that even the slave "*peut toujours prendre le risque de refuser de l'être*" (p. 72). As with Hobbes, at bottom liberty consists of the capacity (Polin uses "*pouvoir*," "*droit*" and "*liberté*" indiscriminately in discussing the point) to go out—whether with a bang or a whimper. The one "right" of disobedience that is allowed the citizen, accordingly, is the right to defend his or her life. "*En effet, chaque fois qu'un individu se trouve menacé dans sa vie par l'application de la loi, d'une loi pénale par exemple, il conserve un pouvoir absolu de la défendre par tous les moyens. En menaçant sa vie, la loi lui ouvre un droit à la désobéissance*" (pp. 73-4). Finally,

Polin echoes Hobbes in contending that when revolutions break out, all questions of right, of justification cease to have point. "*Une prise de position révolutionnaire ne se discute et ne se décide pas en termes de droit. . . . Une révolution est un fait entre deux droits; le droit qu'elle dénie et le droit qu'elle veut instituer*" (p. 97).

But Polin is no mere Hobbist. He has, to begin with, a conception of the end or vocation of man that is far more inclusive than the satisfaction of individual desires that Hobbes emphasized. This larger vocation, secondly, can be pursued only in a political community in which there is authority that can be used to achieve the public good. Thus it is not only legitimate in the narrow sense but fully appropriate and desirable for authorities to command the members of the political community in ways designed not simply to satisfy the desires the latter actually have but to contribute to achievement of the distinctively human *telos* or vocation. Such a community, thirdly, requires much stronger foundations than the narrowly instrumental and strictly individualistic allegiance that Hobbes proposed. It must "*incarner des valeurs décisives, des valeurs sine qua non. Si ces valeurs cessaient d'être tenues pour des valeurs suprêmes, sacrées, si la communauté cessait d'être la patrie, pour reprendre un terme que l'on a tendance à oublier dans la vie ordinaire, mais auquel chaque crise grave restitue instantanément une vertu poignante, alors l'obligation politique perdrait elle aussi son sens*" (p. 65).

Topical polemics aside, the basic defect in Polin's construct lies with the conception of liberty he accepts from Hobbes. The additions he makes to Hobbes's theory are indeed essential to an adequate conception of political obligation. But these additions require a conception of political liberty that will rarely if ever be satisfied by the desperate self- and community-destroying action that Polin treats as the essence of human freedom.

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Anarchy and Order; Essays in Politics. By Herbert Read. (Boston: Beacon Press, 1971. Pp. xxii, 235. \$7.50.)

Anarchy and Order, originally published in England in 1954, is a collection of sociopolitical essays written by Herbert Read, who died in 1968. They form only a small part of the corpus of his work, however, and the philosophy which underlies them is the same as that which

informed his more substantial and influential works on the relationship between art and society (*Education Through Art, Art and Society, To Hell with Culture*, etc.).

Read had little original to say about anarchism, but this collection is essential reading for political scientists interested in the peculiarly undoctinaire tradition of English anarchist thought, in the distinctive aesthetic approach Read brought to political and social criticism, and in the current widespread attraction for anarchist theories in the overdeveloped industrial western world.

Serving as an Infantry officer in France in 1917, Read described the aim for which all socialists should strive as "more decency and beauty of life" (see the entry for 10 October, 1917 in his War Diary, published in his autobiographical reminiscences, *The Contrary Experience*, [London: Faber and Faber, 1963, p. 112]). This was more than simply a reaction to his immediate circumstances, however, for Read, like so many anarchists, was also lamenting the passing of rural society and its values. He may have rejected the backward-looking romanticism of William Morris, as he claimed, but in *Poetry and Anarchism*, the major essay in the volume under review, his acceptance of industrialism was at best reluctant, and he wished only to see it used to restore values, through anarchism, which were essentially those of a rural society. "I despise this foul epoch—not only the plutocracy which it has raised to power, but also the industrial proletariat which it has drained from the land . . . Deep down, my attitude is a protest against the fate which has made me a poet in an industrial age" (pp. 58–9). He was not only a ruralist at heart, but he was a very English ruralist, too. In *A Dearth of Wild Flowers*, an essay in *The Contrary Experience*, he recalled nostalgically the days of his boyhood on a Yorkshire farm, associated "the genius of England" with the country house, particularly the parsonage, and concluded fatalistically that despite their knowledge of the solidity of rural values, men were walking "like blind animals into a darker age than history has ever known" (p. 343).

But Read was not merely a cultural pessimist in the atomic and industrial age. As his essays on this volume make plain, he possessed an acute artistic sensitivity and awareness, for which none of the major ideologies offered a haven, and for which the history of his time provided a turbulent background. Having as a young man avidly devoured Proudhon, Tolstoy, Kropotkin, and Morris—and, significantly

enough, having passingly embraced Disraeli's version of a Tory Democracy—he suffered the disillusionment with the “peace” of 1919, discovering that, in his own words from *Poetry and Anarchism*, “we had won trophies of hatred and greed, of national passion and commercial profiteering, of political reaction and social retrenchment” (p. 75). He withdrew into personal and professional life, first as a civil servant, then later in 1931, as Professor of Fine Arts at Edinburgh. Ex-bureaucrats often make good anarchists, but Read's explicit commitment to anarchism and his political essays (which in this volume date from 1938 to 1953), arose in large part from a rejection of the deadening effects on the artistic imagination of both western and Soviet societies in the 'thirties. For Read, Mayakovsky's suicide in 1930 was symbolic of the failure of communism to create the framework for “decency and beauty of life,” and again for him, as for many in his generation, the Spanish Civil War acted as a catalyst for the open espousal of anarchism. In the religious fervor of the Spanish anarchists he saw the possibility of a new religion emerging as a basis of a new society.

Read's inconsistencies have been frequently noted. A theoretical pacifist in 1914, he nonetheless fought; having proclaimed in *Poetry and Anarchism* that anarchism naturally implied pacifism, he supported the British war effort, and, like Kropotkin before him for the same reasons (in 1914), created a distance between himself and other anarchists; in the 1940s he was closely associated with *Freedom*, the anarchist newspaper founded by Kropotkin in 1886, and with the Freedom Press, which published two of the essays appearing in this collection—*The Philosophy of Anarchism* and *Marxism, Existentialism, and Anarchism*, yet he was never an activist. His quixotic acceptance of a knighthood in 1953 led to a further estrangement, and his energies in the last two decades of his life were almost exclusively directed toward literary and artistic criticism.

Yet the inconsistencies and apparent lack of activism were an integral part of his social philosophy. For Read, artistic awareness and creativity were the solvents of false values, and experience the crucible of that creativity. He defended his war experience for the self-awareness it gave him, and, a romantic, rejected any attempts to restrict and define modes of political commitment. Significantly and characteristically, his main criticism of fascism and communism in *The Philosophy of Anarchism* was “for their lack of any real sensuous and aes-

thetic content, for the poverty of their ritual, and above all for a misunderstanding of the function of poetry and imagination in the life of the community” (p. 45).

This provided Read's anarchism with its individuality. If, as he believed, the worth of a civilization could only be measured by the quality and achievement of its “representative individuals”—its philosophers, its poets, and its artists, and the artist was the essential mediator between the individual and society, then his own substantial contribution to artistic and literary criticism was explained and justified. His work on the relationship between art and society was his form of activism, and his influence on such contemporaries as Gropius and Henry Moore can be judged both from the *Memorial Symposium* in his honor (London, 1970), and from his personal papers deposited in the University of Victoria.

Summing up his fundamental beliefs at the end of his life, Read observed that his profoundest experiences had been aesthetic. “Goodness is living beauty,” he wrote, “life ordered on the same principles of rhythm and harmony that are implicit in a work of art. Vulgarity is the only sin, in life as in art” (*The Contrary Experience*, p. 348). This collection of essays tells us why anarchism appealed to this essentially unpolitical man.

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Freud: Political and Social Thought. By Paul Roazen. (New York: Alfred A. Knopf, 1968. Pp. x, 322. \$6.95. [Paperback edition, Random House, Vintage Books, 1970. \$1.95].)

This is a welcome book and one which seemed long overdue. Despite occasional allegations that political scientists are wedded to a psychoanalytic view of man, the impact of Freud on the discipline—and related social sciences—has been oblique and elusive. The study comes at a time of intense professional ferment, a period marked by uneasiness with prevailing standards of inquiry and canons of explanation. The argument is presented in a lively literary style which contrasts sharply with the turgid jargon so prevalent in contemporary professional publications. Yet, the book is far from flawless; in view of its timeliness and intrinsic qualities its defects are all the more regrettable.

The “Introduction” properly sets the mood. As Roazen notes, verbal invocations notwithstanding, Freud has not become “part of the conventional wisdom of academic political

thinking" and actually has "remained throughout political science something of a spook" (p. 5). Among the many reasons for this state of affairs, Roazen points to the vicissitudes of the psychoanalytic "movement" itself, to the growing divorce of clinical investigation from the legacy of Freud's social thought—a divorce which resulted, on the one hand, in a narrow professionalism and, on the other, in a proclivity for "extravagant" and "overblown" social speculations (p. 7). A basic prerequisite for gaining access to Freudian thought, according to the author, is the clarification of methodological issues: by disregarding these issues "social scientists have missed the full potential significance of psychoanalysis for the study of politics" (p. 35).

Given the crucial status Roazen assigns to methodology, the following chapter dealing with "psychoanalysis and the study of politics" is bound to be disappointing. By contrast to anthropology, we learn, political science has remained largely immune from influence, chiefly due to a neglect of Freudian techniques and especially of the methodological implications of his discovery of the "unconscious." Attention to these aspects, the author affirms, would contribute to overcoming "the limitations of political behavioralism" by bringing into view submerged or cryptic dimensions of political experience. Unfortunately, Roazen's assertions on this point are themselves cryptic; in fact, the significance of psychoanalytic method for political, and specifically behavioral, inquiry remains largely obscure. "Behavioral reality can hide latent meanings," he notes adding that "precisely because of its preoccupation with motivation, because it is interested in psychic reality as well as external behavior, psychoanalysis can help to show the inadequacies of behavioralism" (pp. 54, 65). The admonition to probe motivation and the psychic dimension is good counsel, but one which would hardly be disputed by behaviorists. Clearly, political behavioralism has traveled a long way from Watson's crude reinforcement model; from the days of Merriam (not to mention Lasswell) to Lane's recent studies, psychic factors have been treated as important "intervening variables" in empirical political inquiry. To this extent, Roazen's admonition appears less a challenge than corroboration of empirical behavioral research.

Yet, the situation is more complex; in several passages the author suggests that psychoanalysis points beyond empirical or scientific methodology. "The approach of depth psychology,"

he writes (p. 64), "is designed to focus on the 'way in which human beings in a specific situation subjectively experience themselves.'" The citation refers to Peter Berger and thus, at least obliquely, to the legacy of understanding or *Verstehen* theory in social science. Unfortunately, the reference stops there and is not further pursued. There is no hint of recent hermeneutical reassessments of Freud—the most notable example being Paul Ricoeur's *Freud and Philosophy: An Essay on Interpretation* (whose French edition, after all, appeared in 1965). Silence also covers recent examinations of psychoanalysis from the perspective of structural linguistics (e.g., Jacques Lacan, *The Language of the Self*, translated in 1968). Indications are that Roazen confounds interpretation and the actor's perspective with subjectivism or introspection. "There is a widespread reluctance," he says (p. 62), "to acknowledge the subjectivist character of psychological knowledge, and a corresponding tendency to attempt to render psychological insights prematurely scientific by social survey techniques. That psychological understanding is first of all a question of the sensitivity of the finger tips has yet to be conceded."

In subsequent portions of the book Roazen turns to the central aim of his inquiry: the elucidation of Freud's political and social thought. Initial glimpses into this dimension are provided through an examination of Freud's writings on the psychology of religion, including *Totem and Taboo* and *The Future of an Illusion*. The next section, entitled "Politics: Social Controls," opens with a discussion of Freud's two essays on war, written in 1915 and 1932. Several conclusions, in Roazen's view, can be drawn from these writings. With regard to child development, he notes, Freud's essays point toward "a balanced psychic diet of indulgence and deprivation," since "lack of discipline, as well as an absence of instinctual gratification, can interfere with the growth of ego functions" (pp. 205–206). In the broader context of political theory, "Burkean conservatism" is singled out as a remedy against aggressive drives, while Fromm appears as representative of "anarchistic" tendencies resulting from a revisionist neglect of aggression (p. 210). What one misses at this point, above all, is a consideration and analysis of Lorenz's views on aggression. Turning to *Group Psychology and the Analysis of the Ego*, Roazen affirms that instinctive group behavior, like war, appeared to Freud as a state of regression, a relaxation of civilized constraints in favor of libidinal attachments be-

tween group and leader. As he adds, Freud's study constituted an important landmark in the development of "ego psychology" with its stress on the integrative and regulative capacities of the ego and its differentiation of ego and "ego-ideal" (superego). His interpretation definitely seems an advance over Rieff's treatment of *Group Psychology* as an outgrowth of "conservative snobberies"; Roazen's own political inferences, however, are at best puzzling. Contrary to the previous appeal to Burke, Freud's perspective on social psychology is now described as consequence of his attachment to a "traditional liberal view;" curiously, his liberalism is associated not so much with the accent on regression in group life as with the juxtaposition of state and individual and a concomitant fascination with the model of "the leader and the led" (p. 231).

The section on social controls is rounded out by a summary overview of Freud's "political psychology." As Roazen indicates, Freud never had a direct personal "stake in politics" (p. 242); his one adventure in the broad arena of contemporary political affairs—the Freud-Bullitt study of President Wilson—is commonly viewed as an embarrassing fiasco. (In the "Epilogue" of the book, Roazen probes the background and suggests reasons for the fiasco.) Yet, lack of practical involvement does not necessarily affect the political import of Freud's teachings. Roazen successfully sketches the ambivalence and complexity of Freud's political perspective—but without clarifying the connection between the diverse strands of his thought. As before, Freud is depicted as part of "the liberal tradition" and "a great heir of the Enlightenment." In probing the unconscious and man's elaborate psychic structure, however, Freud also presumably contributed to "liberalism's self-examination." This self-examination, according to Roazen, opened up vistas pointing both in conservative and radical directions: "Along with Burke he recognized the intensity of destructive urges and the sense in which societal coercions can be psychologically necessary. With Marx he extended our appreciation of the extent of self-deception and self-alienation" (pp. 247–249). The study closes with a chapter on "Civilization: Tragedy and Possibility," devoted mainly (though not exclusively) to an analysis of *Civilization and Its Discontents*. Again Roazen emphasizes the inner tension in Freud's thought: "This polarity between the need for individual fulfillment and the necessity of social coercion was a central theme throughout Freud's social thinking. His

ability to retain both poles within his framework separated him from romantic anarchism, just as his rationalism and scientific commitments saved him from conservatism" (p. 255).

The book leaves the reader both intrigued and perplexed: intrigued by its verve, its accomplished style, and the many nuggets of insight strewn through its pages; but also unsure about the drift of its arguments. As a challenge to behavioralism the book is hardly successful or persuasive. Both in regard to methodological and substantive issues the study contains too many equivocations to permit a crystallization and clarification of alternative positions. In trying to accommodate too many perspectives, Roazen risks offending each; more probably, however, his study will be absorbed into "mainstream" political science with its motley amalgamation of views. The reader who uses the book as introduction—and as such it has many virtues—would want to supplement it with competing texts. Compared with another treatment of the same topic, Johnston's *Freud and Political Thought*, Roazen's presentation is more subtle but also more narrow; whether adequately or not, Johnston at least reviews the work of Lasswell (*Psychopathology and Politics*), Money-Kyrle (*Psychoanalysis and Politics*), Osborn (*Freud and Marx: A Dialectical Study*), and Schneider (*Freudian Psychology and Veblen's Social Theory*). The accent on ego psychology needs to be balanced with literature written from a different vantage point, including Fromm's *Crisis*, Laing's *Divided Self*, and Marcuse's *Eros and Civilization*. Moreover, the reader should note an important recent contribution to the general theme: Wollheim's *Sigmund Freud*. What all these studies at least indicate is the immense fertility and richness of the Freudian legacy, a legacy which has only barely begun to infiltrate political science. Despite Roazen's valuable efforts, Freudian psychoanalysis still remains for social theory and research largely *terra ignota*, beckoning to be explored.

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More Essays in Legal Philosophy. Edited by Robert S. Summers. (University of California Press, 1971. Pp. vii, 161. \$6.50.)

Since there is probably no effective line to be drawn between political theory and legal philosophy, the subject of this book (like its predecessor edited by Professor Summers, *Essays in Legal Philosophy* [1968]), deserves attention from the readers of this journal. Surely—so an

argument might go—if a political theorist cannot afford to ignore the views of Suarez, Hooker, Hobbes, and Blackstone, he cannot afford to be ignorant of the views of their successors such as Austin, Maine, Holmes, Pound, Hägerström, and of such contemporaries as H. L. A. Hart, Lon Fuller, and Hans Kelsen. And if one expects to understand the views of these legal philosophers, there should be a scholarly need (cynics might prefer “market”) for critical essays like those gathered in the present book.

What Summers has given us (and he implies he would have given us more and better if only the pickings were not so slim) is a book of six essays, five of which provide, he says, “a ‘general assessment’ of much of the relevant work of a major legal philosopher” (p. vii). Hart, himself the subject of a critical study in this volume by Rolf Sartorius, contributes the lead essay on Bentham. Herbert Morris writes on Pound, Martin Golding on Kelsen, and the editor on Fuller. All the essays were previously published during the 1960s and most first appeared in scholarly journals not likely to be available in general college or university libraries. The sixth essay, “Notes on Criticism in Legal Philosophy,” is the editor’s introduction. No other single book provides such superior critical appraisals of this (or, for that matter, of any other) group of legal philosophers, and in this respect and as a contribution to familiarizing a wider scholarly public with the methods and accomplishments of philosophical criticism in law, the book is quite valuable.

The five main essays show different preoccupations, and provide (in the language of the book’s subtitle) “General Assessments of Legal Philosophies” in divergent and not equally satisfactory fashion. Hart’s essay originated as a Lecture on a Master Mind to the British Academy. For all its distinguished qualities, at most the ten pages devoted to expounding and criticizing Bentham’s conception of legal and moral rights can properly be said to be directly relevant to the putative concerns of this book. Morris’s patient and effective criticism of Pound is in fact only a long book review of Pound’s multivolume *Jurisprudence* (1959). Morris, no doubt unintentionally, persuaded this reviewer that by any criterion, Pound’s legal philosophy—at least as presented in *Jurisprudence*—is too primitive and inchoate to be deserving of sustained and intensive study. Golding confines his attention to a close examination of Kelsen’s notion of a legal system, and in the course of doing so provides not only

acute criticisms of the topic under review but also a number of considerations of general interest (in particular, a study of the notion of a “rational reconstruction” of the concept of a legal system) which are relevant to the appraisal of any legal philosophy. Sartorius writes on the seminal ideas in Hart’s influential book, *The Concept of Law* (1961), especially on Hart’s much-discussed claim that the distinction between so-called “primary and secondary” (duty-imposing and power-conferring) rules of law provide “‘the key to the science of jurisprudence’” (p. 136). The editor’s essay on Fuller is another long book review, in this case of *The Morality of Law* (1964), and thus rather less than a “general assessment” of Fuller’s legal philosophy.

Uneven as the book is in providing such “general assessments,” the essays reprinted here do display a wide variety of critical moves which anyone must be able to deploy if he hopes to do effective work in understanding a legal philosophy. At times it even looks as if these essays were gathered together as much for the conceptual ploys, gambits, and assorted critical maneuvers they exhibit as for their “general assessments.” Certainly, the editor’s own preoccupations in his introductory essay, in which he catalogues and illustrates some tools and techniques of varying sophistication to be found in the analytic philosopher’s kit-bag, will not discourage this interpretation of his book.

Because the editor’s own writing dominates the volume (it amounts to exactly one-third of the whole), it may be appropriate to measure some of it with the same severity he and his co-authors visit upon others in the book. Consider as a sample this short paragraph from Summers’s discussion (elsewhere often instructive) of Fuller’s central doctrine, that the law is a “purposive human enterprise.”

A theory of law should also provide an account of legal sovereignty. But the author’s theory does not. Furthermore, his theory of law as a purposive phenomenon cannot begin to account for the complex factors that influence compliance with the system. According to one whole tradition in legal thought, this compliance is to be represented in terms of “habit,” a conception wholly at odds with “purpose.” The habit theorists may have been wrong, but not wholly wrong (p. 119).

As the essay containing this paragraph was originally published in a law review, the absence of any explanation of “legal sovereignty” is perhaps excusable. However, the editor in his introductory essay to this book comments in

passing that "the so-called problem of 'legal sovereignty' really breaks down into a half dozen separate questions" (p. 2). In neither context is the reader told what any of these "questions" is. Consequently, he is at a loss as to the meaning and significance of the alleged failure of Fuller's theory. But does Fuller's theory really fail as alleged? I am not certain that it does; since Summers gives no analysis or argument for his opinion, the reader of his essay, unless he has carefully studied Fuller's book with this very question in mind, is in no position to judge. Summers's next claim is carelessly overstated. Fuller's doctrine of purpose *does* "begin to account for the complex factors that influence compliance with the system." It *does* go some distance toward that end, even if it fails to give a completely adequate account. Whether or not Fuller implied that it did seems not to concern Summers. Finally, Summers's reference to the "habit" theory comes perilously close to being a red herring. Elsewhere, in his own review of Hart's *Concept of Law* (cited at p. 103, note 3), Summers explained without reservations Hart's now well-known objections to the "habit" theory. Unless Summers has changed his mind about the finality of Hart's criticism, why make it appear to be such a glaring fault of Fuller's "purposive" theory that it and the "habit" theory are "wholly at odds" with each other? In any case, the two theories are *not* "wholly at odds"—they are independent but not necessarily contradictory. Nothing in Fuller's "purposive" theory prevents him from invoking habits of obedience where they exist and are relevant to explaining compliance with law.

The book, like too many nonbooks, has no bibliography or index. Several of the footnotes betray someone's negligence, for the interior cross-references have not been brought into line with the book's pagination. No other errata of moment were noticed.

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Between Two Worlds: The Political Thought of Graham Wallas. By Martin J. Wiener. (London: Oxford University Press, 1971. Pp. 229. \$11.25.)

Dr. Martin J. Wiener deserves our gratitude for having given us the first book-length study ever made of one of the most important and yet curiously neglected political thinkers of our century in the English-speaking world. Further, he is to be congratulated for having done his job well, for having written an intellectual biog-

raphy, however limited its scope, which pulls together an impressive body of basic data, written in a style which is at once scholarly and readable.

The author begins his study with a discussion of Wallas's "Victorian origins," making special note of his childhood as a son of a clergyman, tracing his intellectual growth through his education at Shrewsbury and Oxford, studying classics. Some careful attention is given to Wallas's activities between 1886 and 1904 in the Fabian Society, in which he played a prominent role, together with the Webbs and Shaw. It was during his tenure as a Fabian Socialist that Wallas was elected, in 1894, to the famed London School Board; in 1904 he was elected to the London County Council, as the School Board was merged into the Council, serving that body until 1907. His first book, *The Life of Francis Place*, was published in 1897, when he was thirty-nine, and it was acclaimed, in and out of the Fabian Society, as a pioneering work in social historiography.

As a political thinker Wallas was a late bloomer: he did not produce his first major work until his fiftieth year, four years after his resignation from the Fabian Society and a year after his retirement from politics. When *Human Nature in Politics* made its appearance in 1908 it stirred a sensation as a work of great courage and originality. In it Wallas attacked what he called "intellectualist fallacies" inherent in the prevailing misconception of political behavior, a misconception which he, rightly or wrongly, blamed largely on the lingering impact of Benthamite psychology. It has been said repeatedly, probably without exaggeration, that of all the works written during this period which sought to undermine "intellectualism," or false rationalism, *Human Nature in Politics* stands out as the most significant single achievement in shifting the focus of British and American political science away from the sterile institutional analysis of "government" to a psychological examination of political "behavior." With his later works, notably *The Great Society* (1914) and *Our Social Heritage* (1921), Wallas's influence spilled over into other fields of social inquiry, provoking a chain of serious debates among their leading students.

It cannot be determined from Wiener's book whether the title is a conscious adaptation from that famous line in Matthew Arnold's *Grande Chartreuse* (stanza 15), "Wandering between two worlds, one dead,/ The other powerless to be born." But the phrase as it stands certainly evokes a powerful image, and that serves the

author's purpose well. He locates Wallas's intellectual career in the transitional period between the Victorian world of "evangelical" conscience and the modern world of scientific spirit; but he feels that the Victorian ethos, against which Wallas rebelled in his youth, as did many of his contemporaries, was not yet "dead," nor, indeed, was the new world still unborn awaiting its turn. These two worlds existed side by side, in awkward company, creating a tension in those who sought, without success, to break completely with their cultural inheritance in the hope that they might become full-fledged citizens of the new world. These people were suffering from a certain "identity crisis," if you will, or an attitudinal ambivalence, which marked the "transitional character" of the so-called "Edwardian" period, and the author describes Wallas as its "representative" figure (p. 1). Thus, he was at once "a behavioural scientist and a moralist, a 'debunker' and an idealist, a practical reformer and a misty visionary" (p. 216). Insofar as Wallas was a political scientist, says the author, he was "Moses rather than Joshua: he showed the way out of the desert, but did not himself enter the land of modern political science" (p. 96).

Regrettably, the author nowhere tells us precisely what in his mind constitutes the defining characteristic of "modern political science," for his interest is primarily that of an intellectual historian, rather than that of a political scientist. But my reservation, although limited, goes further, because I believe that the book contains some errors of judgment which appear to have sprung from a careless reading of Wallas's own writings rather than from a possible lack of expertise in the literature of political science.

Dr. Wiener is correct to note the essentially individualistic content of Wallas's psychological interest, and yet, oddly enough, he fails to take notice of the enormous importance Wallas attached, throughout his life, to the method of "introspection." Second, the author claims that Wallas in his later years "altered his methodology" (p. 208) in expressing his doubts about the usefulness of "quantitative method," which he had favored in *Human Nature in Politics*. A careful reading of that book would reveal, however, that what really changed was not so much Wallas's methodological bias as the manner in which he used the phrase "quantitative method." His methodological preference was twofold, i.e., introspection and description, and in this preference he remained remarkably consistent.

One who is interested in intellectual history,

seeking some broad historical perspective to Wallas's life and thought, will find Dr. Wiener's effort highly rewarding. But a political scientist interested in gaining a fresh appreciation of Wallas's contributions to his discipline may be justified in wishing for a much more analytical treatment of the subject, one which would go beyond historical generalities and devote itself more fully to the critical analysis of the substance of the man's thought, especially with regard to his overriding perennial concern with the *method* of political inquiry.

SUGWON KANG

New York University

Congress Against the Court. By Adam Carlyle Breckenridge. (Lincoln: University of Nebraska Press, 1970. Pp. viii, 160. \$5.95.)

The subject of this little book is more specific than its title implies. Unlike *Congress and the Court* (by Walter F. Murphy) and *The Supreme Court and Congress* (by John R. Schmidhauser and Larry L. Berg), it is no general treatise on periods of conflict and accord between Congress and the Supreme Court. Instead, Breckenridge has put together a description of three Court decisions—*Mallory v. United States* (1957), *Miranda v. Arizona* (1966), and *United States v. Wade* (1967)—which he argues led in part to the Omnibus Crime Control and Safe Streets Act of 1968. "Unlike the Roosevelt court-packing plan, the Congress sought to overrule the objectionable decisions through legislation" (p. viii). Title II of the act did alter some of the stipulations and guarantees from *Mallory*, *Miranda*, and *Wade*, for persons accused of crime.

For the political scientist, this book is a disappointment. Professor Breckenridge offers no thesis about relationships between Congress and the Court, even with regard to the policy issue he studies. There is no reference to extensive research by Murphy, Schmidhauser and Berg, Robert Dahl, Harry Stumpf, Glendon Schubert, and Stuart S. Nagel, all of whom have created models of Congress-Court relations to which more limited hypotheses might be tied. These models have used the policy outputs of Congress and the Court as a basis for determining periods of conflict and accord. The Warren Court, 1953–1969, offers a fecund period for hypothesis-building about branch relations and policy output. Breckenridge creates no hypothesis; and he fails utterly to relate his subject to earlier research. There is, in fact, no indication that the author is familiar with existing literature on Congress-Court relations.

The Court's treatment of the rights of accused persons, as well as of civil liberties generally, over a sixteen-year period offers an opportunity to relate Court activism and restraint to the policy choices of other national political institutions. As many analysts have shown, the Warren Court was not uniformly activist. Although Breckenridge mentions the seeming aberrations of two subversive activities cases—*Uphaus v. Wyman* and *Barenblatt v. United States* (both 1959)—he does not deal with differences in Warren Court periods. The analysis of such periods can be significant, for, as Nagel has shown, frequency of Court-curbing legislation correlates nicely with Court composition and output. That Breckenridge fails to treat the court-curbing aspects of crime control legislation more systematically is a major defect of this book.

In the preface, Breckenridge states that "racial issues, unrest in urban areas, large and small, reports of a substantial rise in crime, a growing concern about many social and economic conditions in the nation, all helped climax a near-constitutional crisis" (p. viii). Such a statement serves as a backdrop for the author's description of *Mallory*, *Miranda*, *Wade*, and House and Senate versions of the Crime Control Act. Whether these conditions did precipitate a near-constitutional crisis is difficult to say. The author never examines the proposition in the text; and he offers no guidelines or criteria to define a "near-constitutional crisis."

Without appeal to systematic analysis, we know that many congressmen and senators reacted negatively to the Warren Court's elaboration of the rights of accused persons. Breckenridge feels that "Minority Leader Gerald R. Ford (Rep.) of Michigan . . . echoed the majority view of the House, saying, 'I refuse, Mr. Speaker, to concede that the elected representatives of the American people cannot be the winner in a confrontation with the Supreme Court'" (p. 6). Senator John L. McClellan (Dem.) of Arkansas termed *Mallory*, *Miranda*, and *Escobedo v. Illinois* (1964) "deplorable and demoralizing. They have weakened intolerably the force and effect of our criminal laws. . . . These decisions have set free many dangerous criminals and are daily preventing the conviction of others who are guilty" (p. 38). Breckenridge describes with balance the general "atmosphere" or "mood" in the House and Senate at the time its members considered crime control legislation. His explanation of the differences in attitudes represented by Emanuel Celler (Dem.) of New York and McClellan,

chairmen of the House and Senate Judiciary Committees, respectively, and the way in which such attitudes affected the bills' treatment, is a notable aspect of this study. In the end, the House accepted the more restrictive Senate version of the crime control act; the separate House and Senate measures never went to conference committee. Neither version affected the appellate jurisdiction of the Supreme Court. President Johnson signed the measure into law on June 19, 1968.

The student of constitutional law and the administration of criminal justice will likewise find this book disappointing. The mode of analysis is principally descriptive: viz., the facts are these, the outcome is this. Critical evaluation of constitutional decisions, characteristic of the scholarly work of Alpheus T. Mason, Alexander Bickel, and Martin Shapiro, is absent or undeveloped. Similarly, the sociological and administrative aspects of such decisions are undertreated. For example, Theodore L. Becker's *The Impact of Supreme Court Decisions* (New York: Oxford University Press, 1969) contains two studies on the impact of *Miranda*. Such analyses are useful in assessing the significance and effect of Court policy; but Breckenridge shows little interest in the impact of the decisions he describes. There is no attempt to deal with "law and order" as a social problem. If anything, these sociological and administrative aspects receive indirect treatment through the author's account of expert testimony before the House and Senate committees.

One-third of the book is an appendix in which the *Mallory*, *Miranda*, and *Wade* opinions are printed. For a book of this size, this seems unnecessary. An index and bibliography are not included; they are necessary.

ROBERT G. SEDDIG

Allegheny College

Racial Discrimination and Public Policy in the United States. By Richard M. Burkey. (Lexington, Mass.: D. C. Heath & Co., 1971. Pp. 144. \$10.00.)

This is to be one of a series of country studies sponsored by the United Nations Institute for Training and Research (UNITAR) under a 1966 UN General Assembly resolution calling for studies of "the effectiveness of policies and measures against racial discrimination." Research on the United States was assigned to the Institute of Race Relations at the University of Denver. The scope of the UNITAR studies, envisaged in guidelines reproduced in an appendix to this slim volume, is here approached in a

very preliminary manner. Only an updated country study, on the scale of the Gunnar Myrdal-directed research of the late '30s-early '40s, could supply the need. Moreover, UNITAR seeks a systematic cross-national perspective, but that is not specifically built into this research design.

A genuinely comparative perspective would entail not only a cross-national comparison of race relations practices and policies, but would include intranational comparisons of ethnic, racial, and religious minorities. In the U.S., the Nisei, Chinese, Puerto-Ricans, American Indians, Chicanos, Jews, Catholics, and other minority group experiences can provide a significant analytical perspective for viewing the Negro situation. Much is made of the uniqueness of the black experience in America, and that claim conceivably may be sustained by systematic research-based analysis. But uniqueness should not be concluded from unexamined assumptions. Some aspects of each minority group's experience are unique; discovering which and why is an important research objective. This perspective would benefit a study intended as part of an international, cross-cultural research project. Regrettably, Burkey's prospectus is limited to blacks on "tactical" grounds.

What then has the author derived from the existing literature and from his own analysis to guide the UNITAR project in the U.S.? He lists his "exploratory" purposes as: (1) development of a theoretical perspective on the nature of racial discrimination; (2) clarification of related concepts such as racism, prejudice, segregation and racial stratification; (3) summary of historical patterns of discrimination against blacks; (4) assessment of changes contrasting the periods before and after World War II; (5) identification of social factors, in addition to public policies, affecting the effectiveness of public policies opposing racial discrimination; and (6) providing a guide for further research.

Since the dependent variable, which presumably can be changed by governmental policies, is racial discrimination, careful definition is crucial. The author rejects several alternatives from the literature in favor of his own: "differential treatment by members of a dominant social category which functions to deny or restrict the choices of members of a subordinate [*sic*] social category" (pp. 1-2). This definition, however, assumes that a subordinate social category is incapable of discrimination against members of a generally dominant social category. It also neglects governmental discrim-

ination against majority group members or other minority groups than those favored by a given public policy measure. Thus, compensatory employment quotas favoring one group discriminate against other applicants solely because of their nonmembership in that group.

Discrimination as an object of public policy might better be defined as an invidious distinction directed by anyone at anyone simply on grounds of ascriptive group characteristics. An objective policy analysis should not assume that all policies on behalf of one minority group's interests are *ipso facto* free from invidious ascriptive group discriminations nor that the victims of discrimination are not sometimes afflicted with the same social virus in their behavior toward outsiders. Otherwise, research on Nazi Germany would have to exclude the possibility of finding any Jews with "authoritarian personalities"; no Chicanos would exhibit anti-Negro chauvinism; and no American blacks would exhibit anti-Semitic or anti-white prejudices or practices.

More generally, Burkey's discussion of the nature of racial discrimination and related concepts is a valuable contribution. Particularly noteworthy is his discussion of the relation between discrimination and social stratification. This analysis is needed to protect the conceptual clarity of racial discrimination against the muddying confusion of an indiscriminate use of the term "racism" and particularly of "institutional racism."

The historical account of public policies against discrimination is too cursory to serve as much of a guide for the research to come. A more useful summary is that covering the literature on racial norms, along with an excellent analysis of prejudice and discriminatory behavior.

Ultimately the contribution of the book must rest on its design for analyzing the effectiveness of policy measures to reduce racial discrimination. Burkey identifies five general variables which may influence policy effectiveness: (1) internal organizational efficiency; (2) research-based knowledge; (3) enforcement; (4) support and resistance; and (5) the comprehensiveness of the policy as opposed to a piecemeal approach. More specific variables are derived from these as criteria for evaluation. The result is indeed a helpful guide, though the discussion is marred by an occasional confusion of objectives. UNITAR commissioned the author to prepare a prospectus for research, but this goal is hampered by *ex-cathedra* conclusions that pronounce a verdict on controversial policy

questions before the evidence is collected and analyzed (e.g., pp. 102-103). Nonetheless, a future UNITAR study will owe something to the prospectus provided in this preliminary effort.

HERBERT GARFINKEL

Michigan State University

Air Pollution and the Social Sciences: Formulating and Implementing Control Programs.

Edited by Paul B. Downing. (New York: Praeger Publishers Inc., 1971. Pp. 270. \$16.50.)

Political scientists interested in environmental policy will find this volume of considerable value as a bibliographical resource for studying air pollution. Sponsored by the University of California's Project Clean Air, the book contains review essays on sociological, psychological, political, legal, and economic aspects of air pollution, which seek to identify and assess what is known in each area, and offer suggestions for further research. As expected in a collection of this sort, the contributions are uneven in quality. Those by Harvey Molotch and Ross C. Follett on sociological research, Ronald O. Loveridge on political science, and James E. Krier on legal aspects are excellent—comprehensive, well organized, very useful. The essay by Robert W. Reynolds on psychological and behavioral effects is very brief—principally, I take it, because of the lack of research. Still, no effort is made to outline the many intriguing social psychological dimensions of air pollution problems. The Anderson-Crocker assessment of economics literature is disappointing, since coverage is superficial and material confusingly organized.

Paul B. Downing, an economist, provides an introductory essay that emphasizes economic and political aspects of air quality. His economic analysis is lucid and might well have served as the organizing basis for the bibliographical essay on economic aspects by Anderson and Crocker. The scientific (including health) and technological aspects of the air pollution problem-set, however, receive very little attention in these introductory remarks.

In a final chapter, Downing offers a "policy trade-off analysis model," which "attempts . . . to interrelate the physical, economic, social, administrative, and institutional aspects of problems that affect and are affected by any control policy" (p. 167). The model does identify many of the variables which are more or less involved at different times in decision making for air pollution control. And it reminds us yet

again that our individual disciplines are severely limited in doing the research necessary for policy analysis in an issue area of this type. Aside from this valuable advice to broaden our disciplinary horizons in such research and offering a catalogue of major variables, however, the model has limited practical value—either for immediate research needs or as a basis for policy recommendation.

Finally, an extensive bibliography has been compiled by Michael K. Fox—further enhancing the value of this book for the growing number of social scientists interested in environmental policy.

CHARLES O. JONES

University of Pittsburgh

The Wise Minority: An Argument for Draft Resistance and Civil Disobedience.

By Leon Friedman. (New York: The Dial Press, 1971. Pp. 228. \$5.95.)

"*The Wise Minority* will anger the fascists among us (were they to read it) . . ." proclaims one reviewer, immortalized on the back book jacket. Admittedly, such initial contact tends to "turn off" one of conservative, nonfascist inclinations. The introduction, however, seemed promising: "This book will try to suggest under what circumstances defiance of the law can be viewed as legitimate" (p. xiv).

Indeed, a book which would contribute to the formulation of a "legitimate" theory of rebellion would be an intriguing and valuable addition to the literature—assuming Aristotle, Augustine, St. Thomas, and Justice Douglas have proved inadequate. Even supporters of the Vietnam War (the nonfascist supporters, naturally), recognize claims of conscience, if not to the war they consider just and/or necessary, then certainly to a war which may arise in the future, that is, a war *they* may feel unable to support.

The book is divided about equally into two parts, the first recounting resistance to the Whiskey Rebellion, Alien and Sedition Acts, Abolitionists (reaction to fugitive slave laws), labor and farm revolts, and selected Jehovah's Witnesses, Communist, and Civil Rights cases. The second part deals primarily with the contemporary crisis of conscience—the decision to resist the draft. In brief, the book is an intellectual defense for the Resistance, an attempt to associate the Resistance with the tradition of conscience which preceded it.

Several criteria are noted during the author's discussion of the various movements, but they are never systematically developed, at least not

to my satisfaction. Friedman does note the character of the regime in which one resists, the claims of a higher moral law above the state, the problem of the initial use and kind of force brought to bear by the state, and the inadequacy of the simple test of violence or nonviolence as the sole criterion of justified resistance. Yet, it is precisely on the crucial issue of violence that he remains most ambiguous. This ambiguity would have been avoided had he clearly developed pertinent theoretical boundaries, for example, the nature of the goals sought, the realistic options available, the attitude and actions of the state, and the consequences of submission.

"I am not concerned," notes Friedman, "with reactionary violence, such as that perpetrated by the Ku Klux Klan in the past one hundred years or by other vigilante groups throughout our history. My central focus is on illegal protest as a tactic in political or economic reform movements in America" (pp. xiv-xv). In my opinion, these are precisely the weighty omissions which reflect a somewhat naive division of society's activities. In short, Friedman reconstructs the struggle of the his-ness (how nice), we are not provided with any tools to make such judgments, neither retroactively successful resistance movements and assumes their causes were morally correct. While we may all agree on their moral correctness nor proscriptively. In fact, the one piece of raw material most suitable for developing his argument to meet the issues head on—the sensitive exchange between the trial judge and defendant David Harris—is merely slapped on as an appendix.

Friedman assumes too much, belittles but does not answer the historical and theoretical arguments raised by the intelligent opposition. For example, the author could have, but did not respond to Kendall's brilliant exposition of the claim to order in "The People Versus Socrates Revisited" (*Modern Age*, Winter, 1958-1959, pp. 98-111). Likewise, Friedman discusses several instances of American "suppression" first explored by such men as Levy (*Legacy of Suppression*) and Hyman (*To Try Men's Souls*), but he ignores the fact that his crucial assumption—a certain reading of the Bill of Rights—has been for sometime open to serious question, most recently and with considerable articulation by Kendall and Carey (*The Basic Symbols of the American Political Tradition*). Friedman has an eye, a sensitive one, for pertinent material, but in this book he appears to be preoccupied with advocacy rather

than serious investigation of the topics he identified. Perhaps timeliness made him feel satisfied with so little.

WILLIAM GANGI

St. John's University, New York

The Draft and Public Policy: Issues in Military Manpower Procurement, 1945-1970. By James M. Gerhardt. (Columbus: Ohio State University Press, 1971. Pp. 425. \$15.00.)

This is an analysis of American military manpower procurement proposals and policies during the quarter century following World War II. The study is not simply a history of postwar conscription. Although the issues raised by the revival of selective service in 1948 and its subsequent extensions are considered at length, Professor Gerhardt is also concerned with legislation concerning the reserves, military pay scales, and other matters relating to the recruitment and retention of military personnel. Moreover, major proposals which were not enacted into law, such as Universal Military Training, are treated in detail.

Set in a chronological framework and based largely upon published government documents and newspaper reports, this study lays special emphasis upon certain issues raised by executive agencies, Congress, and, to a lesser extent, by spokesmen for organized pressure groups as military manpower procurement proposals have been debated in the public forum. Professor Gerhardt identifies five issues as having been important in these debates, with shifting attitudes toward them significantly shaping policy outputs. Provision for national security has been the primary concern, but budgetary costs, traditional American antipathy toward compulsion, considerations of equity in distribution of service burdens, and impacts on nonmilitary social goals have been influential. The author methodically analyzes manpower procurement policies with reference to each of these issues.

Within the limits defined by the author and, to some extent, determined by the nature of the sources utilized, the study is rigorous and well-executed. It is a significant contribution to public policy literature. However, its interest for students of defense and foreign policy would have been enhanced if greater attention had been given to relationships between American global strategies and military manpower requirements and possibilities. Although Professor Gerhardt considers such questions as the balance between active and reserve forces, which he properly identifies as having far-reaching implications for overall strategy, he

generally neglects broad strategic goals, as these have shaped manpower policies and simultaneously have been shaped by them. Moreover, the impacts of particular crises—Berlin, Korea, and particularly Vietnam—upon defense manpower policies, while given attention, do not emerge very clearly.

A final caveat may be raised concerning the scope of this book. No doubt many scholars experience external pressures as well as personal temptations to bring their research projects to early completion. Professor Gerhardt may have yielded prematurely to some such influence. The period covered by this study appropriately begins with 1945, marking the end of World War II hostilities and the beginnings of a new American military role in world affairs. But it is questionable that 1970 represents anything more than an arbitrary cut-off date. The four-year extension of selective service in 1967, when Congress rebuffed most proposals for modification of the draft, had yet to run its course. As of 1970, agitation for major reforms, particularly for institution of an all volunteer army, was growing; and future directions became somewhat clearer—if not yet altogether certain—during protracted Congressional debates over a two-year extension of conscription in 1971. Climactic changes in American military manpower procurement policies appear to be impending in the immediate future. In his final chapter the author speculates about future procurement policies, but if he had abided the course of events a few years longer, and taken such events into account, a more finished work would have resulted.

CLYDE E. JACOBS

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The Seniority System in Congress. By Barbara Hinckley. (Bloomington, Indiana: Indiana University Press, 1971. Pp. 146. \$5.95.)

The Seniority System in Congress by Barbara Hinckley, Assistant Professor of Government at Cornell University, provides a compact and systematic statistical analysis of some aspects of the seniority process. This is not a detailed history of the uses and abuses of seniority, nor a recitation of the intriguing ways various congressional committees have circumvented their seniority-selected leadership. However, you will find a careful examination of the committee leadership in relation to the base it represents. Most of the data are drawn from the 1947–1966 period. Senators and representatives are “classified by their age and congress-

sional seniority; by the electoral ‘safeness’ and regional and demographic characteristics of their constituencies; by their support for party and presidential programs; and by the ‘liberalism’ or ‘conservatism’ of their voting records” (p. 15).

Dr. Hinckley concludes that the custom of seniority is not as bad nor as unrepresentative as is often believed. In general, she finds that the profile of the Republican committee leadership is closer to the profile of its membership than that of the Democrats. In both houses, the South (in the Senate also the West) is geographically overrepresented in the Democratic Party.

She believes that “perhaps the single most important finding of this study is that the effect of the seniority system on the kind of committee chairman selected by congress is at most a limited one” (p. 108). Despite her statistical analysis which shows that the Democratic committee leadership does differ substantially from its rank and file, in several respects, she holds that the committee leaders of both parties “reflect with fair accuracy” the composition of their party in Congress.

Dr. Hinckley questions the critics’ traditional objections to seniority (such as that it rewards age, service, and one-party safe areas—especially the rural and conservative interests, obstructs party cohesion by creating independent power centers, and reinforces an antipresidential tendency). Noting that more than 40 per cent of the committee leaders change within a decade, she concludes that “any view of the venerable chairman outlasting President after President, is not supported by these facts” (p. 22). If the presidents are Franklin D. Roosevelt and Dwight D. Eisenhower, maybe so. But since 60 per cent do not change within the decade, maybe a few presidents do come and go.

With incumbents having an 85 per cent success rate, she holds that most Senate and House seats are not competitive. In the period 1947–1966, only four of the forty-eight mainland states did not have a minimum of two senators who had won at least two consecutive victories during that period and thus met the “time served” criterion for rising to a position of committee leadership. That, however, does not explain much. For example, of the 17 Democrats newly elected to the Senate in 1958, eight of them still survived in 1972, having been re-elected in 1964 (Goldwater helped) and in 1970 (an off-year). Of the eight, one (Byrd of West Virginia) was party whip, two (Williams—Labor and Public Welfare, and McGee—

Post Office and Civil Service) were committee chairmen; one (Muskie—Public Works) was second in committee seniority on his major committee; the others were either third (Hartke), fourth (Hart) or fifth (Cannon and Moss) on their principal authorization committee. The two (Byrd and McGee) who were also on Appropriations ranked seventh and eighth on the Democratic side. Three elections and almost 14 years of service obviously were not enough for several senators.

Dr. Hinckley admits that more are qualified by long service for committee responsibility than there are positions available. She also acknowledges that "factors other than congressional seniority, such as original committee assignments and subsequent reassignments, can and do influence the selection of chairmen" (p. 109); but analysis of these other factors is not a part of her work. Conceding that Democratic rural overrepresentation seems to be more dependent on "luck or discretion involved in original committee assignments . . ." (p. 110) is not enough if one is going to examine the impact of the seniority system in Congress. Nowhere is it noted, for example, that in the House of Representatives after the Democrats gained 97 seats in 1932, 75 seats in 1948, 49 seats in 1958, and 38 seats in 1964, a majority or half of the Democrats on Ways and Means who assigned the initial committees for this influx were from either the South or border states—including the chairman. One could argue that given the complexion of the Democrats on Ways and Means, more than "luck" was involved in the assignment of newcomers to "meaningful" committees.

As for the Senate, nowhere is there a discussion of the effect of Democratic leader Lyndon B. Johnson on the seniority process. As chairman of the Senate Democratic Steering Committee which made the committee assignments, Johnson developed a rule in 1953 that every new Democrat in the Senate ought to have at least one good committee assignment before a more senior Democratic senator had two. While some would argue that such an assignment correlated higher with one's agreement with the Democratic leader, an analysis of this change "before and after" might have been revealing. Until 1965, Senate Republicans with rare exception followed rigid seniority criteria in committee assignments. Dr. Hinckley's study shows that the GOP Senate committee leaders rising to the top under that system seemed to be more in tune with the profile of their rank and file than were the Democrats. Perhaps a

justification for adherence to rigid seniority was overlooked.

A few minor factual errors have unfortunately crept into the work such as the assertion, among others, that Walter of Pennsylvania, as chairman of House Un-American activities, was an example of "Southern" predominance (p. 9), the statement that "John" (p. 75) rather than "James M." Mead chaired the House Committee on Post Office in 1933, the implication (p. 79) that Knowland succeeded Taft as chairman of Foreign Relations when both went on as members at the same time and neither one was ever chairman, and the reference to Democratic party support for President Roosevelt as occurring in 1931–32 (p. 82) rather than 1933–34. One can also question whether students of Congress should refer to the House of Representatives as "the junior chamber" (p. 6)—a term annoying to those elected to the House and certainly inaccurate in terms of Constitutional power.

Nevertheless, given the enterprising analysis of much of the data available, Dr. Hinckley's work deserves the scrutiny of all who seek to unravel some of the facts and myths regarding the seniority system in Congress.

STEPHEN HORN

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Political Tendencies in Louisiana. Revised and Expanded Edition. By Perry H. Howard. (Baton Rouge: Louisiana State University Press, 1971. Pp. xxxvi, 476. \$12.95.)

The state of Louisiana occasionally conveys the impression of having been caught up in a political nightmare; but from a different perspective, it may be seen as a political observer's dream. In point of sheer variety, Louisiana equals or exceeds the claims of virtually every other state to a diversity of political experience that has run the full gamut of problems to be found in the political history of the United States. When it was acquired as a territory, Louisiana was already involved in the politics of ethnic groups; it passed through a period of competitive two-party politics in the 1830s, '40s and '50s; it suffered the trauma of rebellion, attempted secession, imperial reconsolidation, reconstruction, and solidification under Bourbon rule; it has passed through periods of dissent from the left in the form of Populists, Progressivists, and Socialists, and from the right in the form of Dixiecrats, Goldwater Republicans, and a considerable range of purely local troglodytes. It is probably the only southern state which paralleled selected states in other sec-

tions of the country in developing an authentic boss rule in its major city in the nineteenth century; and it is certainly unique in having spawned Huey Long and perpetuated, for more than thirty years, the dynasty he founded.

These claims to a special place in the annals of American politics are amply borne out by the quality and quantity of the political literature inspired by the state. At least one reviewer has indicated that T. Harry Williams's life of *Huey Long* is the best political biography ever produced in this country, and more than one commentator has judged *All the King's Men* to be the finest political novel written by an American. And concentration on the flamboyant Huey Long should not be allowed to obscure such minor masterpieces as Roger Shugg's *Origins of Class Struggle in Louisiana*, a neglected study (outside of Louisiana) in which the demographic basis of political conflict is examined at a level of sophistication rarely achieved in the social sciences. Nor should one fail to mention in this connection the late A. J. Liebling's comico-serious *The Earl of Louisiana*. In Liebling's sybaritic response to *haute cuisine*, the splendor and high risk of the race track, and the symphonic violence of the prize ring, Louisiana found a sympathetic and near-perfect recorder of the state's propensities toward the practice and observation of politics as a hedonistic art.

In the pedestrian world of historians, sociologists, and political scientists, it is not easy to keep pace with such fast company. But in the new edition of *Political Tendencies in Louisiana*, Perry Howard has, at the very least, established a claim to having written the most complete historical analysis of the general voting patterns within any major political subunit of the United States. The book is a product of more than twenty years of devotion to the subject by Professor Howard, who is a transplanted Yankee by choice, and a sociologist by profession. The maturity of the study not only reflects the length of time the author has been involved personally, but also reveals an even lengthier set of contributions to the conceptions and methods utilized in it. Professor Howard is a student and long-time associate of Professor Rudolf Heberle who, under the influence of Andre Siegfried's work on *geographie électorale*, advanced the study of political ecology, first in Germany, and then in the United States, with a special emphasis on Louisiana. Unfortunately, this line of endeavor has had less direct influence on the field of voting behavior than it should.

The quality of Professor Howard's study that stands out most clearly is its relative simplicity. The book is based on a careful use of aggregate data, which are examined in relation to the geography, socioeconomic conditions, political institutions, issues, and political personalities which have influenced the history of popular political participation and its results in Louisiana. The author is careful to warn us that the word "tendencies" is to be taken literally; he lays no claims to having established causal antecedents for the phenomena described. Furthermore, he is talking about trends on the part of *aggregations* of voters, and not about definitive conditioning of individuals who comprise these aggregations. Clearly the assumption is that the voter exercises choices which, although influenced by numerous background factors, remain open to rational assessment.

The method culminates in the identification of nine "voter-type areas" within the state; and the voting trends within these areas are compared and contrasted throughout Louisiana's political history. The persisting patterns of competition and conflict consist of such classic divisions as those between ethnic groups (in this case the French-Catholic and the "American"-Protestant groups), planter and farmer, laborer and proprietor, northern and the southern geographic sections of the state and, omnipresently, blacks and whites. Out of the complicated permutations of these perennial pairs of opposing forces come patterns of political domination and subordination, struggles for political power, and policy orientation of various regimes, which, arranged chronologically, constitute the state's political history. Howard uses relatively uncomplicated statistical measures, along with exhaustive historical sources, to examine the way in which gubernatorial and presidential election patterns emerge out of this welter of influences. The result is a complete and intellectually satisfying assessment of a political system as it has operated in historical depth.

This book is not merely a long-delayed second edition; to all intents and purposes it is a new book. Not only is it twice as long as the old volume, but it exhibits an assurance in the handling of materials, a continuity of treatment, and a firmness of conclusions that render it virtually incomparable in relation to the earlier edition.

One may quibble over a few details: the graphic materials, for example, are full-page and excellent in their clarity and composition;

but, despite text references to map numbers, the maps themselves are not numbered. And the failure to use historical maps—which were used in the first edition—tends to be distracting, because so many of the contemporary configurations of the parishes are left blank in the maps covering elections prior to the establishment of more recent boundaries. Although the quality of writing in the current edition shows a great improvement over the earlier one, and although the book is singularly free of sociological (and/or political science) jargon, the syntax occasionally leaves something to be desired.

But such faults are trivial in the context of a volume that tells us so much about the politics of a particular state, and suggests so many useful ways to try to understand what the practice of electoral politics is all about.

WILLIAM C. HAVARD

*Virginia Polytechnic Institute
and State University*

Food and Drug Legislation in the New Deal.

By Charles O. Jackson. (Princeton, N.J.: Princeton University Press, 1970. Pp. 249. \$7.50.)

Using both public and private sources, Professor Jackson has written a narrative case history, with all the limitations that implies, of the 1938 Food, Drug, and Cosmetic Act. The author is a historian, and one cannot help but be impressed by his solid research. He offers few theoretical conclusions of a general nature and is satisfied to note that the evidence in his study seems to support the thesis of Professor Gabriel Kolko (*The Triumph of Conservatism*, Chicago: Quadrangle Press, 1967) that legislation affecting business will not pass unless and until it receives endorsement from the business community. This reviewer concurs with that conclusion.

While I cannot claim competence to judge the value of the study to historians, it does seem to be an accurate and thorough review of the events and people interacting during the five-year legislative struggle. Political scientists, however, will be disappointed in the study's methodology and conclusions. No attempt is made to compare this case study with others of the legislative process. No interest-group theory is tested, nor does the author seem to realize that the study reveals in stark detail how pluralist democracy fails in practice to live up to its ideals. The competition among the affected interests was fully developed. Out of this struggle should have come resolution of the issue

through compromise and consensus. Yet Professor Jackson notes, "four years had elapsed and it would appear that final passage of a drug bill was not even close" (p. 151). The author's own conclusion is that as of summer 1937, "... S. 5 was dying" (p. 150). In this contest among interest groups, where the affected interests had many influential friends and contacts, it was the veto power of the minority which prevailed. That being so, Professor Jackson surely overstates the case when he says on p. 46 that aid of the women's organizations "became a decisive factor in the passage of a new drugs measure," and on p. 213 that without the backing of the national women's organizations no new law would have passed. The evidence seems clear that the bill was dead in 1937 even with considerable backing from industry and from the national women's organizations. The unhappy accident of an improperly tested drug (the infamous elixir sulfanilamide) which killed more than 100 people finally provided the impetus to pass a bill that reason, compromise and consumer political pressure could not provide. While tragedy brought about a stronger law than otherwise might have been passed, Congress was *not* stampeded into approving legislation which really threatened the affected interests. The strongest statement which can be made is merely that some previously unregulated aspects of industry life were thereafter to be regulated. Had the author compared the history of the 1938 act with that of the 1962 Kefauver-Harris Drug Amendment he would have found fascinating parallels. In both instances, the affected interests themselves were badly divided. It was the thalidomide episode in 1962 which provided the stimulus to revive still another dying piece of legislation. Once again it was a minority of even the affected interests which prevented passage. The significance of these examples might well be considered by the defenders of pluralism.

Professor Jackson's evaluation of the importance of the women's clubs for the passage of the 1938 Act can be faulted on other grounds. In almost every instance, when they fought for a particular provision, they lost. They fought for multiple grade labeling and did not get it. They fought to keep advertising under the Food and Drug Administration and did not get it. One could go on. The author, it would seem, mistakenly equates numbers, enthusiasm and decibel level with influence.

Professor Jackson's solid and careful research is indeed praiseworthy and only rarely can one fault him for going beyond his evi-

dence. It is, in fact, the evidence rather than the conclusions that political scientists will find particularly useful.

JAMES R. WOODWORTH

Miami University, Ohio

The Minority Party in Congress. By Charles O. Jones. (Boston: Little, Brown and Company, 1970. Pp. 204. \$6.95.)

Professor Jones begins his study of the legislative roles of House and Senate minorities in the present century by outlining certain *conditions* which hypothetically have a bearing on minority behavior. Some of these are external to Congress (e.g., the "temper of the times," which party controls the presidency, the severity of conflicts and divisions within the parties), others internal (the size of the congressional minority, the strength and unity of majority and minority party leadership, etc.). Jones then specifies the "behavior" he is interested in: a range of legislative *strategies* that extend from simple "support" of majority efforts, to "cooperation" in devising compromise bipartisan proposals, to attempts to defeat majority initiatives and/or to develop distinct alternatives to them. He assumes that the range of strategies open to the minority in a given Congress will be dependent on "the combination of conditions that exist at the time." The question is whether and to what extent the minority party tends fully to exploit these options—and particularly to pursue the more "positive strategies" for which Jones admits a preference.

Not surprisingly, and through an analysis that sometimes confuses the relationship between the situational variables and the supposedly dependent strategic options (e.g., p. 73, where the fact that "the minority party began to develop a more positive role for itself" during the second session of the 89th Congress is adduced as evidence that in fact the strategies *available* to the party had shifted), Jones finds that congressional minorities faced with severely restrictive conditions have tended to limit themselves "to the weak strategies of support, inconsequential opposition, and withdrawal." More interesting are several findings with respect to "participating" minorities (where the party in the congressional minority also controls the presidency) and others enjoying a relatively "unrestricted" situation. The generally unimpressive performance of participating minorities he makes understandable in terms of the heightened incentives to activism and competitiveness experienced by the majority under such circumstances and the necessities under which the president himself labors,

to work through those congressional leaders upon whose pleasure the fate of his program largely depends. The lack of "restriction" that comes with lassitude in the presidency is found to be a decidedly mixed blessing, even when the president is from the opposing party: "Normally, if Congress is allowed to do whatever it wants, it will do very little . . . seldom has the minority party taken the initiative when the president has failed to act" (pp. 115, 130). And the "slack" which the system contains is demonstrated both by the repeated failure of relatively unrestricted minorities fully to pursue their strategic options and, more positively, by the success of House Republicans during the sixties, under conditions that varied widely in their "restrictiveness," in strengthening their central party organs and increasing their legislative productivity.

Jones effectively conveys—and, indeed, seems to share—the ambivalence felt by minority activists as they confront their range of strategic options: "responsibly" to assist in coalition-building and in the refinement of majority proposals is to compromise the distinctiveness of their own appeal and perhaps to strengthen the opposition, while to hold out for alternative proposals is to risk appearances of political grandstanding or obstructionism. He is less perceptive in exploring the peculiar tensions attendant upon the "participatory" situation—tensions to which the ranking Republican members of many committees, torn between White House entreaties and patterns of accommodation and institutional maintenance built up over the years, could currently attest. More serious yet, despite his occasional acknowledgment of differences among committees and of the "individualism" of the Senate, is his failure adequately to portray the *variety* of minority roles and strategies ordinarily to be found in a single Congress. In focusing on the minority party *"qua party,"* Jones has perhaps avoided "many complications" (pp. 6, 98), but he has by the same token given a restricted and overly generalized account of the shape minority behavior assumes and the contextual factors that have a bearing upon it.

It is interesting to speculate how Jones's analysis and assessment might have differed had he adopted a more straightforwardly inductive approach. What could one learn, for example, from a comparison of the minority roles in the Senate Medicare battles of 1962, when Jacob Javits, courted by Democratic liberals and the administration as they sought to build a winning coalition, assumed a major role in reformulating the bill, and 1965, when Re-

publicans could make only token gestures of acquiescence or opposition as the administration bill was whipped through the Finance Committee and the Senate? To be sure, certain of Jones's conditions, most notably the size of the majority and its degree of unity, would emerge as relevant indeed, not only to the leverage minority activists like Javits could hope to exert but also to their enterprise's life-chances. But also of crucial importance are the differences between the *arenas* in which the action was concentrated—(free-wheeling attempts to build a winning coalition on the floor, as compared to the restrictiveness of the Finance Committee) and between the *agents* who chose or were able to put themselves forward on the minority's behalf (Javits, whose identification and working relationships as a "constructive" partisan were well established, as compared to the likes of AMA champion Carl Curtis).

Differences among committees are of particular importance: if the 89th Congress found the Finance Committee minority frequently in a position of "inconsequential opposition," one might argue that the modal strategy among Commerce Committee Republicans was "cooperation," while Javits's Labor and Public Welfare coterie displayed a mixture of "cooperation" and "consequential constructive opposition." Or as an HEW administrator told a Congress Project interviewer in 1972:

You really have to deal with these committees differently. [As a Republican administration,] we'll normally touch base with the ranking minority committee or subcommittee man first, but [in most cases] he'll tell us to go ahead and deal with the chairman directly. He'll say, "That's where things will be decided; and he's the one we'll be dealing with eventually anyway." But it's different with Education and Labor; we'll sometimes work more exclusively with the minority there. That's a divided committee, and Quie [ranking minority member] is a little funny about these things. He really wants to handle things on his own, in his own way.

Behind such differences, of course, lie Education and Labor's history of integrative difficulties and related patterns of committee recruitment and leadership. Can it be doubted that an analyst seeking to account for the shape and the efficacy of minority efforts on any number of recent social-welfare measures would conclude that such factors are absolutely critical?

In his apparent desire to avoid the anecdotal and particular, Jones has also taken a largely ahistorical approach: he groups Congresses together for analysis according to the constraining "conditions" they share (e.g., the 59th,

67th, and 89th Congresses) rather than in terms of the evolving composition and commitments of the parties. But these Congresses often display striking differences, and Jones's quest for timeless independent variables of general significance seems to have rendered little in the way of a capacity for prediction—one of the payoffs, supposedly, of such a quest. For example, consider the question, "Are there differences between the two political parties when serving as the minority party?" Jones acknowledges that "long periods of one-party dominance make comparisons difficult," but nonetheless ventures to answer the question in a general (and negative) fashion (p. 193). The two parties thus have presumably responded to similar conditions in similar ways. But he hesitates (wisely, one suspects) to predict on the basis of this what the role of congressional Democrats might be were they to be thrust into minority status currently (pp. 189, 195–196); nor is it clear that advance knowledge of the general "conditions" under which they would be operating would make possible a much more definitive prognostication. Again, Jones's purposes might have been better served by an analysis that began with and generalized from particular instances of minority-majority behavior and interaction. To specify the constraints imposed by Jones's general conditions is only to begin to understand the variability and potential of the minority's role in Congress.

It is, however, a useful beginning, if it is not oversold as a definitive explanatory scheme. Creative legislative action must be examined on its own terms and in its own immediate context. But Jones has performed a useful service in sketching out certain general conditions that have a bearing on the incentives and prospects for success facing minority partisans, and in developing, from diverse and scattered sources, a comprehensive categorization of past Congresses in this regard. His is thus a study on which students of policymaking in Congress can build.

DAVID E. PRICE

Yale University

Northern Schools and Civil Rights: The Racial Imbalance Act of Massachusetts. By Frank Levy. (Chicago: Markham Publishing Company, 1971. Pp. 190. \$6.95.)

Massachusetts historically has pioneered in state public policy. In the nineteenth century, it was second among the states to enact a civil service law, and in the twentieth century it has led the nation in no-fault automobile insurance legislation and a bilingual education law,

among other legislation. Small wonder, then, that in 1965 the General Court (state legislature) enacted a racial imbalance act designed generally to implement the intent of the 1954 desegregation cases and specifically to achieve a balance of nonwhite students in the public schools of the Commonwealth; especially in Boston. Professor Frank Levy has accomplished a commendable job of describing the events surrounding this legislation and of analyzing the constitutional arguments and the attempts at enforcement of the law.

Levy's story is relatively brief, yet concise and well organized. He sets the stage with an introduction to the topic, then divides the next seven chapters into three basic groupings: first, on the politics of getting the racial imbalance law passed; second, on the questions of *de facto* and constitutional segregation; and third, on the processes and problems of enforcement. These divisions form the three central questions of his inquiry, and they therefore tend to keep the reader focused on the story's development.

The author makes it clear at the outset, although the book's title does not, that his focus is on "Massachusetts' attempts to eliminate *de facto* segregation in the Boston public schools." Boston, with its heavy proportion of nonwhite students, is the chief benefactor or target, depending upon one's point of view, of the racial imbalance act. Other affected cities are Cambridge, New Bedford, Medford, and Springfield. To the extent, however, that he draws conclusions respecting only the Boston scene, they cannot fairly be associated with any general assessments.

Levy's development of the central questions flows well, and he has called upon some credible written sources for much of his documentation. I find myself somewhat bothered, however, in three respects. First, I question his argument that the racial imbalance act in issue represents one of the historic "Boston bills" enacted by a discriminating legislature reflecting a long-standing tension between Boston with its Irish Catholic orientation on the one hand, and the "remainder" of Massachusetts reflecting the Yankee Protestant focus on the other. To be sure, this split once was a reality, and it resulted in Yankee moves to control the city government of Boston from the legislature. My puzzlement arises because legislative dominance of local concerns in general and big city affairs in particular always has been a universal phenomenon of American politics. The historical "father knows best" type of state-local relationship, whereby municipalities possess few or no rights of self-government and exist virtually

at the pleasure of the legislature, is legion. Boston is no special case. The heavy hand of the state legislature is still felt today, even after some home rule gains, and even though the Irish Democrats, rather than the Yankee Republicans, now dominate the legislature, a factor the author acknowledges but seems not to accord sufficient import.

Second, the author's documentation in numerous instances is insufficient. In Chapter 4, which gives a legislative history of the imbalance law, there is little evidence that Levy talked directly with any of the principals associated with background events leading to the 1965 legislation or with any legislators closely involved in its passage—the Senate President, the House Speaker, bill sponsors, floor spokesmen, or committee members. He consulted the House and Senate journals, such as they are, but it is almost a *sine qua non* of state political research that a good legislative history demands direct communication with those involved. The author's major sources of documentation seem to have been three unpublished academic papers by other writers (p. 49).

Finally, although Levy makes it clear that the book is focused on the three previously mentioned questions, he goes little further in his research design than simply to pursue these questions in a somewhat unstructured fashion. It is true that in Part II, which reviews the conditions under which a *de facto* segregation situation might be unconstitutional, he does develop and test a series of related questions. But for a book which has been adapted from a doctoral dissertation, the expectation is that he would have developed a central hypothesis or series of hypotheses and proceeded to test them in an appropriate manner.

Despite these reservations, the subject matter of this book is crucial to our systems of public education everywhere, and Levy's work represents a significant contribution to the literature. The time span under study is limited to the first four years following the act's passage, and the problems encountered in the implementation of the law during this period are clearly identified and assessed. This work assumes increasing importance in 1972 when one recognizes that the law's implementation has become increasingly difficult with each passing year. Attempts have been made to repeal the law, Governor Francis Sargent has vetoed two bills restoring withheld funds to Boston and Springfield, despite their failure to comply with the law, and Commissioner of Education Neil Sullivan has recently tendered his resignation, partly because of the staggering problems of enforcing this law.

Levy argues that passing a civil rights act, such as the imbalance act in question, is easy, and that enforcement is the big problem. "It takes a period of years rather than months," states the author. "Because a civil rights act covers the behavior of millions of people, there are thousands of opportunities for violations" (p. 131). Frank Levy seems to have appraised accurately the cross-currents of political mood within which such legislation must live and struggle.

EDWIN ANDRUS GERE

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The Modern Supreme Court. By Robert G. McCloskey. Edited by Martin Shapiro. (Cambridge: Harvard University Press, 1972. Pp. 376. \$12.95.)

At the time of his lamentably early death in 1969, Professor McCloskey was working on a book analyzing the modern Supreme Court, and had completed the sections on the Stone (1940-45) and Vinson (1946-52) periods. To cover the Warren Court (1953-69), the editor of this volume, Professor Martin Shapiro, has reprinted some journal articles in which McCloskey dealt with various segments of the long and eventful Warren period. The results are a bit unfortunate, though the profession will welcome the republication of so much of McCloskey's scholarly work in this convenient form. The essays on the Stone and Vinson Courts are well-rounded, self-contained essays, each having a beginning, a middle, and an end. On the other hand, the sections on the Warren Court are more contrived. Dividing the period of Warren's Chief Justiceship into the early (Eisenhower) years (1953-60), and the later years (1961-69), the editor has pulled together essays which, for the most part, dealt with the Warren Court one year or one problem at a time. The four essays concerned with the early Warren Court appeared as annual reviews of civil liberties decisions; three of them were published in the *Virginia Law Review*, covering 1955, 1956 and 1957 Terms of Court, and the fourth, dealing with the 1960 term, appeared in this *Review*. Thus there is nothing directly concerned with the 1958 and 1959 Terms. The material on the later Warren Court is even less comprehensive. Included are an essay on the reapportionment cases, which appeared as the introduction to the annual *Harvard Law Review* article on constitutional law for 1962, an essay on the church-state decisions of the Warren Court which appeared in an annual volume dealing with the general subject (Giannella, ed.), and some general "Reflections

on the Warren Court" which appeared in the *Virginia Law Review* in 1965. There is nothing on the Court covering the 1966-68 Terms of Court. In addition it is very doubtful whether a book which, for the most part, analyzes the Court one term at a time, can do adequate justice to a subject which requires the systematic exploration of continuities of thought and experience.

Even so, Professor Shapiro has performed a useful service in bringing together so many specimens of McCloskey's work, for he was one of our most astute students of constitutional law, and wrote in a spirited, literary tradition. As Shapiro remarks in the foreward, McCloskey

distrusted the flamboyant and dramatic. He preferred sound interpretation, that had a chance of survival, to startling originality; the right word to the flashing one; the tempered evaluation to the scintillating criticism. He was by nature judicious, and by persuasion an adherent of the measured democracy that the "sense of the meeting" produces. And he was suspicious of a Court that too precipitously proclaimed eternal verities (p. vii).

During the years when Chief Justice Stone presided over its deliberations, the Court began to devote more of its efforts to civil liberties questions, but only in a tentative and hesitant way, and without solid agreement about the nature of its role. The Vinson Court was much less hospitable to civil liberties claims, particularly in respect to the loyalty program and other by-products of the Cold War. McCloskey draws careful attention to the divisions on the Court during this period, and particularly to the contrast between Frankfurter and Black. Frankfurter, he writes, "was both by instinct and by conviction, what we loosely call pragmatic. He questioned the possibility of certitude; incertitude did not trouble him. He was in a way fact-obsessed. He looked for differences between the circumstances of one case and those of another, and differences of degree were to him not incidental but decisive. Simple and universal rules were not only impossible, but distasteful" (p. 105). Black on the other hand, "was a natural universalist. His mind reached out for the categorical, self-executing rule, rather than subtle distinctions of degree. And his need for such reassuring axioms was so great that he was able to find them, where others could not, in the unmistakable commands of the constitutional text" (p. 105). Of course, the Vinson Court's policy of "prudent self-restraint" was abandoned by the Warren Court which, in McCloskey's words, "found its role,"

and developed a maturity of thought with regard to the basic rights of man and the values of a humane democracy. While McCloskey warned against over involvement in such touchy areas as state aid to religion, where the injuries are often negligible, he welcomed the Court's position on such matters as reapportionment and race relations as effective exercises of judicial power. He concludes that the Warren Court fashioned "a meaningful jurisprudence of civil liberties," and emerged as "a major initiative-producing agency of modern government."

On the whole, McCloskey was optimistic about the modern Supreme Court. He endorsed the shift in the central emphasis of our constitutional law from protecting the business community with respect to regulation and taxation to protecting basic human rights. While the Court lost a constituency, it acquired a new one, and he felt that the Court could hold its ground in the years ahead. He thought that the Court could best serve its purpose by "a carefully reasoned doctrine, infused with the love of liberty but alert to the realities of American political life and the limits of judicial capacity." (p. 220). This book serves to remind us of the keenness of the loss we all suffered with Bob McCloskey's untimely death.

DAVID FELLMAN

University of Wisconsin

TVA and the Power Fight: 1933-39. By Thomas K. McCraw. (Philadelphia: J. B. Lippincott Company, 1971. Pp. 201. \$5.95, cloth; \$2.45, paper.)

This significant addition to the extensive literature on TVA should be of interest to every student of American government, particularly to those who have a special interest in TVA. It contains a tremendous amount of relevant, very readable historical detail, reaching far beyond the indicated years of 1933-39. Chapter 1, on "The Public and Private Traditions," succinctly yet comprehensively traces the circumstances that contributed to the setting in which TVA was created. Included are many interesting tidbits of historical background, such as the early childhood conditioning of the man most responsible for TVA—Senator George W. Norris—in an impoverished rural home where self-reliance was essential for survival of his widowed mother and eleven children. Good biographical sketches of other principal characters (Samuel Insull, Wendell Wilkie, David Lilienthal, and others) add to the reader's understanding of the events that took place.

The main contribution made by Professor McCraw is his discussion of why and how TVA came to be. His critical analysis of Frank-

lin D. Roosevelt's role is especially good; one of his conclusions is that "essentially, Roosevelt rescued the industry from its own abuses, and thereby preserved it" but that he "might have done this much with a great deal less agitation and ill will" (p. 157). The account seems to be written with considerable objectivity, and the author does not hesitate to criticize some of TVA's policies and actions. "At first glance, a government yardstick seemed especially appropriate for the power problem. . . . Actually the idea was particularly invalid for the power business . . . when applied to a multiple purpose enterprise such as the TVA, the notion contained a deep and irreconcilable paradox" (p. 30); "the Authority engaged in some of the kinds of public relations for which Norris, Roosevelt, and others had earlier condemned the private tradition" (p. 148).

One almost gets the impression that destiny decreed the outcome: McCraw writes: "In retrospect, the real wonder is that a single TVA ever materialized. The reasons that it did lie in the history of American crises politics during the twentieth century" (p. 159).

The last chapter, "A Successful but Unrepeated Experiment," is a good explanation of why TVA is still unique, although Roosevelt and others viewed it as a model to be emulated elsewhere:

Wilkie and the private tradition lost the battle for the Tennessee Valley; but they won a qualified victory in the war against public power and valley authorities elsewhere. . . . This paradox—that TVA, though successful, was never duplicated—resulted in part from the private tradition's own exertions, but even more from changing political conditions and opposition to the TVA idea within the government itself (p. 140).

No doubt private power adherents will affirm that the book has a public power bias, but it impresses me as a well-balanced, factual, and insightful account of the interplay of many complex forces and individuals involved in one of the greatest power fights in U.S. political history.

VICTOR C. HOBDAV

The University of Tennessee

Radicals or Conservatives? The Contemporary American Right. By James E. McEvoy III (Chicago: Rand McNally and Company, 1971. Pp. 167. \$4.95.)

Power on the Right. By William W. Turner. (Berkeley: Ramparts Press, Inc., 1971. Pp. 272. \$5.95.)

Journalists and social scientists continue to find the American radical right of considerable interest, and each profession has developed its

own modal approach to the subject. The two books reviewed here, *Power on the Right* and *Radicals or Conservatives, the Contemporary American Right*, represent in turn, the journalist's expose on the radical right's danger to American society, and the social scientist's exploration of propositions on the sources of popular support for radical right movements with reference to survey data.

Radical right activists in the 1960s are the specific subject of both books. William W. Turner describes leaders and the tactics of a variety of organizations in *Power on the Right*. James McEvoy III describes the attributes of voters labeled as rightists in 1964 because of their preference for Goldwater, and as rightists in 1968 because of their preference for George Wallace, and asks if the supporters of both candidates are *Radicals or Conservatives*? Both authors point out that there are differences among right-wing movements with respect to the characteristics of their followers, their strategies, and their goals, and contend that all right-wing activists cannot be dismissed as naive about the acquisition and use of political power. These are the only similarities between the books and are derived from different evidence and analyses. Yet each book, considered in its own terms, is unsatisfactory, for each author overlooks his findings in generalizing about the radical right.

Power on the Right is a collection of reports on right-wing groups based on interviews with their leaders and with informants, and on news coverage of their activities. It will be useful to students of American radical behavior only in the sense that, as journalism, it provides some reference to the location and intensity of activity of several current right-wing organizations. Turner's reports on the right are too often a vehicle for discussing the Federal Bureau of Investigation. Turner, a former FBI agent, the author of an expose on FBI tactics, and a defense witness in trials involving the Bureau, is obsessed by the power of the Bureau. He is sympathetic to the erratic, militant radical right leaders whom he considers victims of frame-ups or harassment by the Bureau. Turner claims it is not these leaders who have the "power on the right," but the articulate and wealthy "establishment right" who, with the support of the FBI and some elected federal officials, may achieve right-wing government for the United States. Turner's interpretation may be worth a hearing, but it is not accompanied by evidence or analytical insight sufficient to make it worth consideration as political research.

McEvoy's inquiry about alleged right-wing voters is based on analysis of the Michigan Sur-

vey Research Center's 1964 and 1968 national election surveys. McEvoy prefaces this inquiry with a separate study of radical right phenomena. He presents the evidence of poll data and of trends in the circulation of right-wing periodicals and in the news coverage of the radical right, to demonstrate that there was an increase in the audience for rightist rhetoric from 1960 to 1965. The introduction to the analysis of the election survey data is a review of propositions from the literature on the characteristics of supporters of the right wing. The review is not comprehensive however, and would not be a satisfactory summary reference to proposals about the characteristics of right wing clientele. McEvoy omits from the review chapter two characteristics frequently attributed to such clientele: (1) They espouse fundamentalist Christian doctrine; and (2) they are socially and politically estranged because they live in areas undergoing rapid urbanization and economic expansion. The centrality of these proposals about the characteristics of rightists is apparent when McEvoy does cite them later in his discussions of the findings from his analysis of the election data. He discusses the religiosity of both Goldwater and Wallace voters. He cites the proposition on the experience of status strain as a result of experiencing economic change in discussing extremism among Goldwater supporters. The development of his arguments would have been served better by including these propositions in his review of the literature on the right wing.

McEvoy presents a good cross-section of the election survey data that bear on the social characteristics and political preferences of Goldwater and Wallace voters in comparison with other voters. When his interpretations of each electorate are based on careful analysis of the data, these are informative. He finds some similarities in the sources of support for Goldwater and Wallace. Both candidates appealed to voters in the South and to voters living in small towns. Neither candidate attracted voters distinguishable from the rest of the electorate by authoritarianism or status anxiety. Goldwater and Wallace voters were somewhat less sanguine about the legitimacy of governmental authority and more inclined to express prejudice against minority groups than other voters. But McEvoy reports findings indicating that the differences between Goldwater and Wallace voters were more extensive than were the similarities. A considerable proportion of Goldwater supporters were high in social status as indicated by level of education, occupation, and church affiliation. Wallace supporters were drawn much more heavily from the lower class

and middle class; they were less well educated than the rest of the electorate; and a considerable proportion indicated a preference for fundamentalist religious beliefs. For the most part Goldwater voters were Republicans and were active in electoral politics, while Wallace voters were more likely to be Independents, Independent-Democrats or Independent-Republicans, without a high level of political involvement. A belief in right-wing ideals was shared by many Goldwater voters who were concerned particularly to curb governmental authority. Wallace voters were *not* partial to rightist ideals and, insofar as they were concerned with the need for law and order, they were not opposed to governmental authority *per se*.

In his conclusions, McEvoy chooses to emphasize the similarities between these two rightist electorates to the extent of neglecting these differences. It is an unfortunate choice of emphasis and results in generalizations about all American right-wing movements that are either contrary to the findings or inappropriate.

The generalizations encompass a definition of right-wing movements; the social conditions associated with their attaining a popular following; the grievance shared by their supporters; and the attitudes expressive of the supporters' grievance. In discussing these generalizations, however, he either has no supporting evidence from the data, or he has presented contrary evidence. For example, no questions on the surveys tapped the sense of a loss of prestige that he says was shared by Goldwater and Wallace voters and by supporters of rightist movements in general. Furthermore, he suggests in one place that loss of social support propels people into right-wing politics while indicating in another that Goldwater supporters had had past histories of political involvement and were not suffering from status anxiety or a loss in status. Finally, when McEvoy stipulates that hostility toward outgroups is the characteristic expression of rightists' grievances, and one that the Goldwater and Wallace electorates share with past American nativist movements, he neglects the differences in the bases of prejudice he found between the two electorates. Finally, these generalizations violate his appropriate introductory admonition against attaching a common label to all political groups right of center.

SHEILAH R. KOEPPEN

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The Politics of City Revenue. By Arnold Meltsner. (Berkeley: University of California Press, 1971. Pp. 303.)

Arnold Meltsner analyzes how city officials in Oakland, California obtain money to run

the city. Unlike many studies with similar titles, this one really focuses on the *political* aspects of city revenue. The book has broad relevance for students of urban politics because raising money relates to many central aspects of governing a city. Also, Oakland's political system and socioeconomic characteristics resemble those of many other cities in the 350,000 to one-million range.

Professor Meltsner describes Oakland's fiscal situation (an erosion of resources coupled with large expenditure requirements), the behavior and orientations of the main fiscal participants (the city manager, city council, and department officials), and its "sleepy political system." The latter is crucial in understanding Oakland. Its political system—as is true of an increasing number of cities—is "reformed": it has fragmented institutional authority, a separation of politics from administration (the top executive is a "nonpolitical" appointed city manager), formal and informal nonpartisanship, and weak and inactive parties. Thus "in Oakland one has to look hard to find politics"; it has a "non-political" political system" (p. 49). After characterizing Oakland's political system, Meltsner analyzes the revenue process and the attitudes of the city's citizen-leaders. He concludes by discussing policy options open to city officials.

The broad analytical themes throughout the book are most significant. First, in Oakland, and perhaps in many other cities, "the politics of taxation is bureaucratic politics." It is dominated by appointed officials who keep it insulated from the citizens, most of whom do not participate in tax politics anyway. Conflict is avoided by selecting taxes (indirect ones and nominal charges) that reduce the tax consciousness of the payer; this results in small yields and small attentive tax publics. The goals of the political actors in the black box are at least as important in shaping the outputs as are the political system's inputs. The analysis here is especially useful because we have learned from other urban studies that the pattern of bureaucratic dominance and insulation from elected officials and citizens often characterizes other policy areas (welfare, police, and schools). Indeed, I infer from other Oakland studies that this is the pattern in these other areas, especially since even the city's top executive—the city manager—is appointed and rather insulated. (Meltsner does not adequately indicate the city manager form as an extreme case of this general pattern of insulation.) The second theme is that the federal structure of our fiscal affairs means that the cities' potentially best revenue sources are dominated by federal and state governments. Third is the po-

litical nature of city revenue problems; fourth is that their solution is basically a political one.

These last two themes are the book's greatest strength, but they also contain its most serious gaps. Meltsner persuasively argues that "the current fiscal crisis of our cities is a political problem, not just an economic one. Although there may be some poor states, some poor cities, and some undernourished rural areas, there is no long-run scarcity of resources" (pp. 248-49). Unlike some critics, Meltsner does not use the term "political" here as a catch-all to explain what appears to be irrational behavior—inadequate decision making, balkanized local governments, inept local officials. He means "political" in the sense of getting revenue plans accepted, dealing with conflict and developing agreement and support. The problem is ascertaining the political limits of a proposed tax and developing a strategy to get it accepted by the public. The most realistic questions about taxes are their "political elasticity—how does public tax support or resistance change when tax rates are changed?" (p. 251). The most significant problem is not one of scarcity, in Oakland and in most larger cities; the money is there, though probably less so than fifteen years ago. The problem is getting the people to agree to be taxed, and *for what*. (A recent federal survey of Oakland and other large cities confirms that the potential resources are there, though they have declined in the last fifteen years, and tax spenders have increased.)

Meltsner's analysis of Oakland's political and fiscal affairs convincingly supports this argument. At a time when there is so much wrong-headed rhetoric from public officials and analysts about the nature of cities' financial crises, and when political science is too often apolitical, I found Meltsner's analysis refreshingly realistic on the whole. I would like to have seen, however, more detail and richer data on the exact operation of the political process of revenue raising. Perhaps this shortcoming is due to the limits of the case study approach or to Oakland's "sleepy political system." Nevertheless, initially the most important and difficult task was creating some analytical order from the empirical reality, and Meltsner has done this well.

As for recommendations, Meltsner suggests that local officials must see the problem as a political one and develop a political solution:

[They should] question the assumption of taxpayer hostility and take sufficient action to *build coalitions* for increased levels of taxation and public expenditure. . . . [But] they should also try to avoid public conflict. . . . The fiscal crisis is in reality a crisis of political leadership. Officials have to walk a thin

line between a threatened taxpayer's revolt and a poor people's riot. The resources though are there (p. 11, emphasis added).

But Meltsner does not specify these tactics sufficiently. Moreover, to the degree they are spelled out, given the nature of politics in our large cities, they tend to be unrealistic and thus sometimes almost irrelevant. For example, a major element in Meltsner's political strategy is the creation of tax coalitions. But with a few exceptions, his explanation of how this and other strategies can be developed are broad, almost abstract generalities (e.g., "Depending on how much money the official needs, the [size and complexity of the] tax coalition . . . will vary . . ." p. 263).

Perhaps so many of his suggestions remain at this level because the tactics necessary to implement this strategy are not realistic for Oakland and similar cities. They slight a major policy consideration—circumstance or context—which here is the nature of the political systems of these cities. For example Meltsner suggests that "there are some clues for building a winning coalition. The local official first should develop a support nucleus composed of citizen-leaders who will work and donate resources for developing community-wide support The appeal should be tailored [so that] each individual of the support nucleus . . . sees some personal payoff to the official's proposal rather than relying on a vague sense of civic do-good" (p. 265).

But to build coalitions, the means and resources must exist—appropriate structures. In Oakland, and increasingly in many other cities, the "reformed" and amorphous political system does not provide them. (Thus, for example, my analysis of Minneapolis indicates many of the patterns present in Oakland.) Parties often serve as a vehicle for creating coalitions as well as issue deflectors, which are very important in tax matters. In Oakland parties are generally weak and are inactive in the formally nonpartisan city government. Organized groups—another potential vehicle for forming coalitions and revitalizing parties—is also in short supply in Oakland, as Pressman has indicated.

Political leadership and informal factors are also important. Meltsner urges "political officials [to] assume a certain measure of risk" (p. 11). But this requires some incentives, and in the nature of these cities' political systems such incentives are scarce. Oakland is "an administrative city"—epitomized by the appointed city manager, who the "man on the street hardly knows exists, [though] if anybody runs the city, he does" (p. 50). The city manager lacks such

risk-taking incentives as an orientation toward program development and expansion and an ambition for higher elective office. Rather his operational code emphasizes "the separation of politics from administration," efficiency (judged by a low property tax), and "fiscal realism" in finding new taxes (pp. 50, 84). Even more generally the stakes in Oakland politics—including city expenditures—are not sufficient to attract ambitious risk-takers. Instead, they turn to the federal arena, such as poverty programs. One black activist told Meltsner, "Why bother with the city? It has nothing to offer." (Again the pattern of federal dominance.)

There also must be incentives at the taxpayer level: the services and programs for which the money is raised. But Meltsner seems to overemphasize the politics of the revenue side—generating public support for a tax. Citizens' reactions to taxes probably are based on both what they are paying and what they are getting. More attention should have been given to the politics of the expenditure side (conflict among citizens about how taxes should be spent—Oakland has a large black population; the role of organized service providers in shaping expenditures and conflict with their clients), and how this relates to the revenue side. This is especially true because in Oakland the question of "who cares" seems to be more important than the question of "who governs" (p. 49).

Because of this political system and its lack of political resources and incentives, Meltsner's strategy for a political solution for the fiscal problems of Oakland and similar cities remains incomplete if not somewhat irrelevant. To implement this strategy perhaps we also need a strategy for revitalizing these cities' political systems which have been sapped by "reform" and more indirectly by changes in the economic characteristics of our population, changes in our parties and citizens' relationships to them, and changes in federal-city relations. Nevertheless, we are greatly indebted to Meltsner's pathbreaking study which has properly focused on the political aspects of the revenue problem.

MARTIN A. LEVIN

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Leadership Development for Public Service. By Barry A. Passett. (Houston: Gulf Publishing Company, 1971. Pp. 135. \$7.95.)

Given the rapidly increasing interest and concern with management development in recent years, it is surprisingly one of the less generally understood aspects of public personnel

administration today. One convincing explanation for this existing state of affairs can readily be attributed to the woefully weak theoretical foundation underlying management development. A quick survey of the burgeoning literature immediately reveals a bewildering variety of forms, innumerable philosophies and approaches, and the emergence of issues and questions unaccompanied by persuasively argued explanations or prescriptions. Moreover, like many other books of this genre, this one by Passett fails to offer a purposeful theory of management development.

The publication of this book makes it understandable that much of management education carries the mark of opprobrium that it does today. If the art of the training and development profession is to flourish, it will need to free itself from the preliterate culture engaged in recreating the known or the obvious; of confusing re-search with research. It will need to apply its talents instead to the creative enterprise of systematically building upon the existing body of professional knowledge. Passett's book is heavily descriptive, weakly prescriptive, woefully unsystematic, and completely nontheoretical. It might even encourage some of the training and development professionals to become apostates from the whole management development enterprise.

While Passett's book purports to focus on leadership development for the public service, what is actually advanced is a mélange of issues which scarcely bear any direct relationship to the topic. Fundamentally, the first few chapters of the book focus generally on the proclaimed discrepancy between promise and performance in the area of management. Passett's thesis is that the future of the administrative process is critically dependent upon the availability of managerial (leadership) talent, and the greatest benefit for society as a whole would be to develop this leadership potential for the public service. What immediately cripples his argument, however, is the looseness of his operational definition of the vital function of leadership. For the author, a leader simply consists of "one who is responsible for others . . ." (p. 2). In other words, he does not view leadership as a process which is goal oriented. Nor does he view leadership as mediating between the task orientation of the organization and the human needs of the individuals in order to secure at least a modicum of satisfaction for both. Needless to say, the noetic rationality of Ramos' "parenthetical man" would not find room in this book.

Furthermore, the book makes no attempt to determine what a leader does, to ascertain what attitudes he holds, to analyze how he behaves, or to explain why he behaves as he does in an organizational setting. Passett does not elaborate on the diversity of leadership roles or theories posited in the literature beyond the uninspiring assumption that "[We] still do not know very much about leadership" (p. 9). And this is a book on leadership!

Actually, the explicit purpose of all leadership development programs is to extend the span of utility for those managers who may be confronted with the imminence of premature obsolescence; and for those middle managers whose past performance has been outstanding, and whose potential for assuming higher managerial functions is promising.

A considerably longer review would be required to enlarge upon the author's statistical inaccuracies in dealing with organizational mobility; his conspicuous omission of the impending issuance of the guidelines for executive development in the federal service; and his inadequate treatment of the evaluative process attendant on management development activity. It is regrettable that this book, the third in a series sponsored by the American Society for Training and Development as part of its continuing interest in advancing the state of the art of human resource development, is a disappointing one. Not only does it not expand any frontiers of knowledge, it fails to introduce any new ideas, promote any agenda for discussion, or analyze any of the current trends in the theory and practice of management development for the public service.

RAYMOND POMERLEAU

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The New Deal and the States: Federalism in Transition. By James T. Patterson. (Princeton University Press, 1969. Pp. viii, 226. \$6.50.)

Following twelve years (1921–1933) of rule under three uninspired Republican Presidents, Warren G. Harding, Calvin Coolidge, and Herbert C. Hoover, the American presidency was taken over, as it turned out, for the next twenty years (1933–1953) by one and then another Democrat more dedicated to action—that is, first by Franklin D. Roosevelt for sixteen years (1933–1949), and then by Harry S. Truman for four (1949–1953). These twenty years (1933–1953) of Democratic domination of the presidency, and of the

leadership also of Congress, began with the nation near the bottom of a downward economic spiral culminating in the so-called Great Depression. That disastrous period ended, and was succeeded by a slow rise of economic activity, assisted and in part directed by a program of governmental actions, mostly national, designed to restore "full employment" and to improve the general economic welfare of the nation.

This national program of recovery from the Depression, and of the growth and expansion of the economy through an active working partnership between the social, economic, and governmental forces of the nation under a program which came to be called the "New Deal," revealed hitherto unused and even unsuspected potentialities for constructive action in and by the federal system as a whole—national, state, and local.

The book by Professor Patterson, which is here barely introduced to the reader, tells ably and amply how the architects, builders, and leaders of the New Deal, shook the American federal system out of its former lethargy and provided the federal, state and local governments with new fiscal and administrative tools for the stimulation and redirection of the national economy. In place of the old governmental "hands-off" policy with respect to the economy, government was made to accept and to use its once latent powers to help revive and redirect the economy to improve life for all the people.

For the full story condensed into one small volume, I can do no better than to recommend the reading of Professor Patterson's small and excellent volume.

WILLIAM ANDERSON

University of Minnesota (Emeritus)

Revenue Sharing: Crutch or Catalyst for State and Local Governments? By Henry S. Reuss. (New York: Frederick A. Praeger, 1970. Pp. 170. \$6.50; paper, \$2.45.)

As this review is being written, the Congress is about to approve a "revenue sharing" bill ostensibly intended to improve the performance of state and local governments by giving them unrestricted funds. After all the compromises which have been extracted by senators from the less urban states, the final bill almost certainly will fail to put the most money in the areas where it is most needed, and thus (we can confidently predict) will fall short of its target. To this extent the bill will fail to follow the main proposition underlying general revenue sharing: that the main trouble with state

and local governments has been fiscal starvation.

The relative increase in expenditures during the 1960s was higher for state and local governments than for any other major economic sector, the rate of inflation was highest in the state-local government sector, and there is wide and increasing concern with state-local deficiencies and the low and falling productivity of labor in the public sector. Given these facts, many have concluded that the major deficiency of state-local governments lies not so much in fiscal starvation as in poor management, bad organization, and lack of efficiency incentives which in the private sector are supposed to flow from the profit motive.

The redoubtable author of the book under review, Congressman Henry S. Reuss of Wisconsin, adopts this second view in his statement that "local government faces a crisis in its organization and administration—a crisis that cannot be met until states amend their constitutions and laws, and local governments respond by modernizing themselves" (p. 37). This conviction leads him to advocate coupling general revenue grants with incentives for governmental improvement. "What is needed," he says, "is not a financial crutch that will prevent states from achieving reform, but a financial catalyst that will cause them to move" (p. 123).

In order to qualify for revenue-sharing grants, a state must submit plans and timetables for a whole series of reforms set forth in a statutory laundry list. The reforms are at three levels: interstate, state, and local.

The improvement criteria listed in the Reuss-sponsored bill (State and Local Government Act of 1971) introduced in the Senate in 1971 by Senator Hubert H. Humphrey are unexceptionable but have a rather dated flavor; they include the short ballot; the single responsible executive, reform of personnel practices (this might be dynamite if directed at the ills of contemporary "civil service" systems and what now passes for collective bargaining, but one suspects it is not); extending home rule powers to local governments of sufficient size and scope; overhauling state and local fiscal systems according to long-accepted principles; liberalizing municipal annexation powers, and authorizing county-city consolidation, intergovernmental contracts, metropolitan councils of government, metropolitan study commissions, metropolitan planning agencies, and like changes.

Most of these good objectives have long since been adopted in the more progressive states. The bill is therefore shooting primarily at the less progressive states where, for the

most part, the problems of urbanism are less acute. The targets are not much concerned with the problems of governing Megacentropolis, nor with handsomely structured but ossified administrative systems, nor with excessive red tape and poor performance, nor with decaying civil service systems, nor with cumbersome procedures and lack of strong central government—in short, the things which plague Megacentropolis today.

Included are some items needed nearly everywhere, such as restricting zoning power to larger jurisdictions in order to prevent zoning by smaller municipalities which exclude housing for lower-income families, and furnishing state financial and technical assistance to *metropolitan areas* (my italics) for such matters as planning, building codes, urban renewal, consolidation and local government finance.

The provisions for monitoring state-local government improvement, moreover, are not completely convincing: governors would prepare and file with the President a master plan and timetable for modernizing and revitalizing state and local governments. The President would report to the Congress regularly on the progress made by each participating state in carrying out its government modernization program.

The approach has been criticized (1) for not touching problems of bureaucratic arteriosclerosis and (2) for involving the federal government too deeply in the morass of state and local administration. There also are questions respecting incentives for improvement. Under the above-described bill, grants would be made for a period of years on nothing more than declarations of good intention, with no rewards for performance but only penalties for nonperformance, imposed by taking away funds which nonperforming recipients had become accustomed to receiving.

For understandable reasons, federal administrators are reluctant to withhold funds already promised to a jurisdiction, and hence tend to emphasize threshold activities—planning and promises to perform rather than actual performance. The exceptional cases, when funds are withdrawn, usually are accompanied by rharbbs similar to those between an umpire and a batter caught looking at a called third strike which he claims was low and outside.

In general, if we are to have general-purpose federal grants, there is much to be said for designing them specifically to improve state and local government—better the Reuss-Humphrey approach even with the difficulties mentioned above than what we are likely to get. Nonethe-

less, it must be admitted that there are formidable political and administrative difficulties in using federal funds to coerce states and localities into pulling up their administrative socks.

LYLE C. FITCH

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James Madison. By Neal Riemer. (New York: Washington Square Press, Inc., 1968. Pp. 238. 75¢, paper.)

Neal Riemer has not written a biography. We never learn if James Madison's character was forged in his youth or if he had a sense of humor. Nor has Riemer written, strictly speaking, an intellectual biography. Rather, he has set for himself the task of illuminating the political theory of James Madison and making comprehensible the strains and consistencies in Madison's thinking between 1776 and his death in 1836. Within these confines Riemer has written a fine book.

The strength of the book does not lie in the depth of its analysis. A much better book in this regard is Vincent Ostrum's *The Political Theory of the Compound Republic* (Blacksburg, Virginia: Center For Study of Public Choice—Virginia Polytechnic Institute, 1971). What Neal Riemer has done is write a well-researched, nicely balanced book on Madison which is largely free of distortion. After wading through Chapter 1 of Robert Dahl's *A Preface to Democratic Theory* (Chicago: The University of Chicago Press, 1956) to discover his brilliant analysis of faction is mostly Dahl and little of Madison, or George Beam's *Usual Politics* (New York: Holt, Rinehart and Winston, Inc., 1970) to discover he has corrected many of Dahl's errors but misunderstood the overall thrust of Madison's thinking because of his own ideological commitments, one is refreshed to find a Neal Riemer willing to match an appreciation of Madison's theoretical brilliance with a critical awareness of Madison's mistakes and shortcomings.

Riemer correctly notes that for Madison theory was intimately linked with practice: "The function of theory was to provide the statesman with a prudent guide to action" (p. 14). Madison's practical problem was how to adapt republicanism to a large state, and the success of his theory in providing a solution to this problem is laid out by Riemer in seven well-written chapters. This accomplishment alone—presenting a clear discussion of Madison's theories of federalism and the extended republic without distortion—is sufficient to make the book ideal for teaching purposes. What makes it doubly valuable is the manner in which Riemer

discusses the later Madison of the 1790s and 1820s as he faces the Alien and Sedition Acts and the threat of nullification.

Clearly one reason Madison has failed to gain his niche in the hearts of modern academia is his apparent shift away from support of a strong central government to oppose Hamilton's program and later seemingly to support Calhoun's position. Riemer shows nicely how Madison's later actions can be made comprehensible if we remember that he was committed to unification in the 1780s not for the sake of mere order but because a stable and effective central government would enhance republican rights. Without turning him into a civil libertarian, Riemer shows Madison to be more concerned with freedom of religion and speech once the national government has been erected. Thus, Madison was searching for the middle ground between tyranny and dissolution of the government. Riemer also shows that while Madison explicitly rejected the right of nullification and was clearly not an advocate of states' rights, his theoretical position during the 1820s and 1830s gave aid and comfort to the states' rights faction and probably went too far in that direction. In short, Madison tried to strike a balance between republican rights and a stable union but was pushed by circumstances too far in one direction.

This view of Madison as a spokesman for civil liberties will engender some controversy, but Riemer shows it to be the only way Madison's actions can be made comprehensible if not consistent. The argument is not entirely convincing, yet it is based on Madison's letters and tracts and not on Riemer's ideology and therefore is provocative. Aside from the usual minor quibbles about interpretation, I believe Riemer's book deserves wider discussion. Ignoring my quibbles, I can say that while it is not the last word, Neal Riemer's *James Madison* is ideal for generating in our students an understanding and appreciation of the man who is still America's best political theorist.

DONALD S. LUTZ

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Contemporary Constitutional Issues. By Alan H. Schechter. (New York: McGraw Hill, 1972. Pp. vii, 293. \$6.95.)

A quick glance at the title and the table of contents of *Contemporary Constitutional Issues* suggests that it is just another casebook of Supreme Court decisions. Careful analysis, however, reveals that Professor Schechter has made a distinctly unique and imaginative contribution to the study of public law, a contribution

which is considerably more valuable than the ordinary anthology of Supreme Court cases. Although the book does contain numerous excerpts from court decisions, it also provides many data which place these decisions in a wider political, legal, and social perspective.

Contemporary Constitutional Issues differs in two significant ways from the traditional casebook. First, the author provides comprehensive legal briefs arguing *both* sides of each constitutional issue discussed. This means that when professors and students examine these cases, they will not have to perform the sometimes impossible task of reconstructing the arguments that might have been made by the two parties to the case. With the typical casebook, which contains only the *decisions* of the Court, the public law student is forced to glean from the written opinions what the contentions of the plaintiff and the defendant might have been. This is not a very satisfactory method but is all one can use unless one has independent access to the briefs filed by the parties. By systematically outlining in the text the arguments of both parties, Professor Schechter allows the student to compare the contents of the legal briefs with the subsequent content of the majority opinion, thus enabling him to know precisely which arguments the Court chose to incorporate and which to reject in their final opinion. Schechter's underlying assumption is, of course, that the nature and quality of legal briefs do affect the outcome of court decisions and that, therefore, these briefs should be studied by public-law scholars. While this assumption may not be shared by the most extreme judicial behavioralists, I think it is an assumption which most students of public law still consider valid.

A second innovative technique is Schechter's discussion of several major social and political questions which the Supreme Court will probably hear at some future date, but which presently have not yet found their way into the court system. With such issues (e.g., aspects of federal open-housing legislation) the author constructs and briefs a hypothetical case, and he concludes the discussion with an analysis of why the Supreme Court has not yet considered the questions involved, what will probably happen when an actual controversy reaches the Court, and what would be the probable social and political impact of judicial resolution, one way or another, of the issues involved. Such an innovative technique forces the student to reject the notion that constitutional law is merely the study of the history of Supreme Court decisions. Rather, he learns to come to grips with

some of the basic and contemporary questions about the nature of law and the role of the courts in American society—without benefit of a higher authority—and to think for himself about what alternatives the Court has available to it for resolving some of the major legal-political issues of the day.

Besides containing these new and imaginative techniques, the text has two additional virtues: it is unusually well and clearly written. Professor Schechter reveals the same considerable editorial skill in this book as in his 1964 edition of the *Interpretation of Ambiguous Documents by International Administrative Tribunals*. Also, the book deals with subjects which are especially current and relevant to the student of contemporary American government. They include: (1) literacy tests and voting rights, (2) freedom of speech in the Vietnamese War, (3) confessions, self-incrimination, and the right to counsel, (4) federal open-housing legislation, (5) federal aid to parochial schools, and (6) de facto segregation and the Fourteenth Amendment.

In sum, one need have no hesitation in recommending this book as a first-rate supplement to both introductory American government courses and upper division courses in constitutional law.

ROBERT A. CARP

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Learned Hand's Court. By Marvin Schick. (Baltimore: The Johns Hopkins Press. 1970. Pp. 371. \$12.50.)

Although *Learned Hand's Court* is the third major book concerning the late Judge Hand (1872–1961) and his life's work to appear in recent years, Marvin Schick's illuminating study is an important, valuable contribution on its own, wholly different from the two others.

First a word about its predecessors. *The Art and Craft of Judging* (Macmillan, 1968) by Hershel Shanks was, as its subtitle made clear, an annotated selection from Judge Hand's many hundreds of opinions as a United States judge in New York. A decade and a half before that, the present reviewer brought out *The Spirit of Liberty* (Knopf, 1952; Vintage Books, 1959), a collection of Judge Hand's nonlegal papers and addresses that quickly introduced the jurist-philosopher to a world-wide audience. Foreign editions included five in languages other than English.

Marvin Schick's book is substantially about Judge Learned Hand, but it is much more about the bench over which Hand presided as Chief Judge—the United States Court of Ap-

peals for New York, Connecticut and Vermont. Even more particularly it is about that court, its members and their decisions and relationships, during the years 1941-1951. The other judges whose work is described and evaluated are Learned Hand's slightly older cousin, Augustus Noble Hand (1869-1954), Jerome New Frank (1889-1957), Charles Edward Clark (1889-1963), Harrie Brigham Chase (1889-1969) and Thomas Walter Swan (1877-).

The author makes no apology for inviting the reader's attention to a 350-page plus study of an appellate court one rung below the United States Supreme Court. Nor need he. Indeed the very fact that political scientists for many years "seemed to think that the federal courts of appeals were unfit for study . . ." (p. xi) became a factor in Schick's choice of judicial area for intensive investigation on a scholarly basis.

Aimed at both lawyers and political scientists, his book, he says, is "an effort to present a comprehensive picture of a lower federal court" (p. xii). But drawing that picture was not easy. Although the Second Court of Appeals in Learned Hand's heyday was unquestionably the best-known appellate court in the country, still Schick found there was a dearth of knowledge about it, as well as about courts of appeals generally. Yet here was a bench consisting of a fascinating group of judges. Two had been dean of the law school at Yale University and a third a teacher there. All six were men of outstanding quality and ability.

The author's opening chapter presents a set of concise, perceptive biographical sketches of Learned Hand and his five colleagues of the decade. Then Schick offers a short history of the New York appellate court from the days of its founding, an account that turns out to be in part a brief history of the development of the United States appeals court system.

Here it is worth interjecting that the Second United States Court of Appeals which sits in New York City is not to be confused with the New York Court of Appeals, the highest state court in New York, over which Benjamin N. Cardozo was presiding at the time of his nomination to the Supreme Court. Referring to the New York Court of Appeals, Schick writes: "Once rated among the very best courts in the country, its reputation has steadily declined" (pp. 94-95).

Schick continues by taking up the process through which Judge Hand and his associates came to their decisions and then proceeds to the matter of judicial relations, that is, relations between courts. Next the author appraises the

six judges and their views and work on the court in considerable detail.

Beginning with Learned Hand, Schick calls him "the obedient judge" (p. 154). Judges Augustus Noble Hand, Swan and Chase are grouped as three "of conservative bent" (p. 194), while Judges Clark and Frank are joined in a chapter that labels them "The Battling New Dealers" (p. 219). Some fifty pages are devoted to a study of "the business of the court," its subject matter, who wrote opinions on what, and relatedly how the court and its members fared when their handiwork went before the United States Supreme Court for review. Finally there is a summing-up that undertakes to assess "the stature of a court" (p. 348).

Quite rightly Schick gives attention to what he describes as "the outstanding feature of the court's work—except for the decisions handed down—the virtually uninterrupted friction between Judges Clark and Frank, the court's junior members" (p. 219). The two Roosevelt appointees among six all otherwise elevated by Coolidge, these New Deal judges apparently were at each other's throats a large part of the time. When the young investigator began his research he knew none of this. The scant literature available gave the impression of "amity and serenity" (p. 221). Law clerks and others who knew what the situation was really like below the surface kept very still.

But Judges Clark and Frank—two of the Yale men—fired countless letters and memorandums at each other, and it is this voluminous material on which Schick bases his revealing, not to say astounding, report. The combatants disagreed not only on questions and issues but almost as much on basic rules and procedure. This antagonistic relationship, Schick writes, "was not merely a Second Circuit sideshow"; it was also "an integral part of the Learned Hand court, going to the innermost source of that institution's vitality" (p. 219).

From references to Learned Hand by name in United States Supreme Court decisions, it might be thought that his views were regularly sustained in Washington, but "an examination of the 1941-51 record does not bear this out. For example, Jerome Frank fared far better in the High Court than his more esteemed colleague" (p. 347). If having a dissent sustained is a measure of a lower court judge's influence in the Supreme Court, Learned Hand did not come off as well as his colleagues. One of the many tables compiled by the author shows sustention of dissents as follows: Learned Hand 10.5 per cent, Chase 13.3, Frank 14.5, Clark

14.8, Swan 23.8 and Augustus Hand 27.3 (p. 340).

Summing up, Schick says that while Learned Hand's court "was not capable of attaining greatness in the sense of the capacity to influence other courts and judges," actually "greatness may consist in doing greatly what a court is capable of doing. In this sense the Learned Hand court was truly outstanding" (p. 355).

Justice Felix Frankfurter called Hand "lucky" in not being appointed to the Supreme Court. Schick tends to concur, for he writes: "Had Hand been on the Supreme Court it is doubtful that he would have become a legend" (p. 19). Part of the legend grew up around the times that he might have been elevated and was passed over. The author tells how Franklin D. Roosevelt turned back Frankfurter's efforts on Hand's behalf, and how Chief Justice Taft earlier wrote Harding this objection: "If promoted to our bench, he would almost certainly herd with Brandeis and be a dissenter. I think it would be risking too much . . ." (p. 15).

Noting that this reviewer, after bringing out the first Hand book later collected Justice Black's opinions (*One Man's Stand for Freedom*, Knopf, 1963), Schick writes that "Dilliard speaks glowingly of this noted civil libertarian. Yet it is just not possible to reconcile Hand's views on the Bill of Rights with those of Black" (p. 12). My rejoinder is simple. I admired both but for different reasons.

Learned Hand's Court is a twice welcome addition to the literature on the judiciary. First, it researches, describes, and evaluates an outstanding federal appellate court during a period of its greatest eminence. Second, it demonstrates the worth of other research into the appellate courts, their personnel, their decisions and their comparative contribution to the workings of American democracy. Physically the book is a model of clean, neat serviceable typography and manufacture with a gold-and-black jacket bearing an unusually handsome drawing of the memorable head and face of Learned Hand.

IRVING DILLIARD

Princeton University

The State of Welfare. By Gilbert Y. Steiner. (Washington, D.C.: The Brookings Institution, 1971. Pp. 346. \$7.50.)

In introducing his last chapter, Steiner accurately portrays the nature of the book he has written: "Prescription has been freely interlarded with explanation and description in preceding chapters and will not be repeated here.

The problem now is to focus on first principles—equity, simplicity, and adequacy in public relief—and on how policies change" (p. 315). The particular mix of description of programs, prescription about how better to handle the problems at which the programs are presumably aimed, and limited generalizations about the politics of welfare programs in general makes for a book that is difficult to read straight through but also for one that is filled with interesting and often insightful passages.

This book does not contain a theory of how welfare policy gets made, but no pretense is made at theorizing. In fact, even generalizations seem to be made somewhat grudgingly, although there are some interesting ones in the last chapter. The fact/value distinction is obviously not foremost in the catalog of canons important to Steiner. The lack of theory, skimpiness of generalizations, and interweaving of prescription and description will no doubt cause some political scientists to reject this as a valuable book. That judgment seems to me to be a mistaken one, however. Once one recognizes what the book is not and does not pretend to be, then it can be appreciated for its strengths. These are four in number.

First, Steiner is inventive in defining "welfare" so that it includes not just the traditional aid to families with dependent children but also public housing, food stamps, and nonservice connected pensions for veterans. It is useful to see these programs in the same context and have them jointly put in perspective.

Second, a number of the chapters present lucid histories and descriptions of public policy in specific parts of the welfare area. Chapter 4, for example, presents an excellent history of the public housing program, chapter 6 is an intriguing discussion of the food stamp program that is in part an anatomy of how a new program develops political staying power, and chapter 7 is a first-rate elaboration of the nonservice connected pension program for veterans. It is useful to have such succinct discussions of specific policies readily available in relatively tidy packages.

Third, the book is not entirely devoid of generalizations that can be compared to other findings about American politics and public policy. At the institutional level, for example, Steiner concludes that the most potent forces for change in the welfare field are strong presidential interest and leadership, federalization rather than federalism, and "intercessors"—groups that monitor "relations between the system's professed beneficiaries and the public

agencies legally responsible for relief policy and administration" (p. 321). To be effective the intercessor must be "a group with some kind of stake—fiscal, fraternal, ethical, or political—in advancing the client cause" (p. 322). The most effective example of an intercessor group offered by Steiner is the National Welfare Rights Organization.

Steiner also concludes that some institutional matters do not make much difference—for example, shifting jurisdictions over programs between executive departments or between congressional committees is not likely to alter the basic political patterns that have produced and sustained certain kinds of policies. Multiyear authorizations by Congress are also dismissed as not very important, and perhaps even counterproductive.

Within a Department, however, the organization chart may in fact be important in determining the kind of priority a program will receive. Public housing is a classic case in which bureaucratic downgrading—both by altering the level of the unit administering the program and by giving scant attention to replacing "acting" directors of the program with permanent officials—signified the lack of any serious desire in the Department of Housing and Urban Development to consider any changes or innovations.

Fourth, the book abounds in what might be called random insights. Space permits the listing of only a few:

(1) There is an excellent summary of why welfare reform moved onto the national agenda in a major way early in the Nixon administration: "Because the President chose important supporters and staff aides who were interested in it, because both the Supreme Court and the Department of Health, Education, and Welfare were staking out new approaches as the administration changed, and because there had been enough theoretical work done in the income maintenance field to make it possible to formulate specific proposals" (p. 95).

(2) There is a superb example of how substantive goals can be transmuted into procedural goals in the discussion of day care centers. The presumed goal was a real increase in day care facilities. The real goal was licensing existing facilities, with the result that a very large proportion of the budget was spent on the licensing staff and very little net increase in day care spaces occurred.

(3) The discussion of the work incentives program in the District of Columbia (pp. 68–71) provides an excellent example, typical

of many social programs, of a program proceeding on the basis of unpredicted or poorly predicted patterns of motivation on the part of the beneficiaries.

(4) Poor prediction of consequences and impact tends to characterize many social programs. Steiner's discussion of the food stamp program highlights this problem exceptionally well.

In short, this is a book that has to be read carefully in order to gain access to its wisdom. It will not satisfy someone looking for a contribution to theories of policy making or American politics or someone looking for a systematic analysis of policy making. But it could be quite satisfying for someone interested in the substance of welfare policy or interested in public policy prescriptions based on careful attention to history, case studies of past performance, competing prescriptive positions, and common sense.

RANDALL B. RIPLEY

The Ohio State University

Alexander Hamilton and the Idea of Republican Government. By Gerald Stourzh. (Stanford: Stanford University Press, 1970. Pp. 278. \$8.50.)

Much can be learned about American government and Western ideas from this study of a great statesman's thought in the light of great thinkers' works. Even when questionable, Stourzh's account generates questions worth asking, for he combines a diligent search into the extensive record left by a brilliant man of affairs with a considerate attention to the tradition of political philosophy which that man's career somehow presupposed. Chapter 1 shows how Hamilton grounded political obligation upon modern natural right, especially as taught by Blackstone; this discussion leads back to Machiavelli's teaching about the need for bringing a society back to its beginnings or principles. Chapter 2 presents Hamilton's solution to the ambiguity of the term "republic"—a stand for modern self-interest against classical virtue, pointing to Hume, Montesquieu, and Machiavelli. Chapter 3 shows how Hamilton, by relying on private passion to serve the republic and by distinguishing those few interested chiefly in reputation from the many whose chief interest is gain, struggled to constitute a nation not constituted of patriots. Chapter 4 presents the primacy of foreign policy in Hamilton's republic as another stand on the side of modern thinkers opposing ancient ones. Chapter 5 depicts Hamilton as a statesman who

sought not power but enduring fame as founder of a nation that would grow greater than Britain in a commercial-industrial age of sea power.

Stourzh makes clear important parts of Hamilton's work but not its principle of energy. The book's virtues make it very much worth reading, but the reader should be wary of its theme, the primacy for Hamilton of foreign policy. Vexed that study of political theory and of international relations "for a century and a half . . . have been divorced," although forms of government and the conduct of foreign affairs cannot be understood separately (pp. 2-6, 127-128, 133-134), Stourzh blames others for considering foreign affairs an "afterthought, a consequence, an implication of the proper ordering of domestic society" (p. 128). He suggests that Hamilton talked about "energy of government" and "forms of administration" able "to direct the passions of so large a society to the public good" merely in consequence of foreign policy (p. 163). If, however, Hamilton gave anything primacy, it was administration, comprehending domestic as well as foreign affairs. Stourzh says that "even Hamilton with his paramount interest in administration" was "forced to wrestle with problems of nation building that involved more than appeal to legal precedent or to the simple image of the social contract" (p. 37). It would be better to say that Hamilton had to handle such problems because of "his paramount interest in administration."

It signified as much for Hamilton as the many meanings of *politeia* did for Aristotle that "administration of government, in its largest sense, comprehends all the operations of the body politic, whether legislative, executive or judiciary, but in its most usual and perhaps its most precise signification, it is limited to executive details, and falls peculiarly within the province of the executive department" (*Federalist* 72). Hamilton's deepest difficulty was the "idea, which is not without its advocates, that a vigorous executive is inconsistent with the genius of republican government," for "Energy in the executive is a leading character in the definition of good government" (*Fed.* 70).

Stourzh notes "the paramount importance" of administration for Hamilton. He calls it "a recurring theme" in Hamilton that the "test of a good government is its aptitude and tendency to produce a good administration" (*Fed.* 68). And, quoting the remark that a government's goodness consists in a vigorous executive even

if inconsistent with republican government (1787 Convention: 18 June), he calls the speech containing it "probably the greatest" of Hamilton's career (pp. 39, 82-83, 234). But Stourzh does not pursue this theme at length. Hamilton's concern for strengthening nation against state and executive against legislature, not only to oppose external danger but also to suppress domestic faction, appears as a subordinate point in the chapter on foreign policy. A mere parenthesis denying that initiative and leadership are strictly "executive" disposes of the vast theme of executive leadership (pp. 201-202). The index, containing dozens of thematic entries, omits "administration," "executive," "energy," and "efficiency."

Confusion results from this neglect. In the foreign policy chapter, presenting *The Federalist's* teaching on Montesquieu's point that confederation could combine a republic's internal advantages with a monarchy's external force, Stourzh says: "This task of blending, as Hamilton put it, as early as 1780, 'the advantages of a monarchy and republic in our constitution,' was properly the core of his lifework" (p. 154). In the letter here quoted—to Duane, 3 Sept. 1780, on "my ideas of the defects of our present system, and the changes necessary to save us from ruin"—Hamilton wrote: "the first step must be to give Congress power competent to the public exigencies," lack of such power being "the fundamental defect." However, Stourzh's quote appears not in this part on confederation but elsewhere: "Another defect in our system is want of method and energy in the administration." Congress, which at first attempted "to play the executive," was led by the inconveniences to employ boards instead. But only "vesting the great executive departments" under Congress's direction "in the hands of individuals" would give "energy to the operations of government. Business would be conducted with dispatch method and system"—this would "blend the advantages of a monarchy and republic in our constitution."

Stourzh cites Leo Strauss to distinguish moderns from ancients by the primacy of foreign rather than internal policy (p. 131). According to Strauss, however, Machiavelli's novelty was not recognition of the primacy of foreign policy, for the principle that the fundamental human fact is acquisitiveness or competition, which enabled Machiavelli to state most clearly the case for imperialism or power politics, applies equally to domestic policy (Strauss, *Thoughts on Machiavelli*, p. 293). Likewise in

Hamilton, provision for defense against foreign danger, although urgently necessary (*Fed.* 8, quoted p. 137), was one aspect of a comprehensive principle requiring that executive energy be employed domestically (*Fed.* 70, 72). In outward posture and in work at home, Hamilton would not trust in fortune to provide splendid feats of virtue that would bless and keep the republic. He deemed it wiser to secure energetic administrative machinery that would operate reliably through an extended territory during an extended time to produce one general interest from many most common interests. Hamilton, even if Plutarchian in concern for greatness, meant business, not adorning a civic stage or arena with spirited displays of polemic glory. Popular representation freed the populace from continual contention, and for productive industry, while safeguarding them against their governors. Americans had accepted this governing principle, but imperfectly, he thought. Only replacing the vestiges of democratic participation by an efficacious administrative system could supply the energy to protect them against turmoil and invasion and to manage their prosperity. The people had to choose: government by the people, affecting democratic workings—or government of and for the people, effecting popular works.

HARVEY FLAUMENHAFT

St. John's College, Annapolis

The Quest for Democratic Law: The Role of Parliament in the Legislative Process. By John Clarke Adams. (New York: Thomas Y. Crowell Company, 1970. Pp. x, 214. \$2.95, paper.)

Professor Adams, of Syracuse University, has taken a vast subject for compression into a small book, and himself describes an important chapter as a "sketch," and his conclusions as "admittedly tentative." What he has attempted, in the main successfully, is a readable survey of the theory and practice of parliaments (using the word in its broad sense to include all representative assemblies in the liberal democratic tradition) with particular reference to the making of law. His six chapters deal consecutively with such considerations as the philosophy of popular sovereignty, the history of parliament, parliamentary actions and the limits on them, and the case for and against parliaments as effective instruments. His sweep is wide, both chronologically and analytically, and the documentation, offered in several languages, is lengthy.

The burden of Professor Adams's message, like most of his chronicle, is not unfamiliar. The bulk of the text, as seems unavoidable in surveys, is often reminiscent of something else: Wheare on committees, Luce (who is not mentioned) on legislative assemblies, and so on. The message is revealed in the subtitle of the last chapter: "A Case of Mistaken Identity and Misjudged Potential." It is the author's contention that the legislative function and the representation of the people's will are not necessarily the same thing, and that well-meant doctrines on the validity of separating powers—"the myth . . . with its emphasis on representative assemblies as the sole lawmaking bodies" (p. 197)—have misled both friends and critics of parliaments as legislators. "Thus," Professor Adams concludes (p. 198), "the most general and most telling accusations against parliament have been directed at the difficulties it has experienced in carrying out a task it was never equipped to undertake. . . . The traditional and the natural function of parliament is that of defender of the people."

The argument through which the author leads up to those words is lucidly presented. Purists may, indeed, be less inclined to challenge his conclusions than to cavil at his brisk treatment of their favorite theory, theorist, or epoch, for it is inevitable that a book so broadly conceived must resort occasionally to generalizations themselves so broad as to be almost meaningless. It is fair comment that the work, despite copious references to many countries' parliaments, gives no clear picture of any one.

A more serious stricture is that the author is too ready to accept uncritically, as still valid, assessments that have become obsolete. For example, his comments on fiscal control in Canada's House of Commons (which bafflingly appears in the index as "House of Representatives") are based on a 1958 source, and they ignore major changes that have since occurred, as well as a literature of some size. He also appears to assume, on dubious grounds, that parliamentary fiscal controls are not an integral part of the legislative process. Other references to Canada contain factual errors which could have been avoided by a reading of more recent works than those cited. It is not easy to have confidence in an apparently erudite work, worldwide in scope, which is so misleading about a neighboring state.

NORMAN WARD

University of Saskatchewan

Sotsial'naiia struktura sel'skogo naseleniia SSSR. By Iurii V. Arutiunian. (Moscow: Izdatel'stvo "Mysl'," 1971. Pp. 374. 1 ruble, 39 kopecks.)

Since the death of Stalin in 1953, the development of the social sciences in the Soviet Union has followed a checkered course. Among the encouraging tendencies has been the restoration of sociology from the status of a "bourgeois pseudo-science" under Stalin to an academic discipline enjoying increasing prestige and recognition. The present volume is an indication of the kind of empirical research and the level and scale of sociological investigations now being carried out in the Soviet Union.

The social structure of the Russian village and, in a larger sense, of rural Russia has been a much-neglected subject of research in the USSR since the early 1930s. Yet it was precisely in the Russian countryside that the October Revolution, especially after 1928, produced some of the most far-reaching changes and transformations. Although in itself only a beginning, the study by Arutiunian constitutes an essential and significant step toward filling a major gap in the literature on contemporary rural Russia.

The objective of this impressive monograph, as translated from the author's words, is

to study the correlation of inter- and intra-class differences, to discover the essence and concrete manifestations of the differences between town and country, between individual social-professional groups of the rural population, to reveal the developmental tendencies of the social structure, the direction and tempi of mobility of individual social groups, to compare the demands and needs of people with the social reality, and to evaluate from this point of view the perspectives of development of rural society (pp. 14-15).

The parameters of the research, carried out for the most part in 1967, reach from the intensive study of individual collective farms, state farms, and enterprises to the investigation of complex regions, including a whole republic with a population of 3 million—the Tatar ASSR.

More specifically, the data for this monograph—which focuses on the Kalinin Region (*oblast*), the Moscow Region, the Krasnodarsk Territory (*krai*), and the Tatar Autonomous Soviet Socialist Republic—were generated through the administration of a carefully planned standardized interview and a questionnaire, as well as through the study of documents obtained from various state institutions, regional and local organizations, newspaper files, and records made available by the directors of collective and state farms. The mono-

graph contains more than a hundred tables summarizing the major findings, as well as summary tables of selected aggregates, and applies relatively advanced techniques of social statistics, including regression analysis, scaling, and latent analysis. The analytical units of the study were carefully chosen to allow the isolation of significant variables—e.g., the role of the ethnic factor in social mobility, the role of education, economic status, geographic location, and the impact of varying degrees of urbanization.

The study is organized into three basic parts. Part I deals with the formation of the contemporary social structure of the Russian village and includes a discussion of its evolution since the 1920s, as well as an analysis of current trends of development. Part II focuses on the social groups in the rural population, their similarities and differences. Besides discussing the socioeconomic bases of group differences, the author here is concerned with the problem of the cultural level in the Russian countryside, as well as certain aspects of the social psychology of social groups in the rural population. Part III, finally, is devoted to an analysis of social mobility in rural Russia. The study also contains an appendix, which sets forth the methodological base of the book, and a useful index.

In many respects, the present volume is a continuation and expansion of Arutiunian's research interests. He is the author of an article dealing with the social structure of the rural population, published in *Voprosy filosofii*, no. 5 (1966), and an earlier monograph on change in the social structure of socialist society (*Izmenenie sotsial'noi struktury sotsialisticheskogo obshchestva* [Sverdlovsk, 1965]). Like his earlier work, the book under review is a very useful and significant contribution. It contains only a minimum of ideological ballast and is written in a detached and matter-of-fact language which is a welcome relief from the typical jargon and propagandistic tenor of most Soviet books dealing with social science problems. Indeed, the book in its entirety is an eloquent argument for empirical social science research and for a more refined and sophisticated application of social statistics. As far as its subject matter is concerned, this volume is a "must" for every scholar who is seriously interested in the sociology of contemporary rural Russia. It contains a wealth of factual information about the immense heterogeneity (economic status, differences in cultural levels and life-styles, etc.) of various elements of the rural population and constitutes solid proof that the Soviet Union is a long ways from realizing the Marx-

ist dream of a society in which the differences and antagonisms between town and country have been eliminated.

ROLF H. W. THEEN

Purdue University

My Odyssey: An Autobiography. By Nnamdi Azikiwe. (New York & Washington: Praeger Publishers, 1970. Pp. 452. \$12.50.)

It is a shock to find that many of the students who read African Politics for the first time have never heard of Nnamdi Azikiwe. They know something about Julius Nyerere and even perhaps about Sékou Touré, while Kwame Nkrumah has a place among past political luminaries. The reason is clear: For most of the period after 1958, when he became President of the Nigerian Senate, "Zik" played a largely dignified role in Nigerian politics. He is a flourishing businessman who may yet contribute to the politics of Nigerian demilitarization. And any student who consults the standard works on Nigeria—for example, James S. Coleman, *Nigeria: Background to Nationalism* (Berkeley & Los Angeles: University of California Press, 1963), and Richard L. Sklar, *Nigerian Political Parties* (Princeton: Princeton University Press, 1963)—will quickly realize how important was the part played by Dr. Azikiwe in the growth of Nigerian nationalism.

Only the early episodes in that story are told in this volume: it stops short at 1947 when Azikiwe, as president of the National Council of Nigeria and the Cameroons, was the dominant figure in Nigerian politics. We shall have to wait for the subsequent volume, which is promised, to learn about Zik's participation in the freedom struggle, his experience in public office, and his intriguing role during the civil war. From Nigerian nationalist to Ibo tribalist? Such a question is best left unanswered until the second volume appears.

My Odyssey fills out the picture already painted by Azikiwe's biographers, of whom one (and perhaps the best)—the Ghanaian, K.A.B. Jones-Quartey—was appointed assistant editor of *The African Morning Post* in 1935 (p. 255). It tells of Zik's early years in Northern Nigeria and of his schooling at Onitsha (the family home-town), Lagos, and Calabar. It describes how, in 1921, Zik became a junior clerk in the civil service but determined to go to the United States for further study. His first attempt to reach America, however, came to nothing. He got no further than the Gold Coast, where he became a police recruit, "bubbling with ambition to achieve fame as a police officer" (p. 60). His mother persuaded him to return to Ni-

geria. He was employed as an advocate's clerk at Calabar until his father, at considerable personal sacrifice, found the money for him to go to the United States. Before leaving Calabar, the young Azikiwe prayed to God to "teach me to be humble in my quest for greatness" (p. 74).

Chapters V-VII tell of Zik's experience in the United States over a period of almost nine years (1925-34). We learn of his academic and sporting achievements, of his debut as a journalist, and of the various menial jobs (ranging from janitor to dish-washer, from steel-worker to coal-miner) which he took to pay his way through college and Howard and Lincoln Universities. The worst period was after he left Storer College in West Virginia and was jobless in Pittsburgh; fortunately, his attempt to commit suicide was frustrated. Economic hardship left a deep mark on the impressionable Azikiwe. On the last day of 1933, faced with the prospect of unemployment, he vowed that he would not only work to eradicate imperialism in Africa but would also devote his energies to accumulate wealth and so "ensure that I shall never be in want" (p. 175).

Zik's efforts to secure lucrative employment came to nothing. He offered his services to Emperor Haile Selassie of Ethiopia "to assist in safeguarding his territory from aggression" (p. 170) and to the President of Liberia; but in vain. He found that the colonial governments in British West Africa discriminated against educated Africans; he therefore decided to become a journalist, as a first step to founding his own printing and publishing business. In 1934, he returned to West Africa and became editor of a new Gold Coast daily, *The African Morning Post*, owned by the Accra businessman, Mr. A. J. Ocansey. Zik selected as the paper's motto: "Independent in all things and neutral in nothing affecting the destiny of Africa," and his vigorous editorials attacked anything that threatened to delay the Gold Coast's independence.

In July, 1937, Azikiwe returned to Nigeria, with his name in West African journalism already established. Except for a final chapter on "Sporting Activities," the rest of the book is devoted to the founding and management of the Zik group of newspapers, and to the involvement of the latter with the law. Zik used the *West African Pilot* and his other newspapers as vehicles for the spread of nationalist ideas; but he also showed that journalism could be a successful business enterprise.

My Odyssey is a story worth telling. Most of it is well written, though some sentences are ponderous—as, for example, that beginning at

the foot of p. 187. The picture unfolds of an ambitious and rather immodest man who is rightly confident of his own ability to succeed. There are many references to Zik's philosophy. He tells us, unconvincingly, of his commitment to socialism, and that he is returning to West Africa "semi-Gandhic, semi-Garveyistic, non-chauvinistic, semi-ethnocentric, with a love for everyone, of every clime on God's earth" (p. 162). It is not easy to get behind these grandiloquent phrases and penetrate the real man. This task is made harder because Zik omits from his autobiography any intimate, personal details of his life, especially after his return from America. Thus, we are told briefly of his marriage in April 1936 (p. 260), but nothing of his subsequent family life. It is Dr. Azikiwe the public figure who emerges from these pages, and the reader is left to bridge for himself the gap between Zik's public and private life.

WILLIAM TORDOFF

University of Manchester, England

The Myth of the Guerilla: Revolutionary Theory and Malpractice. By J. Bowyer Bell. (New York: Alfred A. Knopf, 1971. Pp. xiv, 285. \$7.95.)

Revolutionism. By Abdul A. Said and Daniel M. Collier. (Boston: Allyn & Bacon, 1971. Pp. ix, 191. \$3.95.)

Peasant Wars of the Twentieth Century. By Eric R. Wolf. (New York: Harper & Row, 1969. Pp. xv, 328. \$3.95, paper.)

The study of revolutionary warfare is as full of ambushes as the technique itself, and many are the unwary scholars who have stumbled into its booby traps as a result of underestimating the complexities of the field and the need for firm positioning on the ground. Now, alas, we must add two more to the body count. In both cases, the causes of the disasters are instructive warnings to those who would venture into terrain where only the most highly trained may hope to emerge in one piece.

J. Bowyer Bell has made the elementary mistake of divorcing the guerrilla from his background. After lengthy and conscientious interviews with assorted insurgents, and a cursory study of the classic cases of guerrilla struggle, he has set out to cut the guerrilla down to size by demonstrating that he fails as often as, or more often than, he succeeds. On the basis of this finding, he is eager to dispel the myths which animate both Western radical romantics

and the more tenacious activists of root-and-branch change in the Third World.

Unfortunately, Bell has attempted short cuts to his objective which turn out to be blind alleys. His "theory" amounts to no more than a rudimentary and highly unsatisfactory outline of the role allotted to people's war in military thinking and revolutionary doctrine. An account of the Chinese revolution, for example, which treats Mao as acknowledged leader of the communists in 1927 (p. 20), attributes to him the view that the people are "immobile, without vitality" (p. 21), and reduces his theory of revolution to the three stages of "guerrilla war, positional war, and mobile war" (p. 24), hardly recommends itself as a reliable introduction to the subject.

Theory having been disposed of, Bell then tackles three case studies in guerrilla warfare. Two of them—the armed struggle in southern Africa, and especially in the Portuguese colonies, and the Palestinian *fedayeen* movements—are intrinsically interesting and under-researched. But Bell, ignoring his own maxim that "the study of war is the study of society" (p. 19), fails to come to grips with any of the central problems posed by the interaction of such factors as social structure, international relations, and leadership formation, which would enable him to present a meaningful account of the guerrilla experience and guerrilla prospects in these cases. His discussion amounts to little more than a confrontation between revolutionary rhetoric and academic rhetoric. The exercise is futile, and the honors far from even—at least the guerrilla myths perform some role in mass mobilization and morale-building, while Bell's counter-myths are homilies addressed to an audience which is irrelevant to the guerrilla enterprise.

The third case study—that of Guevara's Bolivian venture—need not delay us long. The carcass of Che's forlorn mission has been dissected too often at length to yield new discoveries in a brief outline which chiefly represents a crude psychologism in which the hero is seen as seeking "to expurgate his sins" in a suicidal undertaking (p. 211).

It is necessary to add that Bell's writing is at times incomprehensible. To cite only one instance, take this sentence: "The revolutionary-guerrilla envisioned by Mao, unlike the partisan-guerrilla, had been converted to the validity of the new ideology *fight for the future not the past*, to transform the old order, to destroy existing institutions, to create a new system, not to restore an old regime" (p. 27. Italics in origi-

nal). Insofar as I understand this assertion, it is so simplistic in its oppositions as to suggest imperviousness to the considerable body of literature demonstrating the mix of past- and future-orientations in all modern revolutionary wars.

If Bell has lost himself in the guerrilla jungle, Said and Collier have hovered so high above the scene that heavy cloud cover completely obscured their view of the ground before their timely crash. I frankly confess to bailing out well before the end. There is, after all, a limit to what even a reviewer can absorb in the way of contorted mystifications, and this sample of passages taken from the first few pages of the book contains my justification for failing to complete my mission:

"The credo of revolution has become divorced from the execution of revolutions; the intangible and seductive concept of change has succumbed to the idiosyncrasies of the political system" (p. 1). "Their systematic attempt to solve the complexities of the world provides an ideational framework to an essentially value-free political milieu" (p. 1). "With the breakdown of the Church as a primary ordering principle, man views life with confusion and the knowledge that the only certainty in life is death" (p. 2). "Revolution appeals to the hidden essence of man that rebels against order and is captivated by abstract assurances of achievement and satisfaction in the present life" (p. 2). "The nature of revolutionary phenomena is bound to the nature of man and his never-ending quest for relevance during the finite period allotted to him on the planet" (p. 2). "The faith which he seeks may be founded in the past or the present; its exploration may be facile or arduous" (p. 3). "The truth about man is not as important as the miracle that he exists at all" (p. 3). "When man can no longer share the beliefs of his ancestors, a break with the past usually translates to disruption of the present" (p. 4). "Revolutionism . . . condemns ideology yet adheres to dogma . . . It aspires to a nationalism that can only artificially arouse the populace . . . Revolutionists are those who are most sensitive to the needs of the times" (p. 5).

Teachers of political theory may find these texts instructive, but there, I believe, their value ends. However, I shall treasure the thought that revolutionism, defined as "the ideology of constant and total political change" (p. 2) united among its practitioners Lenin, Mao, Christ and Luther (p. 4). Nor should I overlook my most cherished understatement of the season: "In

the twentieth century the United States has tended to view revolution abroad as essentially an unsettling experience" (p. 7)!

Eric Wolf's study of the six great peasant-based revolutions of the century, by contrast, demonstrates a mastery of his field and the methods required to negotiate it that evokes respect and admiration. In six crisp essays and a brilliant conclusion, he extends our understanding of the nature of peasant reactions to social change appreciably by his skill in isolating and analyzing those factors, which, by a magnification of the anthropologist's techniques, can be shown to be crucial in linking local grievances and protest to larger movements of political transformation.

Wolf is not alone in perceiving the genesis of modern revolutionary movements in the impact of capitalism upon the social, economic, and cultural supports of pre-industrial and semi-industrial societies, nor is there anything novel in the emphasis he places upon the specific disruption wrought by these changes upon the peasantry as a class. Although his exposition of these themes in his six case studies is exceptionally vivid and perceptive, particularly in its discussion of land use and rights, his major contribution to the analysis of the peasant component in the revolutionary process lies in his convincing argument that "it is the middle and poor but 'free' peasants, not constrained by any power domain, which constitute the pivotal groupings for peasant uprisings" (p. 292). He demonstrates repeatedly how factors which "increase the latitude granted by that tactical mobility reinforce their revolutionary potential" (p. 293) and contribute decisively to the general outcome of the confrontation between the forces of change on the one hand and accommodation to exogenous pressures on the other. He effectively disposes of the communist mythology surrounding the revolutionary role of the landless and poor peasantry, while at the same time showing the inadequacy of those theories which try to explain the revolutionary mobilization of peasants simply in terms of such vague notions as "nationalist appeal."

Wolf does not exaggerate the role of the peasants, although they form the focus of his study. He is well aware that it is the synchronization of peasant uprisings with wider political movements which produces revolutions. Within the short compass of his overview, he examines the pivotal role of other classes, the types of nonpeasant leadership which determine the nature of the post revolutionary order, and the limitations of peasant action and

consciousness. The central paradox of the middle and free peasant is beautifully outlined: fueled by anarchism and an apocalyptic vision of the world, his very attempt to remain traditional makes him revolutionary; hence his fate "is essentially tragic: his efforts to undo a grievous present only usher in a vaster, more uncertain future" (p. 301).

From the standpoint of the political scientist, Wolf's examination of the character of revolutionary leaderships, and the manner in which they forge links with and mobilize peasant rebels, is perfunctory, and he places far too little emphasis on the importance of institutional breakdown at the national level, in most cases under the stress of war, in creating the "space" for revolutionary groups to maneuver. But it would be carping to make too much of these deficiencies in a study which is primarily concerned to hone our analysis of one crucial dimension of the revolutionary drama.

A more serious general problem is posed by the nature of the study itself. By concentrating exclusively upon the successful instances of peasant-based revolution, Wolf is driven to stress structural similarities at the expense of cultural variables. As his anthropological work confirms, he is well aware that the dilemmas of societies in the throes of adjustment to the pressures of modernity involve a cultural crisis of a far-reaching kind, but since he is here dealing with only one mode of adjustment, through revolution, he fails to elucidate the circumstances under which such an outcome proves abortive or is bypassed. He adverts in passing to the issue, noting that "the very spread of the capitalist market-principle also forces men to seek defences against it. They could meet this end either by cleaving to traditional institutions, increasingly subverted by the forces which they were trying to neutralize; or they could commit themselves to the search for new social forms which would grant them shelter" (p. 282). The "search for new social forms" does not necessarily imply a revolutionary option, of course, and the likelihood of success in resorting to this path would seem to be affected by the extent of ethnic and religious diversity and by the strength of the specific forms of cultural cohesion between traditional elites and peasantry in the community in question. By failing to explore these variables, Eric Wolf misses an opportunity to make his stimulating comparative study even deeper and richer in texture.

REX MORTIMER

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Crisis in Costa Rica: The 1948 Revolution. By John Patrick Bell. Latin American Monographs, No. 24 (Austin, Texas: The University of Texas Press, for the Institute of Latin American Studies, 1971. Pp. xiii, 192. \$7.00.)

Costa Rica is one of those few countries of Latin America, and the only one in Central America, where civilian government has almost always prevailed; where constitutional stability is more characteristic than turbulence; and where, at least since 1889, increasing popular participation has guided her political fortunes. During eighty-three years, Costa Rican government has been overturned by force on only two occasions, in 1917 and 1948. Some scholars have reflected that it is useful to political science to know why Costa Rican political institutions and processes are unique within the Latin American and especially the Central American-Caribbean context; and what were the circumstances which produced exceptions to the Costa Rican pattern of peaceful constitutional stability. Therefore, this brief study by Professor Bell, historian at Indiana University (Fort Wayne), is worthy of examination by those political scientists who are still more concerned about such old-fashioned quirks as popular rule and political stability than about "modernization" or "development."

A widespread account of the Costa Rican revolution of 1948 holds that during two administrations (that of Dr. Rafael Angel Calderón Guardia, 1940-1944; and of Lic. Teodoro Picado Michalski, 1944-1948) the relative tranquility of Costa Rican political life was disrupted by fiscal irresponsibility, administrative bungling, ham-handed executive authoritarianism, electoral corruption, and intrusion by influential Communist elements into governmental affairs. This account also has it that in 1948, in response to legislative annulment of the popular election of conservative opposition candidate Otilio Ulate and attempted imposition of ex-President Calderón Guardia, José Figueres saved the day by undertaking a revolution which restored and improved upon the constitutional order.

For the most part, Professor Bell sees the revolution in a different light. The regimes of Calderón and Picado, he believes, attempted valiantly to solve profound social questions. Communism and Communists were no problem. Fiscal corruption and mismanagement existed and were admitted by Calderón, but were not terribly important. There was indeed an electoral problem; but José Figueres, a naturally

combative person (p. 87), had been plotting revolution for from six to eight years. Furthermore, Bell tells us, the revolution led by Figueres was largely inspired by an oligarchic elite which couldn't tolerate the social reforms of 1940-1948, including especially a new income tax.

This interpretation will grate on the nerves of those scholars who had supposed that the electoral question, including what Bell rightly calls the "fraudulent election of 1944" (p. 95; and see p. 112) and the grave irregularities of the election of 1948 (described accurately on p. 130), were central causes of an unusual revolution by democratic Costa Ricans of all ideological persuasions under the leadership of José Figueres. The traditional version does seem plausible when one considers the following: (1) José Figueres turned out to be a reformer no less radical than Calderón or Picado (see pp. 160-161); (2) he did in fact turn the government over to Otilio Ulate in 1949; (3) the Constitution of 1949 assured more than did any previous one the preservation of the institutions and processes of democracy; (4) José Figueres was elected to office by popular vote in 1953 and 1970; but (5) opposition candidates were freely elected in 1958 and 1966. A fact which Professor Bell does not mention is that Calderón Guardia, who died in 1970, was a very widely known and highly regarded family doctor as well as a babe in the political woods. Thus his regime, and that of Picado which relied on his leadership, suffered from serious deficiencies and naivetés which helped to push Costa Rica over the brink of armed revolt.

Professor Bell would have improved his case if he had provided more impressive substantiation for many of his claims. He tells us repeatedly (pp. 45, 95, 98, 131) that Figueres or his followers had been fomenting terrorism since 1940, but gives us no sources except a biased pro-regime newspaper, *La Tribuna*, and a Communist sheet, *Trabajo* (p. 99, n. 74 and 75). There are sweeping, often unsupported generalizations about the Costa Rican class system (pp. 8-9), the thoughts or actions of this or that class (p. 24), alleged problems of intense misery (pp. 20, 27), or the "call for modernization" (p. 81). Despite a thirteen-page bibliography which includes most of the scholarly sources on Costa Rica, the author occasionally relies on newspapers or authors (e.g., Carlos Luis Fallas, Manuel Mora) who were notoriously biased.

The case that Professor Bell is trying to make deserves a hearing, and other readers

may be more impressed by the verification he provides. I cling to the view that in Costa Rica, a successful revolt requires more than the efforts of a rebellious man in combination with a disgruntled oligarchy. Otherwise the causes of the unique political character of this Central American republic, or for that matter of many persistent patterns of difference among polities, are reduced either to simplistic doctrines of psycho-economic determinism or to meaningless happenstance.

JAMES L. BUSEY

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Political Violence & Public Order: A Study of British Fascism. By Robert Benewick. (Baltimore, Maryland: Allen Lane, The Penguin Press, 1969. Pp. 340. \$9.95.)

Of all the irregular events that have punctuated modern British politics, few have been as unnerving as the scenes played out in the 1930s by Sir Oswald Mosley and the British Union of Fascists. While the episode might easily be written off as a fringe response to the interwar crisis, it continues to trouble observers of the British scene. British "traditions of civility" are, after all, the model that others follow. Rather than dismiss the BUF simply as a nuisance, therefore, students of British politics have sought to explain the rise and impact of the BUF. As most attempts have thus far been only partial, Robert Benewick has performed a distinct service with this straightforward and fairly complete study of British fascism between the wars. His study is all the more welcome as a corrective to Mosley's own dreary and egocentric apologia which has recently appeared in this country as *My Life*.

Although British fascism in the 1930s was irreducibly Mosleyite, Benewick wisely paints the larger picture of the movement as well as the man. By so doing, he provides a fascinating if plainly factual picture of the BUF as a mass movement. What is striking is the extent to which the BUF fits the models of mass movements developed by such writers as Kornhauser or Selznick. The BUF was characterized by such "mass" characteristics as promiscuous recruiting, organizational instability, rapid membership turnover, ideological restlessness, and a socially and economically marginal membership led by dispossessed middle-class subleaders. It never developed the discipline, the social base, the sureness of purpose, or the organizational solidity necessary for a really serious political movement. Such influence as it had was due to its ability to gain publicity through

violence and theatrics and by the catalytic presence of the Mephistophelian Mosley. (Benewick is altogether fair on the former point in showing that much of the violence that accompanied Mosley's meetings was caused by others—though provoked by the fascist style and politics.)

While Benewick avoids attempts at psychohistory in dealing with Mosley, he does supply sufficient data for us to reject the hitherto conventional and uncritical interpretations of the BUF leader. As against the tiresome assumption that he could have been leader of either the Labour or Tory Party, Benewick resists the temptation to dwell on the myth of the "tragedy of Mosley." That Mosley was endowed with more than enough charm, intelligence, competence, and even charisma can hardly be doubted. But, as Benewick emphasizes, he also demonstrated qualities of mind, character, and ideological instinct that made him quite unsuited for leadership within the constraints of the system. Like others at the time, Mosley was a dissatisfied and restless loner seeking an outlet that the system could not provide. He was an aristocrat as mass man. Benewick is partly right in terming him an opportunist. But an authentically unprincipled politician who had tasted the popularity and power that Mosley had by 1929 would surely have bided his time and not sought to gain power, by forming first an "oppositionist" group (the New Party), and, then, a party which looked across the Channel to foreign models. As Benewick points out, the New Party may have started where the Keynesian Mosley Memorandum left off, but it began to evolve almost immediately in an autocratic direction. By the time he visited Italy in early 1932, Mosley was already emphasizing characteristic fascist values: youth, character, race, nation, energy, manhood. Both Mosley's impatience with the Labour Party and the subsequent evolution of the New Party, then, prefigured the formation of the BUF rather than reflecting simple opportunism. Adventurer though he may have been, Mosley nonetheless moved within fairly stable ideological parameters.

Interestingly, Mosley's fascism was initially something other than it was to become or than it appears now when measured by contemporary experience. It was the "modern movement," stressing the values of action, youth, decisiveness, resolve, and discipline. Like other movements at the time, especially those on the left, it was a response to the political, moral, and economic failures of the system. Though it learned from Italy, it sought do-

mestic roots, concentrated on domestic reform, and urged national renovation. While it began as a reformist group, however, it developed into a movement of reaction and of revolutionary change. As its chance of becoming a serious force diminished, it took on the extravagant trappings and sinister purposes of fascist movements elsewhere. By 1934 it was a discredited force. Yet precisely at this moment, the BUF took its final plunge. It launched its anti-Semitic campaign in East London and shifted its admiring sympathies from Rome to Berlin. The "seige of terror" it thereafter visited upon East London—it had little following elsewhere—brought it more publicity than it had attracted in its relatively sedate period. The "Battle of Cable Street" was the climax of the period, representing as it did for both those on the left and the right an opportunity to play out vicariously the deadly drama of the fascist struggle. The result was the passage of the Public Order Act, which, as Benewick shows, deprived the BUF of its blackshirts and checked its capacity for provoking or initiating violence. By 1938, Benewick concludes, it was "little more than an anti-Semitic band" (p. 138).

In an understated conclusion, Benewick suggests that fascism failed because of the strength of British culture and institutions in withstanding the stresses of the 1930s. His argument is, however, so brief and so general that one can hardly disagree with his comments on the civic culture in Britain. Yet given the complexity of any political culture, one's understanding is not substantially advanced by such broad generalizations. Which elements in British culture played the resistant role: the democratic and liberal? the working-class collective? the deferential and conservative? or some particular constellation? While we can know the answer intuitively, Benewick's failure to link his descriptive examination of British fascism more closely to the theoretical questions he correctly poses does not allow us to answer these questions in any depth. This modest reservation notwithstanding, Benewick's study remains a cool and comprehensive account of a troubling phase in a troubled time.

HENRY J. STECK

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Urban Policy-Making: Influences on County Boroughs in England and Wales. By Noel Boaden. (Cambridge: Cambridge University Press, 1971. Pp. xiv, 150. \$7.50.)

At the start of the 1960s the academic study

of British local government was mainly a branch of public administration, concentrating on the formal structure and rarely venturing into either the processes or the outcomes of decision making. Local authorities seemed almost extensions of central departments and in no way political systems in their own right. Theories about local government were based on fragmentary observation and common sense. During the 1960s a great change took place. New provincial universities were established and departments of politics expanded. Staff could now specialize in local government and were eager to explore local authorities close by. The new Social Science Research Council pumped public money into studies of city politics. Above all, British students were inspired by American investigations into community power, their rigorous methodology and sophisticated statistical techniques.

Dr. Noel Boaden of the University of Liverpool represents the new trend. His book is important both for its methodology and substantive conclusions, at least for British scholars, although American political scientists may find it crude. He first demolishes the traditional assertion that strict central control produces uniformity in the provision of local services. Behind the façade of control British cities display variations in their levels of expenditure. About a quarter spend generously on all services, about a quarter spend very little on all, and about a half reveal a complex variety. Local authorities are therefore "authoritative allocators" (p. 19).

He formulates a model of three local features to account for the diversity of levels of expenditure: the incidence of need, the disposition to spend and the availability of resources, each of which he analyses into its components. Need is seen in terms of the social composition of the population and the physical structure of the locality; disposition in terms of party control, professionalization of the permanent officials and public involvement; and resources in terms of money, labor, and land.

To measure the relationship of his variables to expenditure Dr. Boaden examines eight local services which cover 70 per cent of local government spending: education, children, health, welfare, libraries, police, fire, and housing. His methods are the simple and partial correlation coefficient, and he points out that he reveals associations which although suggestive are not explanatory.

His general conclusion is that needs, dispositions, and resources show important, although varying, relationships with the various local ser-

vices. Most significantly he stresses the influence of party control. Many observers have held that party politics has little impact on local government decisions: notably only in the reorganization of secondary education, council house rents, direct labor building departments and patronage. Dr. Boaden shows that the Labour Party, whatever the needs and resources, tends to spend more on education and housing, the most costly services, than the Conservatives. Conservatives tend to spend more on the police, and Labour more on the fire service. Dr. Boaden suggests that Labour reflects a working-class pride in the fire brigade or is more susceptible to pressure from the fire brigade trade unions.

Labour authorities also expedite their business more speedily and have a more elaborate committee structure which enables their members to participate more fully in decision making. Labour authorities trust these committees more, have a lower attendance of the public at council meetings, are less open to the press and are less generous to the other side in the exercise of patronage. Party has been restored as a significant variable.

Dr. Boaden indicates that the poorest authorities spend the most heavily on the most costly services: the poor are the big spenders and big taxpayers. Central grants, most of which go to the poorest authorities, do not wipe out local factors on expenditure levels, and do not seem to restrict local discretion. Central control is not related to the amount of money the center issues.

Dr. Boaden's book is an important prelude for further research. He deals in general totals. We now need to move into a number of local authorities to observe in detail their decision-making processes and outcomes. We must focus on how need is transmitted to the authority, the role of voluntary organizations and the relationships between the elected members and the full-time officials. These areas may not be amenable to treatment by statistical techniques; more traditional approaches are likely to be required. Dr. Boaden has rendered a service by providing through the apparatus of political science systematic evidence for what some students had long suspected.

G. W. JONES

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Zhukov. By Otto Preston Chaney, Jr. (Norman, Oklahoma: University of Oklahoma Press, 1971. Pp. 512. \$9.95.)

Worthwhile biographies of leading Soviet per-

sonalities (with the exceptions of Lenin, Trotsky, Stalin, and perhaps Khrushchev) are rare. Official Soviet biographies are usually of little value or interest except for selected (and sometimes amended) facts pertinent to political or propaganda purposes. There is other source material which varies considerably depending on the subject, but only infrequently is there sufficient relevant information to form the basis for a well-rounded biography.

Colonel Chaney, a US Army officer with a Ph.D. in Soviet military affairs, has written a worthwhile biography of Marshal Zhukov. He had made the most of the available materials that contribute to an understanding of this leading Soviet military figure of the past two generations. He has combed through the growing shelf of Soviet military memoirs, including naturally Marshal Zhukov's own, as well as Western and the few relevant former Soviet sources that throw some light on the Marshal's career. Chaney has a fully adequate understanding of the general framework of Soviet history within which Marshal Zhukov played his roles. This reviewer is aware of no significant sources which the author has neglected. (One of my very few criticisms is directed at the uncharacteristic error made by the author in accepting as valid two allegedly former Soviet sources, "Colonel Kalinov" and "Captain Krylov," whose memoirs are fabrications and who do not in fact exist. These works, however, have only been cited in a few instances on peripheral matters.)

This biography does not contribute any new insights into the Soviet political process or the history of the Soviet Union. But it does illuminate, probably as usefully as any biography of Marshal Zhukov can, many facets of the internal working, and to a lesser extent the foreign relations, of the Stalin and post-Stalin Soviet regimes. It is well written, and through the narrative of Zhukov's personal experiences and activities, we are given a lively and rewarding picture of developments, particularly in the period immediately preceding and during World War II.

Zhukov first received wide recognition as a result of his success in defeating a Japanese incursion into Mongolia in 1939. His role in that battle, and less successfully as Chief of the General Staff in the period shortly before the German attack in 1941, catapulted Zhukov into the first rank of Soviet military leaders. The Marshal's various services as a leading military planner and trouble-shooting field commander during the Soviet-German war (including the battles of Leningrad, Moscow, Stalingrad, and Berlin) made him clearly preeminent among

Soviet military leaders.

The unique relationship which developed during the war between Zhukov and Stalin helps contribute to our understanding of Stalin. So, of course, in a different way, does Stalin's banishment of the well-known Marshal soon after the end of the war. Through no fault of the author, we do not really learn anything new about the postwar Stalin period.

Much more of course can be said about the later era, as seen from the rise and fall of the Marshal in the middle and late 1950s. The author makes use of such information as is available to sketch out Zhukov's political role in the period from 1953 through 1957. The author may in a few instances exaggerate Zhukov's political role in the 1955-57 period; for example, to say that "Zhukov almost certainly issued the orders which sent Soviet tanks into the streets of Budapest in the early hours on October 24," while almost certainly true, is also almost certainly irrelevant to the question of the Marshal's weight in making the Soviet policy decision to intervene; the Minister of Defense, whoever he might have been, would have issued these orders, whether he had been consulted in making the decision or not. But such passages are the exception in Chaney's carefully and generally balanced appreciation of the available evidence on Zhukov as "the soldier-politician."

Zhukov is a well-researched, well-written book. The author is sympathetic to his subject, but not uncritical. It is hard to imagine a more complete treatment of the subject. It not only represents a good and interesting biography of one of the outstanding historical figures of his time, but it also contributes to the reader's understanding of Soviet internal and external affairs in the recent past.

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The Lingering Crisis: A Case Study of the Dominican Republic. Edited by Eugenio Chang-Rodriguez. (New York: Las Americas Publishing Company, 1969. Pp. 178. No price indicated.)

This thin anthology is not a major contribution to our understanding of the Dominican Republic's troubled politics, nor does it significantly add to available analysis of the equally disturbed pattern of Dominican-American relations. Subtitled "a case study of the Dominican Republic," the volume makes little pretense beyond the title at providing material for systematic comparative purposes. Published in 1969, the book was dated even then; most of the arti-

cles brought together in the collection were written (and several originally published) in 1966-67. Even the book's most comprehensive and insightful article, Howard Wiarda's "From Fragmentation to Disintegration: The Social and Political Effects of the Dominican Revolution," written in 1966, makes somewhat frustrating reading in 1973; one would prefer an attempt, such as Wiarda might make in a forthcoming book on Dominican politics, to account more fully for the years of Dominican history since the 1965 intervention.

Besides Wiarda's piece and others by Harry Kantor and Larman Wilson, the book comprises a number of articles of ephemeral worth on the 1966 election. Most are reports by committed social democrats (including the late Norman Thomas) on their work as "observers" to insure that the electoral process would be fair. These articles, however, reveal little beyond the noble intentions of the authors and their consensus that the elections were unrigged in the mechanical sense, even if flawed by the presence of American troops and by the known disinclination of the Dominican Armed Forces to accept Juan Bosch's return to the presidency.

I was struck by the discrepancy between Professor Wilson's suggestion that "unless the United States can help change certain basic aspects of Dominican society . . . the holding of elections will be meaningless—no matter how free, fair, and large the percentage of participation" (p. 125) and the conviction expressed by Norman Thomas, Frances Grant and others that they, as "election observers," were participating in an important and meaningful process. It would have been interesting to include in this collection an analysis of the relevance to Dominican reality of the assumptions the electoral observers made; consideration of the fact that almost every "free and fair" election in Dominican history has been held at the direct instance of the United States government would have made a useful starting point for such an inquiry.

In-depth analysis of the long-term political, economic, and social effects in the Dominican Republic of the 1965 intervention and its aftermath is much needed. Of particular interest, for instance, would be an examination of the US military's renewed attempts to train an elite corps in the Dominican First Army (seemingly repeating the process from which Trujillo emerged in the 1920s) or a study of the various civic development efforts financed since 1965 by USAID in the Dominican Republic. These and other aspects of the Dominican Republic's still "lingering crisis" might provide

material for a very good book, but the Chang-Rodriguez volume is not it.

ABRAHAM F. LOWENTHAL

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The Machine-building Industry in Communist China. By Chu-yuan Cheng. (Chicago: Aldine-Atherton, 1971. Pp. xvi, 339. \$9.75.)

During the past two decades, machine building has expanded from a small prewar base into one of the largest sectors of Chinese industry, a development which has passed largely unnoticed in western analyses of the Chinese economy. Machine building is a difficult topic. In addition to the task of assembling and interpreting fragmentary data which bedevils any economist in the China field, an investigation of the machinery industry must confront further obstacles: an extraordinarily wide variety of products; complex and shifting administrative structures; and the secrecy surrounding defense-related installations. These difficulties, which are responsible for past neglect of this important industry, have been overcome by Professor C. Y. Cheng, whose massively documented monograph provides a welcome addition to existing literature on the Chinese economy.

Cheng's study focuses on the derivation of basic quantitative information concerning Chinese machine building during 1952-66. The most significant contribution is his estimate of the volume, composition, and growth rate of domestic machinery output. For 1952-57, Cheng has painstakingly constructed an output index for 23 civilian machinery products. Although this new index advances less rapidly than the official series for machinery production, nearly all of the difference seems attributable to growing output of new products and to overall improvement in the quality of output. For the period 1957-66, Cheng provides a smaller sample of 11 products which is used to assess the growth of output. The thrust of Cheng's results is to confirm the high growth rate claimed by Peking for machine building during the 1952-57 period and to indicate continued progress, though at a much reduced rate, during the subsequent decade.

In addition to estimating output, Cheng provides quantitative indicators for all significant aspects of the production, trade, and utilization of machinery. Clear presentation of data, sources, and assumptions will permit others to take full advantage of Cheng's command of Chinese materials on his subject.

While Cheng's primary objective is to provide quantitative insights, he does not neglect issues of analysis and interpretation. Two final chapters appraise the overall development of

the machinery industry since 1949 and its contribution to China's major economic goals. During the period reviewed, machine building contributed substantially to China's national security, industrial growth and national self-sufficiency, but only marginally to agricultural transformation. The apparent overexpansion of this industry, in purely economic terms, appears related to its importance as a supplier of military goods.

Although Cheng carefully acknowledges the imprecise nature of his sources and methods and the consequent uncertainty inherent in his results, his estimating procedures seem less than satisfactory at several points. Most important is his failure to explore the quantitative implications of changing product quality. By assuming constant standards of quality, Cheng understates the growth of output during 1952-57 and 1961-66, periods of overall quality improvement, and greatly exaggerates output expansion during 1958-60, when quality deteriorated sharply. This error leads to overstatement of the extent and duration of China's industrial setback during the early 1960s. Less central, but also problematic, are such items as the inconsistency among declining unit input coefficients for labor and steel, improvements in product quality, and falling rates of value added during 1952-57; failure to specify a price base or to account for long gestation periods in estimating capital input; and use of a machine tool price so low that output per worker falls short of annual wages.

Despite these shortcomings, Cheng's monograph adds a new dimension to our knowledge of the Chinese economy, not only because of the volume of research reflected in this account of an important industry, but also because the broad framework of its inquiry provides a significant advance toward a much needed integration of the Chinese economic field with development economics.

THOMAS G. RAWSKI

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The Challenge of Bangla Desh. Edited by Pran Chopra. (New York: Humanities Press, 1971. 159 pages. \$5.00.)

The Rape of Bangla Desh. By Anthony Mascarenhas. (Delhi: Vikas Publications, 1971. 168 pages. £2.00.)

Chopra's book consists of essays contributed by well-known Indian writers, four of them university teachers, two defense analysts, and two journalists. Mascarenhas, also a journalist, received international attention by publishing a report on the Awami League's "mutiny" and

"brutal massacre of thousands of non-Bengali men, women and children" (Sunday Times, May 2, 1971) and then an account of the Pakistan army's "genocide" of the Bengalis in the same paper on June 13, 1971.

The two books differ more in tone and temper than in the substance of their argument. Mascarenhas is jeering, sneering, and shrill in his denunciation of Pakistan's successive governments. Here and there he engages in poetic exaggeration, as, for instance, when he reports that the air at Dacca airport on a certain afternoon was so heavy "you could have sliced it with a knife" (p. 2). These characteristics of style and approach damage his credibility.

Both books are offered as analyses of why East Pakistan had to break away from the West and become independent. It is argued that a common allegiance to Islam was not a sufficient basis for a separate Pakistani national identity; that culturally the two wings were vastly dissimilar; that the East Pakistani elite was persistently excluded from the nation's decision-making processes, which were dominated by West Pakistanis; that a disproportionately high percentage of East Pakistan's foreign exchange earnings and rupee contributions to the central government was diverted toward West Pakistan's economic development and the maintenance of a large military establishment, which also profited West Pakistan; that these political and economic oppressions drove East Pakistanis to seek regional autonomy; and that after the army's use of terror to suppress the autonomy movement, the East Pakistanis fought to be independent.

Students of Pakistani politics are familiar with the main elements of this analysis, which are now conceded by a growing number of Pakistani commentators also. It is therefore in the detail of presentation that one must look for the distinctive contribution of these books. And it is here that a corrective needs to be applied. Space will not permit more than a brief reference to a few cases.

Sisir Gupta makes one of the truly insightful observations in Chopra's book, i.e., that the major cause of East Pakistan's alienation is to be found in the eclipse of politics in Pakistan and, beyond that, in the Pakistani system's failure to produce national, rather than predominantly regional, political parties. Given East Pakistan's underrepresentation in the civil and military bureaucracies, business, and industry, the logic of its numerical majority could not have been asserted, and the wrongs done it righted, except in a democratic system of party politics. But then Gupta and some of his associates, especially Chopra and V. P. Dutt, hold

American military assistance to Pakistan as mainly responsible for this decline of politics and for the rise of bureaucrats and generals to positions of supremacy. The United States, it is said, thought of Pakistan as being conterminous with West Pakistan; East Pakistan never entered its calculations! Beyond considerations of its global anti-Communist strategy, it wanted to use Pakistan to put pressure on India, and to this end, under British tutoring, it sought to maintain an "artificial balance" between Pakistan and India. The United States did not trust Asian democracy and felt more at ease with authoritarian regimes. Both East Pakistan and India suffered as a result of this policy (pp. 5-7, 25, 72, 102-106).

American military assistance did indeed strengthen the Pakistani army and bureaucracy. But surely Americans did not create the political debility of which the bureaucrats took advantage. A few pages earlier in Gupta's essay the reader will find that Ghulam Mohammad had dismissed Nazim-ud-Din from the prime ministership of Pakistan and installed Mohammad Ali Bogra in that office in 1953, and that the regionalization of politics had happened on a grand scale in East Pakistan in 1954, when American military aid had not even begun to arrive (pp. 22-25). The politicians, including the East Pakistani, did nothing to stop Ghulam Mohammad's high-handedness. It might be argued that it was the docility of Pakistan's first two Bengali prime ministers (Nazim and Bogra) rather than American weapons, that ushered in a long period of bureaucratic rule.

West Pakistan's "colonial exploitation" of the East is a recurring theme in both books. But only incidentally is it mentioned that the exploiters were a group or a class that exploited the people of *both* West and East Pakistan (Chopra, p. 37). This heavy stress on the regional character of exploitation serves to disguise its class character, which one of Sheikh Mujib's own observations brings out forcefully:

... when I speak of East Pakistan's wealth being flown to and concentrated in West Pakistan I only mean regional concentration. I do not, thereby, mean that this wealth has reached the masses of West Pakistan. No. I do not and cannot mean that. I know there are millions like us in West Pakistan who also are unfortunate victims of this economic exploitation. I also know that the entire wealth of the country is concentrated in the hands of a few families. This will continue till the capitalistic pattern of our society is changed. (Mascarenhas, Appendix 2, pp. 151-152.)

In this context our authors speak of West Pakistan synonymously with the province of Punjab. The Punjabis are represented as the political, military, bureaucratic, and economic

overlords of Pakistan and as exploiters not only of Bengalis but of Sindhis, Baluchis, and Pathans in West Pakistan. In the higher bureaucracy, the Punjab does indeed have a larger representation than does any other province. But if one wanted to be accurate, one would have to add that only once was a Punjabi the Governor General, never the President, never the Commander-in-Chief of the army, and for less than two years (August 1955-September 1956, December 1957-October 1958) the prime minister. Dewan Berindranath does say that under Yahya Khan hardly one-fourth of the dozen most important military officers were Punjabis and that "out of the 22 top industrial families . . . only two are Punjabis" (Chopra, pp. 146-147). But Dewan Sahib does not bring out these facts to challenge the popular rhetoric about Punjabi domination. He is suggesting that all groups in Pakistan, including the Punjabis, are dissatisfied, so that the worst might be expected even for West Pakistani unity. In time there might be no Pakistan at all!

There is a curious attempt to rewrite history in these books, which was not necessary to establish the case for East Pakistani independence. It is suggested that the Muslim League was never really a party of the Bengali Muslims, that they would have preferred two independent Muslim states as envisaged in the Lahore Resolution, that they did not quite believe in the two-nation theory, and that their leaders were "steam-rollered" into demanding "Jinnah's Pakistan" (Chopra, pp. 17-21; Mascarenhas, pp. 13-14). These extravagant assertions do no credit to their authors, some of whom are reputable scholars. Equally reputable Indian historians will tell us that the Muslim League was established in Dacca at the instigation, among others, of Nawab Salimullah Khan; that right from the days of Ameer Ali, Bengali Muslims had been "building brick by brick a house of communal separatism"; that Hindus and Muslims were farther apart in Bengal than they were, let us say, in Punjab; and that the two-nation theory represented the objective reality in Bengal. (Ram Gopal, *Indian Muslims: A Political History*, Bombay: Asia Publishing House, 1959, pp. 101, 279; Nirad C. Chaudhury, *The Autobiography of an Unknown Indian*, New York: Macmillan, 1951, pp. 225-232, 401-413.) The Muslim League was the ruling party in Bengal at the time of Indian independence. It should be clear that if the Bengali Muslims had wanted to stay out of "Jinnah's Pakistan," they could not have been forced into it. Strangely enough it is also argued in these books that Pakistan would not have come into being without vigorous Bengali

support, that Sheikh Mujib was a patriotic Pakistani and an old Muslim Leaguer, and that East Pakistan and West Bengal would not come together in a United Bengal because the East Pakistani Muslims were not liberating themselves from West Pakistani domination "merely to exchange it for domination by West Bengal Hindus" (Chopra, pp. 4, 77 and Mascarenhas, pp. 17, 137).

In both books the position is taken that since the 1970 election had been contested in East Pakistan on the Six Points platform, these points should have been conceded after the Awami League won a massive electoral victory. The Six Points proposed to entrust defense and foreign affairs (minus foreign economic relations) to a national government which, however, would have no revenue raising power. It would meet its expenses from subventions made to it by the provincial governments, an arrangement not unlike the one America had under the Articles of Confederation. Thus it might fairly be said that the Six Points envisaged a virtual disbanding of the national government. Mohammad Ayoob and others in Chopra's book see Yahya Khan and Bhutto conspiring to thwart Mujib's right to rule from Islamabad. Mujib, it is said, was not willing to share power with Bhutto. When he realized that he would not be allowed to control the national government, his strategy shifted beyond the Six Points toward an even looser confederation (pp. 52-55). But Ayoob does not explain why Mujib or Bhutto should have coveted national office: there would hardly be any power to exercise in a government organized under the Six Points; the provincial governments would be far more powerful.

Many valid reasons for criticizing Yahya Khan can be found. But it seems to me that another man in his place—neither wicked nor grasping nor militaristic—would also have opposed a constitutional design that made the national government thoroughly inefficacious. The historian, I think, will condemn Yahya Khan not for his opposition to the Six Points but for his failure to see that the tide of popular sentiment in East Pakistan had turned irresistibly toward separation. But then the historian will have the advantage of hindsight.

ANWAR H. SYED
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Japanese Blue Collar: The Changing Tradition.

By Robert E. Cole. (Berkeley and Los Angeles: University of California Press, 1971. Pp. xi., 300. \$9.50.)

This welcome study is an investigation of Ja-

pan's factory workers by an American scholar who spent three months working as a machine expeditor at a diecast company in Tokyo with the aim of accepting, "so far as was possible, the same working conditions as other blue collar workers. This meant working a six-day, 51-hour week, asking for no special privileges, being paid a reasonable wage, socializing with the other workers after work, and living in a Japanese-style apartment in a lower middle-class/working-class neighborhood near the factory" (p. 41). He also worked for several weeks at a new suburban plant of the same company, and for one month on the clutch assembly line of an auto parts company outside the metropolitan area. Although this represents a good deal more mobility for Cole than the long-term commitment of the Japanese worker, it still brought him closer to the actual, as opposed to the ideal, worker type described by earlier writers. One welcomes this focus on the workers, even though the "blue collar" title has its problems in a setting in which "the blue collar workers commonly arrived and left the factory in a suit, white shirt, and tie" (p. 144).

The author's concern is not descriptive but analytical, and he uses his experience only to document treatments of government policies, wage rewards, advancement and security, unity and cleavage among workers, and relations with company and labor union. He takes up, and discounts, the "convergence" theory of Clark Kerr's *Industrialism and Industrial Man*, and steers a careful and sensible course between it and the equally unacceptable (to him) emphasis on Japanese uniqueness. His objective is to "provide a bench mark, an empirical graph of the role of tradition in Japan today" (p. 12). One conclusion is that "It is oversimplification to say that pre-industrial societies move from ascription and particularism to achievement and universalism . . . Far more subtle combinations and shifts are possible" (p. 210).

One of the book's merits is the author's reliance on Japanese research. His discussion of the seniority and "commitment" patterns makes clear that this was not fully "traditional," since it developed in the years after World War I in response to management needs and hardened after World War II in response to worker insecurity. Nor, he finds, is it irrational or inefficient. He does find a tension between seniority and ability, with the seniority principle in large firms possible only through its absence in small firms (p. 92); and he finds, predictably, that the recent shortage of labor is increasing the wages of young workers and compressing the wage-rate structure. He also finds a major

discrepancy between the attitudes of workers in Tokyo and those of the younger work force in suburban and rural plants, who are more likely to accept management's goals and assurances. And he finds that "inter-firm mobility of blue-collar workers is increasing faster than that of other occupational groups" (p. 123).

Cole's discussion of the role of the foreman, whose sanctions and responsibilities remind one of the village headman, is of particular interest. His discussion of the labor union confirms earlier treatments of enterprise unions, often co-opted by management, with few professional leaders, and limited grievance machinery. In 1967, in fact, only 37 per cent of all unions had such machinery (p. 230). Union and worker acceptance of goals of production thus comes as no surprise. Neither does the discussion of the "instrumental" use and discharge of personal obligations, unless one comes to the book with the traditional stereotypes in mind.

The author's conclusions from all this are that "the stereotype of the submissive, loyal, and non-competitive Japanese worker has been exaggerated" (p. 277) and that recent "changes are weakening the traditional structure of management authority and creating a more militant worker" (p. 273). Supporters of convergence theory as well as those who hold for Japanese uniqueness will find arguments for their case in this useful book. Nor are they always at opposite poles, for some of this change was predicted by James Abegglen in 1958 when he wrote of the factories he described that "Young Japanese who are urban reared, born in the large cities of laboring and white collar fathers, educated in urban schools beyond the legally prescribed minimum of middle school educations, and steeped in the impersonality of modern cities do not fit well into these factory relationships" (p. 137). That description fits many more Japanese in 1972 than it did in 1958.

MARIUS B. JANSEN

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Weimar Germany's Left-Wing Intellectuals: A Political History of the 'Weltbühne' and Its Circle. By Istvan Deak. (Berkeley and Los Angeles: University of California Press, 1968. Pp. xii, 346. \$9.75.)

The dismissal of rationalist liberalism by early twentieth-century Germany in favor of a return to a romantic ethnic primitivism continues to be of interest to historians and social scientists. There remains an uneasy feeling that the decline of the Weimar Republic has deeper and more universal causes than the temporary aberration of a people unable to make the tran-

sition to modernity. Quite justifiably, the Weimar years, viewed from a more distant historical vantage point, appear as a general human object lesson and a warning. It would indeed be hazardous to confine the search for explanations to the German national character or to traumatic historical experiences peculiar to the German people. It is therefore not surprising that a voluminous literature on this unhappy chapter of European history has come into existence and continues to be enriched by specialized studies. There has been a tendency to analyze events in terms of the ideology and politics of the Right and to dismiss them as pathological outbursts. One may wonder, however, whether a balanced explanation can be obtained without a critical examination of the forces that eventually were subdued. What positive social contributions did the leftist intellectuals make? What were their inadequacies? Why were their ideas incompatible with the German ethos despite the respectable liberal tradition of mid-nineteenth century Germany? Istvan Deak's study of *Die Weltbühne* attempts to provide a better understanding of the leftists' role in the collapse of the Republic.

The author starts out with a general characterization of the writers of *Die Weltbühne* and, in doing so, explains the alienation of the leftist intellectuals from German life. Their aspirations reflected naivete and a lack of realism. They flaunted the traditional values of the bourgeoisie, and yet, being themselves bourgeois in origin and social position, failed to make effective contact with the masses. The "Berlin style" which they represented was the product of non-Berliners and even non-Germans. Their historical heritage as free intellectuals, mostly of Jewish background, left them without a home base. In short, "they were out of place in Germany" both socially and intellectually. These generalizations are substantiated by a chapter containing biographies of the editors of *Die Weltbühne*: Siegfried Jacobsohn, Kurt Tucholsky, and Carl von Ossietzky. The latter, although of non-Jewish background, was because of his leftist intellectualism no less an outsider.

The second part of the book deals with the "Causes and Campaigns" championed by *Die Weltbühne*. The author examines its position during the revolution of 1918-1919; its support of European federation and friendship with France; its sympathy toward Poland and hostility to collaboration with Soviet Russia; its advocacy of a more militant republic than was run by the Social Democrats; and its opposition to the overt and clandestine military forces as well as to the nationalistic judiciary, which

had remained irremovable under the Republic. The main criticism that runs through the account of these campaigns is the absence of a clear political line. Their idealism prevented these ideologically committed intellectuals from placing themselves in any political camp. They remained homeless even on the Left. Their commitment was abstract and kept them free-floating in relation to political organizations.

The main endeavor of *Die Weltbühne*, to which Professor Deak devotes the third part of his work, was the advocacy of socialist unity. He maintains that the efforts of men poorly acquainted with the working class cannot be judged in terms of their failure to produce concrete results, since not even the organized leadership of the working class was able to attain such results. There is no doubt, however, that rationality and consistency of argument, detached from practical politics, could not have a tangible effect upon the course of policy. When during the years of relative stability of the Republic from 1924 to 1927 *Die Weltbühne* became increasingly radical, it urged socialist-communist cooperation, condemning both parties for their lack of realism. But the purism generated by intellectual abstractions was wholly unrelated to the experience and problems of the workers' parties. Even when, during the final struggle of the Republic against the fascist menace, *Die Weltbühne* increasingly identified with the Communists, it persisted in its severe criticism of the Party leadership for not living up to the program. Its main target, however, was Brüning and his government. This misdirection of its polemics had its basis in the rationalist fallacy which tended to dismiss the Nazis as psychopaths incapable of securing sustained popular support. In this view, the high quality of German education would serve as a reliable check on the Nazi madness. Part IV describes the demise of *Die Weltbühne* at the hands of the victorious Nazis and explains the attractiveness of the Nazi movement for the average German. The author does not accept the frequently voiced charges that the left-wing intellectuals contributed to the downfall of the Republic through their failure to align themselves with one of the leftist republican movements. Their literary activities remained without political effect partly because of the many inconsistencies of their intellectual position and partly because of their political isolation from both the governing forces and the workers' and peasants' opposition.

The study is competently done: the ample documentation, comprehensive bibliography, and almost sixty pages of concise biographies of

the "*Weltbühne* Circle" and "Some Friends and Enemies of the *Weltbühne*" provide much valuable information on the period. The author might perhaps have added a further dimension to his analysis by illustrating through the case of the *Weltbühne* the effect of political commitment upon the intellectual's role in society. Does not his commitment, one may wonder, turn him away from an honest striving for objective analysis and thereby undermine the very rationalism which provides his only *raison d'être*? Is not the atomism inherent in the liberal intellectual's rationalism at odds with man's craving for community life and therefore doomed to failure as a political force? Must not the analytic function of the intellectual, to be of any value, be differentiated from the activism of the man of affairs? To raise these questions is not to detract from the merit of the book. It is a valuable contribution to the history of Weimar Germany.

PETER J. FLIESS

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The Prussian Welfare State Before 1740. By Reinhold August Dorwart. (Cambridge, Mass.: Harvard University Press, 1971. Pp. 328. \$12.50.)

This book reminds us that the idea of the welfare state is very old indeed, dating back to the Late Medieval communes. Under the Hohenzollerns, as in other Early Modern monarchies, legislation affecting the morality, health, education, and general protection of the population was enacted on the territorial level, so that by 1740 there existed on paper a welfare plan whose declaration of purposes is in most respects comparable to that of modern nation states. The evolution of ordinances, edicts, and administrative organization is minutely spelled out in Professor Dorwart's study, providing ample evidence of the paternalistic concern of the Hohenzollern monarchs and the comprehensive nature of their policies even before the middle of the eighteenth century.

Political scientists interested in comparing institutions will find in this book a wealth of detailed information. Dorwart also provides an adequate survey of the idea of welfare as it emerged in the seventeenth and eighteenth centuries. But despite the attention to legislative detail, his work tells us little about the substance of the Prussian welfare system, i.e. whether or not it actually met the individual and general needs of the population. Thus, the possibilities of comparison with other functioning welfare systems is limited. The book relies entirely on legal codes and published admin-

istrative orders, making little or no attempt to separate result from intent. The detailed description of institutional change is not integrated with social, economic, or demographic history in such a way as to tell us much of what we would like to know about the impact of particular measures. From what little can be surmised from the repetition of ordinances, it would seem that seventeenth- and eighteenth-century welfare schemes were ineffective in important respects, meeting neither individual needs nor, ultimately, the demands of the state itself. As the contradictions between economic growth and political development lie beyond the scope of this monograph, we learn little about why this should have been so. This is the inevitable result of isolating legal and administrative history from the broader trends of the Early Modern period.

The underlying causes of the evolution of welfare policy, treated here largely as the function of the history of ideas, are not easily fathomed through the kind of documents on which Professor Dorwart relies. The "politics" of welfare, as it was played by the absolute monarchs of Europe, deserves more careful attention. Dorwart himself stresses the *ad hoc* nature of governmental responses to economic and social crises, but he pays relatively little attention to the role of pressure groups, the saliency of status conflicts, and the perpetual fear of popular unrest in the decisions of monarchs and bureaucrats. He seems to be led more by the nature of his evidence than by the substance of social and political history to conclude that change was due mainly to princely paternalism and, after 1740, to *raison d'état*. These explain little about the actual distribution of welfare and how it was manipulated by the state and its clients to serve their special interests. For the most part, welfare measures in Prussia served conservative purposes, respecting the boundaries of corporate society, always sensitive to the interests of the higher status groups. Plans that directly affected the life of the poor or the outcaste inevitably involved ulterior motives reflecting the tensions inherent in that highly stratified society. The prince was by no means as free to concern himself with the well-being of his people as the author would make out. In an era before the professionalization of the civil services, his agents were themselves a corporate interest group.

Little of the complexity of the relations between state and society comes through in this work. The fault is largely that of German legal and administrative historical writing, which for so long has assumed the autonomy of the state.

Even the published documents on which Professor Dorwart has relied reflect this bias, making it impossible without fresh archival material to push the frontiers of history much beyond their traditional limits. As intellectual and institutional history, Dorwart's work measures up to the best produced by German scholars. But by making their method his own, the author falls short of his own goals of writing about the substance of the Early Modern welfare state.

JOHN R. GILLIS

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Teachers as Agents of National Development: A Case Study of Uganda. By David R. Evans. (New York: Praeger Publishers, 1971. Pp. 247. \$15.00.)

Uganda, like many African states, makes an enormous resource commitment to the development of education—in the Ugandan instance, averaging about 25 per cent of the recurrent budget in the early years of independence. David Evans assesses one aspect of the return to society for its investment in education in *Teachers as Agents of National Development*. One of the major tasks of the educational system in a polity committed to development and change is to create in its new generation positive orientations toward nation and modernity. Evans focuses upon the secondary school system in Uganda, and seeks to understand its impact on the political socialization process. On the whole, Evans finds the performance deficient. Certainly his data would be of little support to a Minister of Education in defending his pleas for more funds for secondary education to the stone-faced inquisitors of the Ministry of Finance at budget time.

The book, apparently based on Mr. Evans's doctoral dissertation at Stanford University, is built around a survey of secondary school teachers. Evans used a random sample of secondary schools stratified by region, applying the survey to all teachers in the schools selected. One should note a major limitation on the scope of the study deriving from the restrictive conditions on which government clearance was obtained. Evans was refused permission to interview any pupils; thus the inferences on the political socialization impact of the schools derives entirely on what the author learned from the teachers. This is obviously quite unsatisfactory, but the author cannot be faulted for circumstances over which he had no control. Compounding this problem was a requirement that 16 questions exploring teacher attitudes toward the government be removed from the survey instrument. But despite this dilution of his

data, Evans deserves credit for persevering; a great deal of interesting information was collected.

Uganda offers a particularly interesting field for such a study; although expectations as to what the educational system can achieve for state and nation have increased, the short-term impact of independence is the de-Africanization of its secondary teaching cadre. At the time of the study, only 15 per cent of the secondary teaching staff was African; 15 per cent were Peace Corps and other overseas volunteers; 23 per cent were Asian, many Ugandan citizens; 10 per cent were missionaries, many with long teaching service; 32 per cent were overseas, mainly British, recruited on contract arrangements; the balance, about 5 per cent, were primarily wives of British officials, serving on local terms. The extraordinary difficulties in forging an efficient instrument of planned political socialization out of this incredible variety of teachers needs no further elaboration.

Much of the study is devoted to a delineation of the differences in attitude uncovered among these different groups of teachers. Africans and the overseas volunteers appeared overall to be most receptive to the socialization role defined by the author as functional to development and nation building. The missionaries and British wives, for different reasons, occupied the bottom rungs of his table, for which he used such criteria as awareness and attitude toward government, student participation, awareness of social environment and community contact, and attitude toward educational innovation and greater curricular relevance. One might note in passing that not all of the assumptions on which the scales were constructed were convincing: It is not self-evident that respondents' perceptions of Ministry of Education receptivity to their views betokens a positive image of government, nor that this dimension can be tapped by the number of hours spent listening to Radio Uganda (pp. 71-72). Nor does ability to name ministers plausibly measure "awareness of government" (p. 79), in circumstances where several of the groups analyzed are only short-term residents, with a high turnover rate.

Evans notes that political socialization was not a high priority of the government. Had his focus been broader, the reasons for this would have seemed evident enough. The sheer burden of sustaining the expansion of the school system, in response to the enormous popular pressures for its enlargement, creates problems of such magnitude that more diffuse goals such as improving the socialization performance will

necessarily be shunted aside. For example, it is the pace of expansion which will force reliance for the foreseeable future on a huge cadre of foreign teachers, whose responsiveness to a government-directed socialization program is obviously in doubt. Add to this the lack of elite consensus as to what precisely the Ugandan "political culture" should be, and the problem is more intractable yet. The one nation-building task on which substantial headway does appear to have been made receives little notice in the Evans work—the attenuation of the level of religious conflict.

Thus, in sum, we have a somewhat disappointing study. It does present some quite useful information, and provides us with a well-executed survey of a crucial occupational group. Yet, partly because of restrictions on scope imposed on the author, but also partly because of self-imposed limitations, the volume falls short of presenting us with an integrated analysis of the contribution of the secondary school system, actual and potential, to the goals of nation building and modernization in Uganda.

M. CRAWFORD YOUNG

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A History of the People's Democracies. By Francois Fejto. (New York: Praeger Publishers, 1971. Pp. 374. \$13.50.)

We have long been needing a comprehensive text dealing authoritatively with political developments in Eastern Europe since the Stalinist period. This deficiency is now largely remedied by Francois Fejto's book, *A History of the People's Democracies: Eastern Europe Since Stalin*, translated from the French and with an epilogue covering events since the book's first appearance in 1969.

Fejto's account begins with the New Course and de-Stalinization of the mid-1950s. The bulk of the book and by far its most interesting part deals with domestic political developments in Eastern Europe. There are also useful, if more perfunctory chapters covering social structures, the economy, cultural trends and other aspects of the development of the Eastern European states. Relations among the nations of the bloc, and with the Soviet Union, form an integral part of Fejto's treatment but are utilized mainly to explain the course of domestic political developments. Thus the volume serves as a useful complement to Brzezinski's work in the area of foreign relations, *The Soviet Bloc*. The value of Fejto's account is greatly enhanced by the excellent chapters on Yugoslavia, a country which is often ignored in other accounts of domestic developments in Eastern Europe.

Fejto's outstanding achievement lies in presenting a highly readable account of the contemporary history of Eastern Europe without sacrificing any of the complex details which are indispensable for understanding the nature of politics in the region. A sense of the unpredictability and intricacy of the events which shaped Eastern European politics is sustained by careful attention to the power struggles which determined developments in each of the Communist countries. At the same time the account returns continually to the broader social, economic, and political problems facing the governments of Eastern Europe during this period. These observations rarely fail to hit the mark, and show a truly extraordinary feeling for the conflicting forces which have shaped the development of the Eastern European states over the past two decades.

Fejto, who left Hungary in 1951 and subsequently served as a correspondent for *Agence France Presse*, does not conceal his feelings about the Communist regimes and the role of the Soviet Union in influencing their domestic policies. He convincingly demonstrates that Eastern Europe's destiny is still governed by the actions of the Soviet Union. Even in the case of Yugoslavia, Fejto is persuasive in connecting the struggle between the conservatives and the liberals in the mid-1960s with the outcome of the power struggle within the Soviet Union.

Of course, placing the Soviet Union in the center of events suggests that it is the Soviet Union, not Eastern Europe, that must be studied if the future of the region as a whole is to be understood. This implication emerges at the close of the book when Fejto, in analyzing the failure of the Communist East European regimes to liberalize in the face of Soviet opposition, is left with only one ray of hope, "that the next Dubcek will appear in the nerve center of the system: Moscow" (p. 317). Fejto's concern with Soviet refusal to allow change in Eastern Europe is nevertheless balanced by a recognition that the forces for change in the region are powerful and growing stronger. Notwithstanding references to the need for a Dubcek in Moscow, there is the implication that the slightest show of indecision on the part of Moscow could lead to new demands for reform in Eastern Europe.

The major problem with Fejto's volume is a lack of documentation. The work is based largely on newspaper accounts, Western journals, and the author's own great knowledge of the area. While the American reader will find sources of interest in the book (especially cer-

tain publications, such as *Documentation française*, better known in Europe than in the United States), the material on which Fejto bases his account is not always adequately revealed. A case in point is his dramatic description of the events taking place in the power struggle in Yugoslavia during 1963 to 1966. In this instance, as in certain others, Fejto appears to be relying on accounts provided by the victors in the power struggle, persons who have had their own self-interest very much in mind in describing what took place.

The Fejto volume is nevertheless distinguished by its outstanding grasp of Eastern European history and detailed knowledge of the events of the past two decades. It can serve as a comprehensive introductory text for serious students interested in the politics of Eastern Europe, and as a source of much new information on Eastern Europe for the scholar familiar with the area.

PAUL SHOUP

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Power in Africa. By Ruth First. (New York: Pantheon Books, 1970. Pp. xiv, 513. \$10.00.)

Ruth First is a rather remarkable journalist, born in South Africa and now in exile, who writes with commitment and perspicacity about African affairs. Up to now she has written about southern Africa. This time she takes on the topic of coups d'état in independent Africa—why and how they have occurred, and what is their political significance.

She seems to have read, and read well, virtually all that the scholars have been writing on contemporary Africa. And as a good journalist, she has tracked down the details on the three countries about which the bulk of the book is concerned: Sudan, Nigeria, and Ghana.

Although several others have written about these coups, none has I believe sought to place them within a general perspective of the whole process of decolonization. The book is a critique of these coups from the left, but one that happily rejects a conspiracy explanation as a substitute for analysis: "The basic structures of African society in new states, Ghana and Mali included, hold the seeds of a coup d'état within themselves. It is precisely because foreign powers and bodies like the CIA understand this well, that their interventions, even very indirect ones, are so effective" (p. 414).

What then are the "deeper reasons" which Ruth First adduces as explanations? There is first of all, she argues, the fact that the colonial experience had a greater impact than many will admit. "For the sediment of colonization lies

deep in African society" (p. 27). Basically she sees the military-bureaucratic regimes as "a very close copy" (p. 432) of colonial government. Instead of Lugard and Lyautey, Africa now has Mobutu, Gowon, and Bokassa.

But why? Was there not a nationalist revolution against colonial government? Ruth First admits that there was. But the rapidity of the decolonization process, which the colonial powers precipitated to prevent radicalization, "if it made the early 1960's heady with optimism, left behind a damaging legacy of myth and illusion" (p. 49). And the military regimes continue to breed myths and illusions. They claim they come to power to overcome the dangerous internal divisions bred by the politicians. But First sees no significant difference among politicians, bureaucrats, or army officers in terms of their policy objectives: "stability and order, national unity and rapid modernization. Politicians . . . failed to reach these goals; bureaucracy and army failed even more abysmally" (p. 362).

And yet, she ends with empathetic warmth and understanding:

The government of Africa, in the hands of the politician-manipulators, or the less flamboyant but infinitely more parochial soldier-rulers, is not on the whole tyrannical, but bumbling. Time and again it makes false starts, and spreads false hopes. Condemnation there must be; but compassion, too, for those who talked so boldly about freedom but had so little freedom of manoeuvre" (p. 465).

This book is an excellent introduction for students to the complexity of post-independence politics. Its approach is far wider than its topic would suggest. For researchers working on the details of the coups in the three countries treated at length, there is no doubt material here worth culling. The tone is in general very balanced even if, as in my case, one does not share every bias First has. It is at least a book that gives the reader a sense of the concrete reality of African contemporary life, which cannot always be said about some of the pseudoanalytic works that engulf us.

IMMANUEL WALLERSTEIN

McGill University

The Commissariat of Enlightenment: Soviet Organization of Education and the Arts under Lunacharsky. By Sheila Fitzpatrick. (Cambridge: The University Press, 1970. Pp. 380. \$13.50.)

This is a fascinating account of the confrontation of revolutionary romanticism with post-revolutionary political and economic realities. Through a detailed study of the creation and

inner workings of a single Soviet ministry, rich in historical detail and based in part upon hitherto inaccessible Soviet archives, the author illuminates the problems faced by the Bolshevik regime in its early years as it attempted to translate ideals into practice.

The Commissariat of Enlightenment was assigned the task of reorganizing both education and the arts to serve the needs of the nascent socialist society. Under the leadership of Anatoly Lunacharsky, the philosopher-poet and revolutionary enthusiast who described himself as "an intellectual among Bolsheviks and a Bolshevik among intellectuals," and of Lenin's wife Krupskaya, who devoted a lifetime to the cause of popular education, the Commissariat played a central role in mediating the relationship of the new regime to the Russian intelligentsia.

The book documents the enormous problems faced by the Communist Party in making the transition from political opposition to governmental leadership, and above all the severe shortage of experienced personnel and administrative skills. It describes the organizational struggles which masked conflicts over power and policy: the efforts of Narkompros to wrest control over technical and professional education from the economic ministries and over political education from Party agencies, and at the same time to resist encroachment by other Party and governmental agencies on its educational and cultural domains. The economic dislocation of the times created additional preoccupations: discussions about the procurement of firewood, food rations, and general supplies vie with more abstract organizational and educational controversies in the minutes of the Narkompros collegium.

But the study is more than an institutional history. It is at the same time an account of the efforts of the Commissariat to draw the prerevolutionary cultural elite into contact, if not cooperation, with the new regime, and into sympathy with its broad objectives, while simultaneously defending itself from antagonists within the Communist Party who viewed Narkompros as politically unorthodox, administratively incompetent, and, in the turmoil of civil war, expensively irrelevant.

In this effort Lunacharsky and Krupskaya had the full support of Lenin himself, whom the author places squarely in the camp of the old fashioned revolutionary enlighteners, defending cultural traditionalism and the value of a broad general education against a newer generation of Party militants more narrowly utilitarian in its concerns, more "military" in its

methods. Apart from one disagreement over the autonomy of Proletcult which reflected important differences about the relationship of bourgeois to proletarian culture, there were no serious conflicts of principle between Lenin and the Narkompros leaders. Lenin was more deeply concerned than they with creating an orderly administrative structure out of the anarchy which constantly threatened to engulf the Commissariat. But he consistently sided with Narkompros when it was attacked, defending its policies against pressures brought by the technical lobby which sought to replace broad general education with early specialization and to subordinate educational policy to the immediate tasks of economic reconstruction.

Other studies have documented the progressive defeat of early revolutionary ideals both in education and in other realms. The failure of Narkompros to enlist popular participation and local initiative in education through a system of educational soviets and the restoration of centralized bureaucratic control brought to nought the efforts of a generation of educational reformers, including Krupskaya herself. The partial abandonment of genuinely polytechnical education and the concessions to early specialization diluted earlier commitments. The consequences of the New Economic Policy were particularly serious for Narkompros, for the general retrenchment in governmental activity entailed the withdrawal of central financial responsibility for the educational budget, and therefore a loss of control over the implementation of a uniform educational program. The loss of central subsidies also resulted in the reintroduction of fees at all levels of the educational system, a defeat, however temporary, for efforts to achieve free and compulsory universal education.

While the author chronicles these defeats in some detail, she also draws attention to small triumphs. In the midst of political chaos and general poverty, she argues, it was no small achievement to have maintained a policy of tolerance in the arts—defending the cultural heritage of the past along with opportunities for the vociferous avant-garde—nor to have respected the independence of science, nor to have defended the principle of universal general education despite concessions in practice. State subsidies for the arts and sciences necessarily entailed state supervision, for the central problem was patronage rather than freedom. But Lunacharsky, and Lenin with him, are credited with resisting pressures to use this power to interfere or to intimidate.

This balance sheet of achievements and dis-

appointments implicitly suggests that the general economic situation and the personal roles and values of key figures are the decisive explanatory factors. It reflects the absence of a broader analytical framework for examining the interaction of conflicting pressures and priorities in the context of the developing decision-making institutions, and for evaluating the outcomes from this perspective. The study is nonetheless a most welcome contribution to the literature of the early Soviet period, and offers rich materials for more theoretical and comparative analyses of social and political change.

GAIL LAPIDUS

University of California, Berkeley

India's Green Revolution: Economic Gains and Political Costs. Francine R. Frankel. (Princeton: Princeton University Press, 1971. Pp. 232. \$7.50.)

In this volume, Francine Frankel reports on her research into the social, economic, and political implications of agricultural change in five widely dispersed Indian districts: Ludhiana in Punjab, West Godavari in Andhra Pradesh, Thanjavur in Tamil Nadu, Palghat in Kerala, and Burdwan in West Bengal. These districts were chosen from among the 15 districts selected in 1961 for special attention under the Intensive Agricultural Development Program (IADP). Since that time they have remained major foci of administrative energy in the propagation of the Green Revolution.

Professor Frankel's argument has two central components: First, she suggests that the economic gains deriving from the technological revolution in agriculture have been distributed very inequitably, with a greatly disproportionate share going to large landowners, leaving the landless and small holders with little gains and indeed, with a substantial decline in economic well-being relative to their neighbors. Second, she argues that growing income disparities contribute to increasing antagonism and the breakdown of permanent bonds between land-owning patrons and their clients, leading, in the presence of Communist parties, to increasing political radicalism, social disorder and violence.

The majority of Professor Frankel's book is given over to the first argument, and here she presents a wealth of valuable comparative evidence from the five districts and makes a thoroughly convincing case. Each of the five district chapters contains an extensive analysis of the characteristics of the new seed varieties being used and of their relationships to the district's farm ecology, land ownership patterns, and

prevailing tenancy and labor practices. In each case, she shows why agriculturalists on each descending rung of the land-based village economic ladder are able to derive proportionately less benefit from the innovations.

The second argument is much more central to the concerns of political scientists. Evidence of change in the level of social disorder, violence, and radicalism in specific districts over time is, however, difficult to come by. Moreover, in periods of pervasive social change it is especially hard to isolate the effect of agricultural change even where an increase in political radicalism can be documented.

The unusual characteristics of Professor Frankel's sample of districts compound the problem in this case. Districts were picked for inclusion in the IADP back in 1961 because of their unusual economic promise. In the subsample of districts chosen for this study, at least, these economic characteristics are accompanied by equally atypical social and political patterns.

Professor Frankel's four rice-growing districts, for example, have population densities that average 878 persons per square mile in contrast to the all-India mean of 348. The number of cultivators per 100 acres averages 58 as opposed to a mean of 38 in India as a whole. Even more striking, the percentage of agricultural laborers in these four districts averages 51.8, in sharp contrast to the all-India mean of 19.3.

It is precisely these characteristics of high rural population density in combination with extensive landlessness that Donald Zagoria, in "The Ecology of Peasant Communism in India" (*APSR*, 65 [March 1971], 144-60), has convincingly identified as being consistently associated with high levels of Communist Party voting. Thus it is not surprising to find that, in the early 1960s before the Green Revolution began, all four of Professor Frankel's rice districts were in the top 15 per cent of 305 Indian districts ranked by Communist Party of India (CPI) vote in Legislative Assembly Elections in 1960 (for Palghat) and 1962 (for the other four districts). Palghat ranked first with 53.8 per cent, Burdwan fourteenth with 32.3 per cent, West Godavari twentieth with 25.3 per cent and Thanjavur forty-fifth with 15.9 per cent. The only district from this sample in which the Communists had not done well was the wheat-growing district of Ludhiana where the CPI won 5.6 per cent of the vote in 1962. This district has relatively low pressure on the land (20 cultivators per hundred acres) and a

relatively low percentage of agricultural laborers.

Professor Frankel relies heavily on high levels of 1967 and 1969 voter support for Communist parties in her districts as evidence of the radicalizing effect of the Green Revolution. But when one looks back to the pre-Green Revolution voting figures of 1960 and 1962, it turns out that in four of the five districts, these voting levels represent significant declines in Communist strength from the levels of the early 1960s. Only in Burdwan has Communist voting strength increased, and there the agricultural growth rate has been very low. Thus, despite evidence of radicalism and political violence in the districts, these data do not support the case for a causal relationship between rapid agricultural growth and the rise of radical and violent agrarian politics.

Problems with the adequacy of her evidence notwithstanding, Professor Frankel presents a logically and intuitively compelling hypothesis: that the nature of traditional landlord-tenant relationships is being eroded by the modernization and monetization of agriculture and by the exposure of peasants to arguments for the radical restructuring of the rural economic and political order. Her supporting evidence on the distributive impact of the Green Revolution is rich in its detail and its comparative coverage of the five districts. It is perhaps inevitable, given the scope of this effort, the nature of the sample of districts, and the attention to considerations of distributive economics, that the evidence on political dynamics associated with the Green Revolution should only be suggestive. Nevertheless, Professor Frankel's book does place in clear relief a range of issues which will require the continuing attention of political scientists and political anthropologists interested in the process of change in rural India.

STANLEY J. HEGINBOTHAM

Columbia University

La Pyramide de Babel. By Maurice Giroux. (Montréal: Les Editions Sainte-Marie. 1967. Pp. 140. \$2.50.)

La Législation Electorale au Québec. By Denis Laforte and André Bernard. (Montréal: Les Editions Sainte-Marie, 1969. Pp. 197. \$2.50.)

Réflexions sur la Politique au Québec. Second edition. Edited by André Bernard. (Montreal: Les Presses de l'Université du Québec, 1970. Pp. 100. No price listed.)

La Jeunesse du Québec en Révolution. By Jacques Lazure. (Montréal: Les Presses de l'Université du Québec, 1970. Pp. 141. No price listed.)

The death of the autocratic premier of Quebec, Maurice Duplessis, in 1959 ended a long period of political corruption and decay, negative nationalism, and opposition to political and economic reform in the province. The floodgates were opened to a veritable tidal wave of new ideas and programs reflecting both a positive assertion of the Québécois personality in its relations with the rest of Canada and with other countries, and a genuine dedication to internal socioeconomic reform and democratization. These changes, which collectively have come to be known as the Quiet Revolution, also brought with them, not unexpectedly, great internal divisions and social tensions leading to the emergence of a strong independence movement. The principal democratic and electoral expression of this movement is the Parti Québécois which won 23 per cent of the popular vote in the last provincial election in 1970; it is widely recognized today as the major opposition party. There is also a small but determined group of *indépendantistes* committed to violent revolution, known as the FLQ, whose tactics, including political kidnapping and murder, triggered the October Crisis in 1970 and the temporary suspension of the civil liberties of Canadians.

The four books reviewed here address themselves directly to these developments. They also reflect the gradual evolution in the political thought of most of Quebec's intellectuals and academicians during this period. Giroux's essay, published in the Canadian centennial year of 1967, typifies an earlier concern with achieving more genuine political equality for French Canadians through reform of federal governmental institutions. Laforte and Bernard's historical description of electoral legislation in Quebec and the more speculative *Réflexions sur la Politique au Québec*, which appeared in 1969 and 1970 respectively, manifest a later interest in the democratization and reform of political institutions in Quebec itself. Lazure's social-psychological analysis of contemporary Quebec youth, also published in 1970, is characteristic of the more recent preoccupation with the forces and ideas of independence.

La Pyramide de Babel metaphorically describes the structure of the present Canadian polity. A central feature is the party system, which fails to encompass the social reality of a highly regionalized and fragmented society

made up of diverse ethnic-linguistic groupings; rather, it is a top-heavy national organization set on a base which is regional and limited in scope. Giroux suggests that the Confederation structure resembles the Katimavik of Expo '67, an architectural monstrosity in the shape of an inverted pyramid. The central question which he poses is: How does such a structure survive?

Giroux proceeds to a critical examination of Canadian Confederation from the standpoint of French-Canadian aspirations. Wisely rejecting the tendency among so many of his Quebec contemporaries to focus on Canadian legal-constitutional shortcomings, he concentrates instead on the electoral system (for him, the key to structural reform), the party system, and executive-legislative relations. He analyzes, as a comparative illustration of the primary importance of these variables, the system which operated during the Union Period in Canadian history from 1840 to 1867. The designers of its constitution created a unified structure for the Canadian provinces in an effort to assimilate French Canadians into the English-speaking milieu. The French Canadians thwarted this objective by turning the electoral and party system to their advantage. Thus they succeeded in transforming the original legal-political structure into a quasi-federal system in which genuine cultural dualism and equality operated in the legislative, executive and administrative spheres. Giroux proposes an analogous reform for the contemporary period (1945-67), including the adoption of proportional representation, the fostering of a decentralized multiparty system in which major parties would be truly national in scope, and the election of a president by universal suffrage. He fails to explain, however, why French Canadians would be more likely to achieve political equality and cultural survival within such a system. He also does not come to grips with some of the obvious shortcomings of multiparty systems in presidential regimes (here Latin American experience might be pertinent). Moreover, it is rather doubtful that Canadians as a whole would ever agree to abandon so completely their British parliamentary heritage.

The books on institutional reform in Quebec are less ambitious in their proposals for political change and also less controversial. Laforte and Bernard painstakingly trace the evolution of electoral laws in Quebec from 1791 until 1967, showing that in the nineteenth century Quebec moved even more rapidly toward universal suffrage than did Britain during the same period. By the end of the nineteenth century,

Quebec had legislated most of the present-day regulations ensuring freedom of voter choice, wide eligibility for candidacy, equal representation of constituencies in proportion to population, and limitations on electoral malpractices and frauds. In the twentieth century, however, during the "long" regimes of Gouin, Taschereau and Duplessis, this progress was slowed (e.g., in women's suffrage, redistribution) and even reversed (e.g., in government election subsidies). Since the beginning of the Quiet Revolution, the movement for electoral reform has once again accelerated, culminating in recent demands for a complete reassessment of the relative merits of the single-member plurality electoral system itself in comparison to other systems such as proportional representation. The same reform-mindedness seems to have motivated the authors of the *Réflexions*. They advocate important changes in Quebec's lower house, the Legislative (now National) Assembly, such as the elimination of much of the traditional and outmoded parliamentary ceremony, the establishment of fixed parliamentary sessions, a drastic limitation on debating time and parliamentary questions, greater use of committees, election of a permanent Speaker, and more closely regulated control of public funds. They also call for the abolition of the upper house, the Legislative Council. Several of these proposals, including the termination of the Legislative Council, have since been implemented. A major area of as yet unfulfilled reform, as one contributor astutely points out, is the regulation of pressure groups, since the present system allows disproportionate influence to groups representing capitalist and business interests. Other more sweeping proposals for institutional change, such as the adoption of a presidential-congressional system or some hybrid of the presidential and parliamentary systems like that of Fifth Republic France, are unfortunately not explored in this volume, although they are presently provoking considerable debate among Quebec intellectuals and political activists.

None of these proposals, however, meets the requirements set by the ideology of total liberation which now propels so many of Quebec's intellectuals, professionals, and above all, its youth. Former French President de Gaulle is acknowledged to have given the movement an important psychological impetus in his appeal "Vive le Québec . . . libre!" in 1967. By 1970, if Lazure is to be believed, the independence ideology had already seeped deep into the consciousness of the vast majority of Quebec youth, so that this group no longer debates

whether independence will come, but rather when and in what form. Lazure uses a Freudian analytic framework to encompass the totality of this consciousness on the part of revolutionary youth. The ideology of sociopolitical revolution, which operates at the level of the superego, has substituted for the earlier ideology of national survival implanted by the Roman Catholic Church. It assumes any of three forms: revolutionary activism, libertarian anarchism, or pragmatic reformism. This ideology in turn shapes the attitudes of youth toward academic revolution and changes in modes of learning, which operate at the level of the ego, and their revolutionary sexual mores and practices, which operate at the level of the id. Although all three levels of consciousness are closely related and affect each other, it is the sociopolitical revolution at the level of the superego which is dominant. It is defined primarily in political rather than in economic terms, and is closely linked to Freudian notions of liberation from an Oedipal dependence on the mother. Lazure argues that the revolution at all three levels together constitutes a cultural revolution which rests on a strictly political base, and is unlike youth revolutions in other societies in North America or Europe. Its two distinctive features are its political rather than its anthropological nature and its search for a new collective identity for Quebec rather than for individuality.

Lazure acknowledges an obvious weakness in his analysis; it is largely hypothetical and exploratory, and needs much more verification. He promises to carry out this empirical investigation in subsequent studies. One may question, moreover, the necessity of using a Freudian framework to integrate the numerous ideas which are so imaginatively developed. The notion of a collective superego, ego and id, although recently developed by social psychologists such as Erich Fromm, is difficult to accept in other than purely symbolic terms. Lazure argues that the dominance of the sociopolitical revolutionary ideology in the consciousness of Quebec youth can be linked to continued repression of free sexual behavior carried over from an earlier period when the Roman Catholic Church sternly regulated these norms; in other words, libidinous behavior is sublimated to the superego. This seems to me to be a rather crude translation of individual Freudian psychology into collective terms.

Nevertheless, his study does pose the obvious question which all observers of Quebec politics are now asking: Is political independence the logical and inevitable culmination of the rev-

olution of political renewal which began in 1960? Lazure himself obviously believes so, since he describes this process as "ineluctable as a sun ray which signals a new spring" (p. 141). Time alone will reveal whether this present optimism concerning the coming revolution is justified.

MICHAEL B. STEIN

McGill University

Yamagata Aritomo in the Rise of Modern Japan, 1838-1922. By Roger F. Hackett. (Cambridge, Mass.: Harvard University Press, 1971. Pp. ix, 377. \$12.00.)

Western writers frequently have characterized Yamagata Aritomo as the father of Japanese militarism. Now, in the work under review, Professor Hackett has made the most effective refutation yet of this bit of political mythology. Yamagata was, of course, the creator of the modern Japanese army; and he was preoccupied with protecting Japan's national security. But he was also an essentially cautious and moderate man who keenly sensed the limits of national power. His "determination to enhance the position of the nation was fused with caution in the expansion of Japan abroad," (p. 348) as the author puts it. We learn in these pages that Army Minister Yamagata opposed early military adventures into Korea and Taiwan in 1873 and 1874; that, while he was a sponsor of the Sino-Japanese and Russo-Japanese wars, both times he traveled to the battlefield to induce reluctant commanders to accept unpopular peace arrangements; and that, indeed, much later, he held back fire-breathing foreign ministers in the era of the Twenty-One Demands and the Siberian Intervention. His political realism in matters of foreign relations, if perpetuated, might have spared Japan the disgrace of the Pacific War.

The fault lay less in the modern military establishment which he fathered than in the shape he gave to the political institutions that might have controlled it. His military reforms, synthesizing foreign and native elements, were a model of their kind. As soldiers he preferred pliable commoners to the tradition-ridden samurai; and through promotion of the Conscription Ordinance of 1873 he broke the hereditary samurai monopoly on arms. The conscripts were organized under a German-style command structure with foreign weapons in their hands, but disciplined through an updated version of the traditional samurai ethic as set forth in the Imperial Rescript to Soldiers and Sailors of 1882.

In contrast to the participation of common-

ers in the armed forces, the notion of popular participation in any meaningful way in the legislative process under the Meiji Constitution of 1889 was anathema to Yamagata. In his two terms as prime minister, Yamagata was alarmed at the divisive spirit of the political parties: He thought that the political structure ought to insure harmony under the Emperor, and he fought a long holding action against party cabinets lest "our nation . . . suffer the same fate as Spain and Greece" (p. 184). Near the end of his life, then a Genrō, he reluctantly accepted a party leader as Prime Minister; but he had taken care that his beloved army should stand beyond the control of economizing party leaders. It was this independence of the military that appeared a fault in the later era of uncuddled militarism when the successor generation used the superbly organized and disciplined instrument in a rash way which probably would have met with Yamagata's disapproval.

It is the aftermath, then, that, makes Yamagata's experiment in nation building less than a desirable model for contemporary statesmen in developing nations. There is, in this volume, a memorable vignette of Yamagata in mid-career eagerly awaiting installments of J. R. Seeley's *Life and Times of Stein* (1879) as they were translated into Japanese. Stein provided a notable inspiration to Yamagata; and, in office then as Home Minister, he thought highly enough of Stein's fundamental lesson of binding people to the state through civic spirit cultivated in service to local government to have the translations distributed to local officials of his Ministry. Indeed, the biography of Yamagata under review might serve today's nation builders in the same way as a guide to political and military modernization, for Japan modernized early and rapidly; and Yamagata was a central figure in that process; but the Pacific War casts a shadow over his Japanese model.

Students of comparative institutions and modernization, nevertheless, will welcome this remarkably complete record of the process whereby Japan became a modern military power and a centralized political entity. For six decades, from the 1860s to the 1920s, Yamagata participated in most of the military, political, and diplomatic decisions of state; and for much of that time his was the dominant voice in the oligarchy which modernized Japan. Not only did he promote military innovations such as the General Staff system; but he also gave final form to the system of local government in which "local policy was shaped from above," (p. 110) and he steered Japan into the Anglo-Japanese Alliance. We have before us then,

nothing less than a comprehensive history of early modern Japan.

Professor Hackett gives less attention to his subject's personality than to the public record. He treats us to a few intimate glimpses of a hard-working, upwardly mobile youth who later became an austere, laconic, Japanese gentleman, always reverent toward his Emperor. The handful of anecdotes, the more vivid for their sparsity, sharply etch the portrait in mind. One tells of the suicidal plunge of his self-sacrificing grandmother into the Abu River in 1865 lest she be a burden to her ambitious grandson. Another reveals the old soldier in politics, Prime Minister Yamagata, quivering visibly as he read his first speech to the Diet while members crouched forward to catch his almost inaudible words. Still later, we read of the aged Prince Yamagata walking daily, in rain or snow, through his garden to worship at the shrine which housed a personal sword of his late, beloved Emperor Meiji.

The volume should be read, however, not as conventional biography, and certainly not as psychohistory; for the focus is on the public record, not the private. Professor Hackett's contribution centers on his making available the first serious biography of Yamagata in any Western language, a biography firmly anchored in the Japanese sources. Nonspecialists may not realize what a tour de force such a pioneering work is. To them he gives access to this important record through a sound, detailed narrative interspersed with generous and pertinent translations from the Yamagata state papers and correspondence.

The author is conscious of changes over time in his subject's political orientation, as Yamagata, the revolutionary, became an innovator, and finally a conservative. He delineates clearly the proportions of the mix between traditional and modern elements in Yamagata's personal life and statecraft. Commendably, he avoids claiming too much for his subject, noting that Yamagata's was merely a "supporting role" in the Meiji Restoration of 1868, "a movement planned by others," (p. 51) and observing that he had hardly anything to do with Japan's economic modernization. If the author does present a more favorable, even more idealistic, picture of Yamagata than we have viewed heretofore, it is because the story is narrated through the Yamagata documents and because the time has come for reinterpretation of the loose generalization that Yamagata fathered Japanese militarism with all of its excesses.

SIDNEY DEVERE BROWN

University of Oklahoma

The Astonishing Adventure of General Boulanger. By James Harding. (New York: Charles Scribner's Sons, 1971. Pp. xxi, 251. \$8.95.)

James Harding, London-based, a musicologist, specialist in nineteenth-and-twentieth century French literature, whose previous books include ones on Saint-Saëns, Massenet and Sacha Guitry, is the author of what Scribner's publicity blurb describes as "The First Complete Biography of the Original 'Man On A White Horse.'" I am sure Mr. Harding will be fascinated to know that he holds, according to the same blurb, a *Diplome* [sic] *de la Civilisation Francaise* [sic].

Born in Rennes, in northern France, on 29 April 1837, Georges-Ernest Jean Marie [sic] Boulanger was the son of an *avoué*, that is, a confidential legal advisor, still a common feature in France, and an English-born mother. Both parents belonged to a milieu called, in French for there is no English equivalent word, *embourgeoisé*, very provincial as against very Parisian. Harding gives, correctly, due attention to Boulanger's early ambitions and his military services. It is unfortunate that he neglects that particular and peculiar milieu that was nineteenth-century French provincial life.

Georges-Ernest saw military service in what would become the perennially troubled areas of the French colonial empire: Algeria and Indo-China; he also served in Italy.

The future youngest general of the French army was wounded several times. In order to endure his wounds, he was heavily medicated with habit-forming pain-killers. As a result, and because his wounds caused him great pain through the remainder of his life, he became, in effect, a drug addict. While Harding notes this, his addiction is barely mentioned as a contributing factor to his sometimes erratic political and emotional behavior. His military abilities were quickly recognized; promotions were rapid, and his concern for the ordinary ranks, the 'soljers,' was reciprocated by adulation on the part of the *poilus*. By 1870, and the *Commune* which he helped put down, he was a full colonel, for awhile. Here, Harding, typically, makes one of his existential leaps: he tells us that Boulanger, briefly demoted to Lieutenant-Colonel, developed a fierce streak of paranoia which the author attributes to this unjust, (according to Georges-Ernest) demotion. This extreme personality characteristic is neither further explained, nor further illustrated.

Ably blending Boulanger's military and political career, as well as describing, but never explaining his hero's wenching, Harding introduces the "love of Boulanger's life," Marguerite

Caroline Laurence Brouzet, Vicomtesse de Bonnemains. His love for this woman, and the effects on his career are the most psychologically sound elements of this biography. This very personal section of the work is both a strength and a weakness: a strength in that we are better able to understand Boulanger; a weakness in that the personal life of the General is not sufficiently correlated to his political career.

Promoted to general in 1880, through means appropriate to a sycophantic courtier, Boulanger flashed across the firmament of French politics in the decade of the 'eighties as a member of several French cabinets serving as Minister of War. Boulanger's reforms of the army are rightly insisted upon by Harding. Boulanger's early political career was due to the support of the radical wing of French politicians; the Republicans, Clemenceau. However, Boulanger's days of public adulation, his flirting on the precipice of a military dictatorship, and the overthrow of the Third Republic, found him associating himself with Royalists and Bonapartists. On the one hand, Harding suggests that Boulanger's advisors manipulated him; at other times, that Boulanger himself believed the "Boulanger myth"; but mostly Harding gives no explanation at all. It happened, *c'est tout*.

Boulanger, his successes, and his failures could have been the point of departure for an analytic study of nineteenth century French society. We have, in Harding's work, a lively well-written description. In himself, his personality, character, and activities, Boulanger could have become the focal point of an inquiry into persistent characteristics of France's society since the Revolution. The persistence of *ancien régime* tendencies, in particular the transference of the *roi soleil* image of prerevolutionary days, into a nonroyalist *chef*, the "leader-father-savior" figure (cf. the French emperors, more recently, de Gaulle), are not even hinted at. Nor is there any mention of that peculiar trait of French political society: the cohabitation of conformity-statism with anarchy-individualism whose contradictions have sundered French society and politics for almost two hundred years. Boulanger's own career clearly demonstrated these consistent patterns of French political life. It is unfortunate that his biographer did not.

Even though harsh, the evidence furnished by Harding indicates an inescapable conclusion regarding his subject's political career: as has been said of a contemporary American politician: "deep down he's shallow."

It is sad that in this definitive biography that

the most commendable part of the book has little to do with France; it has, however, everything to do with Boulanger, for "nothing so became him as his death." His love for Marguerite was constant and deep. Boulanger had been exiled as a result of a supposed plot to overthrow the Republic; Marguerite accompanied him to London, to Jersey and to Belgium. Her health seriously weakened by inclement climates, Marguerite died in Belgium on July 16, 1891, at the age of thirty-five. Below her name, on her tombstone, Georges-Ernest had inscribed "*à bientôt*": soon.

On 30 September 1891, Boulanger put a bullet through his head; he was standing at Marguerite's graveside. He left wills, one of which contained his instructions for the inscription on his tombstone. It read: "Was I really able to live two-and-a-half months without you." Beautiful!

CAMERON NISH

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Social Stratification as an Obstacle to Development: A Study of Four Turkish Villages. By Jan Hinderink and Mubeccel B. Kiray. (New York: Praeger Publications, 1970. Pp. 248. \$15.00.)

This book, based on collaboration between a Dutch geographer and a Turkish sociologist, explores the development process from a micro perspective. Comparative data from four villages on the Mediterranean coast of southern Turkey at different levels of development are used to support the argument that, although economic development does lead to some changes in power and status, the ordinary villagers have not benefited from such changes. This study is creative, challenging, and original.

According to the authors, alterations in social stratification in the village have occurred as a result of several different kinds of events: wars, changes in the prices of primary products, and such technological developments as new breeds of cotton and new kinds of farm machinery. Such changes have made it possible for a relatively few entrepreneurs to become richer landowners, but "the lowest incomes are found in both the technologically most- and least-developed villages. . . . Even the average consumption scores of the most-developed villages are lower than that of the least developed one, due to changes in occupational structure and working conditions without essential changes in control power" (pp. 241-242). Existing power relationships are proposed as "the real obstacle to true social and economic

development in the villages" (p. 243).

A number of plausible rival hypotheses are explored. The authors observe that religiosity, fatalism, and authoritarian intrafamilial relations did not seem to stand in the way of development; the villagers expressed "a definite favorable disposition toward change" (p. 242). On the other hand, there was considerable population growth, out-migration, and pressure upon the local labor market. These problems might have been explored more fully.

Given the author's findings, it is important to discuss the quality of the data used. A number of methods—structured and unstructured interviews, analysis of household budgets, observations of standards of living, and the taking of a census of the village—provided the necessary information. Such a multimethod approach is one of the strengths of the volume. On the other hand, the data analysis might have been more extensive and additional checks on the data collection might have been incorporated.

Comparisons with available data gathered independently from other sources would have improved the internal and external validity of the research. Hinderink and Kiray used student interviews; one wonders how these findings might compare with other data collected through different sorts of interviews? More generally, comparison of the authors' information on the four villages with data on the same villages from the national census would have been useful.

Such sources of village information—including some published after the authors did their field work in 1964–65—might also have provided better longitudinal data. Such data would have been particularly valuable because retrospective techniques generated the longitudinal material which was used in this study. Such recalled information is obviously subject to distortion, and additional checks would have been desirable. Thus, some possibilities for stronger research designs were not fully utilized. In this connection—since the field research for this book was conducted nine years ago—the time is ripe for a return and restudy of these villages. A number of the suggestions made above might fairly easily be incorporated into a new design.

Because the findings of this research are important and controversial, further questions are raised: For example, might the population increase reported for these villages be at least partially responsible for the lack of improvement in living standards? And how general are the findings from these four villages?

From a slightly different perspective, the authors are to be commended for the linking of

their data with various theoretical formulations. One co-author of this study (Kiray) has been conducting field research in Turkey during an extended period, and thus could presumably make more pointed generalization. Such an effort by Dr. Kiray to summarize her findings, hypotheses, and speculations would be most welcome.

NORALOU P. ROOS

LESLIE L. ROOS, JR.

University of Manitoba, Canada

Indonesia: The Possible Dream. By Howard Palfrey Jones. (New York: Harcourt Brace Jovanovich, 1971. Pp. 473. \$12.95.)

How does a man reconcile his love of a people with his official responsibilities as an ambassador? How does he reconcile his friendship and respect for a national leader who pursues policies that are often inimical to the interests of the nation the ambassador represents?

In *Indonesia: The Possible Dream*, Howard Palfrey Jones provides us with his recollections of more than a decade of involvement with Indonesian politics. Significantly he humanizes diplomacy by dealing with relations among men as well as nations.

When Ambassador Jones first came to Indonesia in 1954, as Chief of the United States Economic Aid Program, the young Republic had just emerged from a violent war of national independence. Subsequently, Jones witnessed the transformation of a fragile parliamentary system into a semiauthoritarian state which was rapidly moving toward the left on the eve of his departure. Shortly after the Ambassador left, a bloody coup and reaction turned Indonesian politics around on yet another uncertain course.

Throughout this work, the Ambassador's knowledge of Indonesian culture and society is apparent. His lucid survey serves as an excellent introduction to ancient and contemporary Indonesian history. As the Ambassador who "acquired the record for the longest service at a single post by an American diplomat in the post-World War II period" (p. 332), Jones acquired a rare sensitivity to Indonesian affairs.

It is because of this sensitivity that the problem of reconciling official responsibilities and unofficial empathy for a people becomes apparent. Jones's assessment of Indonesian politics is correctly and succinctly stated, "... a sensitive nationalism easily aroused at the slightest hint of an encroachment on Indonesia's precious sovereignty by a foreign power" (p. 71); one wonders, however, if the Ambassador's perception clouded his judgment in the formulation

and execution of United States foreign policy? Was he too sympathetic to Indonesian demands and aspirations? In analyzing the decline of the Republic under "Guided Democracy," Jones candidly admits that, "I have been optimistic longer than most Americans that even with his 'noisy diplomacy,' Sukarno would finally come around" (p. 358).

Admittedly, this reviewer has the benefit of retrospect, but one must question Jones's assumption that "Sukarno's emotionalism and preoccupation with colonial issues . . . often led him to adopt an irrational course of action" (p. 272). Perhaps the Ambassador's optimism was largely predicated on his enduring friendship with Bung Karno. Jones maintains that

. . . I managed to retain the confidence and friendship of President Sukarno over such a long period, especially when the policies of our respective governments were sharply opposed and there were very few occasions when I was not either protesting specific actions or disagreeing with his policies. I can only assume there were two reasons. I always told him the truth, however unpleasant it might be. Also, he knew I loved his country and its people, and was genuinely interested in helping them attain their legitimate objectives (p. 331).

The Ambassador's virtue is admirable, but one questions whether the President believed in reciprocity. It may be that the former Ambassador was as dazzled by the President's legendary charisma as countless Indonesians were. The dangers of personalizing international relations either at the presidential palace or the summit are self-evident.

Interestingly, while the Ambassador was usually optimistic, he was subject to excessively pessimistic assessments at times. In analyzing the West Irian dispute, Jones stated, "I became convinced that war was just around the corner, for we in the embassy knew what was happening as a result of an operation that was then top secret—direct observations by U2 plane, a fact later publicly revealed by Sukarno. The Indonesians had the troops and the equipment, and public opinion had been whipped up to the high-tension level" (p. 210). Given Sukarno's mixture of sabre-rattling and bluffing in foreign affairs and the army's concern with the growing strength of the Communist Party, and given troops that were available, it remains doubtful whether war with the Dutch would have ensued.

Jones suggests that the destruction of the Communist Party in Indonesia occurred because the PKI "were all things to all men, and were found out; they themselves exposed the

wolf inside the sheepskin. They failed because they employed every weapon in the political arsenal: when they did not get their way by persuasion, they used intimidation and terror. They failed because they came with favor and stayed with fear" (p. 167). A far more useful question is why the Party came so close to power. The PKI's growth after the abortive Madiun uprising of 1948 was not simply the result of fear. Aidit and his colleagues effectively mobilized down to the village level, and often struck a responsive chord among a peasantry that was either ignored or oppressed by Djakarta-based political forces. The PKI may yet reemerge if the new military leaders fail to establish direct links with the peasantry. Indirectly, Jones is aware of the inherent dangers in military rule: "But let the military leadership at any point be tempted by the heady wine of power to exceed its basic functions and a nation that has shed so much blood on the altar of freedom could again be denied the rewards of its revolution with a consequent renewal of internal unrest and upheaval" (p. 413). I share the Ambassador's respect for the Army's very impressive programs of economic stabilization over the short run. But, the threat of future conflict will remain unless the new leadership can build a political process that effectively encompasses the diversity of the Indonesian political culture.

In his epilogue Jones says: "The basic failure of American foreign policy in Asia in the period following World War II was in the coldness of our response to the new nationalism of Asia and Africa" (p. 420). For an individual who worked within the superheated climate of Indonesian nationalism, the Ambassador displayed an awareness of the potency of a new force that many of his peers may have missed. Perhaps he was too responsive to the rhetoric of Sukarno, but the liabilities of overidentification may be far less dangerous than the insensitivity toward people's aspirations that has led to the continued nightmare of the Indochina War.

STEPHEN SLOAN

University of Oklahoma

Monarchy in the Emperor's Eyes: Image and Reality in the Ch'ien-lung Reign. By Harold L. Kahn. (Cambridge: Harvard University Press, 1971. Pp. 314. \$10.00.)

If one measures success in terms of lastingness and capacity for self-adjustment and renewal, the political system which prevailed in China under the empire was without doubt one of the most successful ever devised by man. It first took shape with the unification of the sepa-

rate Chinese states by the Ch'in dynasty in 221 B.C. After incorporating Confucianism as its official ideology in the following Han dynasty, it persisted in essentially the same form with, of course, a great deal of elaboration and accumulated subtlety in its operation, down to the abdication of the last Manchu emperor in 1912. The emperor and the bureaucracy were the two principal elements in this system and their complementary, but also often conflicting, roles provided most of the dynamic behind both the political theory and the actual evolution of institutions.

More attention has been paid by western scholarship to the history of Chinese bureaucratic institutions than to the other side of the picture, the history of the monarchy. Professor Kahn's study is a notable contribution toward redressing this balance. The emperor known by his reign title as Ch'ien-lung lived through most of the 18th century (1711-1799) and was on the throne a great part of that time (1736-1795). His reign coincided with the apogee of the Manchu dynasty, in terms both of imperial power and of economic prosperity, intellectual achievements, and cultural splendor. As a monarch fully conscious of his position during his own reign and as part of the long line of Chinese monarchs extending down from legendary times, he provides an excellent focus for studying the monarchy in depth.

A problem one has to face in approaching any political system is the contrast between how it appears in its outward forms and symbols and how it is in actuality, controlled and influenced by the interests and personalities of the fallible human beings who are involved in it. When we are concerned with an institution as wrapped in ritual symbolism and as weighed down with traditions from the past as the Chinese monarchy had become by the 18th century, the difficulty of getting at the human reality behind the appearance is truly formidable. Professor Kahn's strategy has been to examine in detail three facets of the multifaceted problem (a) official historiography, (b) the education which Ch'ien-lung received as a prince, (c) the abdication of Ch'ien-lung after 60 years on the throne.

The problem in historiography that is taken up is at first sight surprising since it is not even about the Manchu dynasty. It is the succession of the Yung-lo emperor of Ming, who supplanted his nephew, the Chien-wen emperor, in 1403. This succession became crucial for Chinese official historiography not only because of still unresolved obscurities about what actually happened, but also because of many accretions

to the story that grew up in popular legend, and because of the felt need to present through the official version an acceptable image of the imperial institution. The account which Professor Kahn gives of the handling of this issue over the years from the time of Yung-lo onward, therefore, is relevant to his larger theme, the Chinese conception of emperorship; but it is immediately relevant to Ch'ien-lung because the emperor was personally responsible for a final revision of the *Ming History* which incorporated his views on how the incident should be interpreted.

As an appendage to the section on historiography, Professor Kahn explores briefly the differing pictures of the Ch'ing imperial system that one can find in unofficial works, ranging from the private writings of high officials to the often salacious extravagances of popular fiction. He also touches on the theme of dissent as found in the writings of independent-minded political philosophers. Though such criticism of the system had to be increasingly veiled and circumspect as the pressures for conformity grew more and more severe under Ch'ien-lung, it was never completely stifled.

The problem of image and reality is again the underlying theme of the section on Ch'ien-lung's education, which was designed to prepare him to become the ideal Confucian sage ruler. Because of the practice of keeping the succession secret—a practice initiated by his father Yung-cheng—he was not yet clearly marked for his unique role, and he shared his upbringing on a basis of ostensible equality with other princes. The grounding he received in the classical curriculum of Confucian learning was comprehensive and rigorous, but that in the traditional martial arts of his Manchu ancestors was less so. When one gets past the clouds of sycophantic adulation in which his every utterance and action were inevitably wrapped at the time, one finds his achievements as a poet, essayist, philosopher, calligrapher, and painter mediocre and uninspired. On the other hand, his historical scholarship was genuine and deep, and his awareness of historical precedents certainly played a great role later both in his actual conduct of affairs and in the image of himself that he sought to create for posterity.

From this point of view the final episode in his reign—his abdication—is especially fascinating. Professor Kahn reviews the history of abdications in China which begins with the legendary sages Yao and Shun. Their voluntary withdrawals in favor of their chosen successors after long reigns theoretically set a model for later rulers. In practice, however, these exam-

ples had mainly provided convenient precedents through which the forcible seizure of the throne by an imperial prince or the founder of a new dynasty could be legitimized. In southern Sung there had been voluntary and genuine retirements. Ch'ien-lung's differed from these in being voluntary but not genuine. That is, he went through the elaborate charade of abdication for the sake of his place in history while still keeping the actual control of affairs in his own hands (or rather, alas, in the hands of his corrupt favourite, Ho-shen).

Here, and throughout his study, Professor Kahn illuminates the immediate questions at hand by means of parallels and contrasts with earlier dynasties. This makes the book much more than a commentary on certain limited aspects of one reign. It becomes a richly worked tapestry in which many themes are subtly interwoven. Without setting up any elaborate framework of social science concepts, Professor Kahn makes a major contribution to our understanding of the workings of the traditional Chinese political system that is also full of human interest and a pleasure to read.

E. G. PULLEYBLANK

University of British Columbia

Government in Rural India. By Iltija H. Khan. (Calcutta: Asia Publishing Co., Calcutta, 1971. Pp. 185.)

Professor Khan has developed the thesis that the success of nation building in India will depend upon the governmental process at the village, block, and district level. In developing this thesis he has summarized the history and development of Indian panchayats since Independence and has realistically evaluated the shortcomings of factionalism, the failure of villagers to understand the functions of panchayats, administrative neglect, lack of coordination, the failure to use panchayat machinery, the inertia of rural society, and the underestimation of the divisions in Indian society.

The Report of the Study Team for Community Development and National Extension Service is proof that the initial enthusiasm for community development has waned. Professor Khan's major emphasis is upon the unworkability of the block as the keystone of the program and his support of the district as the basic unit of planning and coordination of development with execution of project development at either the village, block, or district level.

Professor Khan distinguishes between those types of planning which may realistically be carried on locally or at the district level and planning which is an essentially regional or na-

tional. He realistically observes that adoption of the district as the unit of planning does not necessarily mean that it should also be the unit of implementation of programs and indicates that the nature of the subject matter should determine the operation unit of administration. What is significant here is the need for integrated development.

Nearly all examination of the development of the panchayat have assumed consensus and partyless government. The Indian experience fails to support these assumptions. As Professor Khan observes, factionalism which has not been diluted by party politics has pervaded the panchayat. He pleads instead for strengthening party organization and involvement in panchayat affairs as a means of socializing the individual and increasing involvement in the local political process.

A serious problem facing the program is the relationship between the public officials (such as the Block Development Officer and the District Collector) and the Panchayat Samiti Pradhans and the Zila Parishad Pramukhs. The political power of the Panchayat Samiti Pradhan is often greater than that of the Block Development Officer, while the District Collector is likely to be a stronger personality than the Zila Parishad Pramukh. The result is often conflict, stalemate, and confusion. While Khan's assessment for the future is optimistic, I have seen a sufficient number of these persons functioning to experience great concern that the community development program will be arrested by a rapid turnover of official personnel and factionalism.

Professor Khan has made it clear the future of community development depends less upon the structural relationship between the official and nonofficial groups than upon the broad cultural factors. Until feudal social and economic structure can be replaced by an open, competitive system, supported by higher levels of education, awareness of alternatives and increased communication, the existing structures will continue. His volume should be read by those who continuously sing paeans of praise for the progress in idyllic Indian villages.

BENJAMIN N. SCHOENFELD

Temple University

Memoirs of a Russian Diplomat: Outposts of the Empire, 1893-1917. By Andrew D. Kalmykow. (New Haven: Yale University Press, 1971. Pp. 290. \$12.50.)

These diplomatic reminiscences could have been written deliberately to epitomize the differences between past and present methods of

analysis in international relations. The author is a near-perfect example of an outdated diplomatic type, a reflective and somewhat romantic dilettante observing life from the Foreign Ministry and the consular office. Gently critical at times, aware that the Russian diplomatic service was less than adequate, he was yet incapable of the kind of penetrating study and forceful reform effort the diplomatic bureaucracy needed. Unknowingly, he provides a shocking demonstration of how his nation at the turn of the twentieth century foundered in its own ignorance and arrogance, and, for lack of a guiding intelligence, blundered willy-nilly into a series of catastrophes.

Kalmykow himself was somewhat better trained for his profession than the average Russian diplomat, for he had studied Persian at the School of Oriental Languages in St. Petersburg, and he was driven by an abiding fascination with the cultures of the East. Most of his colleagues in the Foreign Ministry were simply well-groomed young men whose parents had secured them a quiet and respectable post; they were content to come to work at noon and while away their duty hours scheming for promotion; and not even the high officials who headed the ministry concerned themselves overmuch with studying the issues of conflict, or worried about their ignorance of matters crucial to Russian interests.

Kalmykow, a quiet zealot, insisted upon assignments to exotic areas feared by other men. As a consequence, from 1896 until 1917, he served, at ascending rank, in Teheran, Bangkok, Tashkent, Ashkhabad (Poltoratsk), Uskub (Skopje), and Smyrna. Most of his pages are devoted to accounts of famous people and formal social functions, as if he judged his own accomplishments by the numbers of state ceremonies attended and people of high rank encountered. As a faithful servitor of the tsars he seems generally complacent about his associates; yet he repeatedly reveals levels of incompetence among them that are staggering. Assigned to Bangkok, he diligently sought information about Siam at the ministry, only to discover that no staff member could tell him anything at all about the country. Assigned to Uskub at a tense moment just before the outbreak of the First Balkan War, he received no instructions and no mission, except, ludicrously, to keep the peace. As such illustrations pile up, rounding out the picture of Russia's disdain for lesser nations and the suicidal bureaucracy of the Foreign Ministry, the reader increasingly wonders how the Russian Empire survived as long as it did.

Genuine diplomatic achievements, as described by Kalmykow, were usually the result of the audacity or imagination of some official in the field who dared to seize hold of an opportunity without checking with St. Petersburg. Here and there a consul general or ambassador, without directions from above, managed to cultivate a friend in the host government or to take action based on a daring estimate of the probable course of events. Thus, on occasion, in Persia and Siam, bold men took grave chances and advanced Russia's interests. Kalmykow himself, for example, halted a threatened war by imaginatively employing his personal influence at Bangkok and Saigon, and in Serbia-Macedonia he helped to avert bloodshed by negotiating the takeover of an embattled city.

Through these nostalgic recollections of court functions, decorated nobility, little work, and considerable ennui, the tragic message is always present. Behind the grandeur, a Foreign Ministry submerged in formalism and trivia and crippled by its inept staff was incapable of performing its complex functions. Kalmykow's romantic view of leisurely events, seemingly light-years behind us, outlines only too clearly the approaching ruin of a political system that could not function well enough to save itself.

ARTHUR E. ADAMS

The Ohio State University

Guerrillas in Power: The Course of the Cuban Revolution. By K. S. Karol. (New York: Hill and Wang, 1970. Pp. 624. \$12.50.)

The author of this book, a Marxist journalist and writer on politics, was born in Poland, spent seven years in the USSR and since 1946 has lived in Paris. He visited Cuba at crucial points in the history of this nation, for a total of some seven months: April and July 1961, July-August 1967, and January-March 1968. Karol traveled throughout the country and held long interviews with Premier Castro, Che Guevara, and President O. Dorticós. The first edition of his book, published in France, provoked a violent critique of Castro which became generalized against the European radical left during Cuba's Cultural Congress of April 1971.

The book is divided into two parts. The first includes an account of the early years of the Revolution until Cuba closed her links with the USSR in 1961 (and Karol visited the country for the first time) an abridged history of the pre-revolutionary Communist Party of Cuba (PSP) from its founding in 1925 until 1959; and a description of the relations between the USSR and Cuba until 1967 (at the time of Karol's

second visit). Part two of the book is composed of a discussion of what Karol calls the "Cuban heresy" (i.e., the 1966-68 departure from Soviet orthodoxy and development of a unique system influenced by the Chinese but shaped by Guevara and Castro); a description of the internal problems confronted by the leadership and the conflict with the USSR; and an analysis of Cuba's return to the Soviet path. The book closes with a series of appendices: a useful chronology of Cuban events from 1868 to February 1970; a brief and partially obsolete selection of Cuban official statistics on population, education, public health, agricultural and industrial output and foreign trade; and a list of Cuban officials in top posts, completed around 1968.

The book is well written and interesting. Most important are the book's yielding accounts of interviews held with Cuban leaders and of direct observation and anecdotes, unknown facets of the Cuban leaders' ideological stand. Karol provides new angles on well-researched events, clarifies some obscure points in recent Cuban history, and offers a perceptive analysis in his concluding chapter. But the book suffers from several flaws: a serious lack of organization, excessive details in dealing with events that have already been fully documented (e.g., the 1961 Cuban National Production Meeting), misinterpretations of Cuban history (for instance, the role of the Cuban "Soviets" during the 1933 Revolution is over-emphasized, while that of the ABC party is underemphasized), important quotations from Cuban officials left unidentified (apparently an influence of C. R. Mills' *Listen Yankee* style), and frequent misspellings in Spanish names and quotes. There are also some gross factual errors. (For example Karol states that the prerevolutionary sugar harvests used to begin in late November, when the sugar content of the cane was at its highest, and employed some 400,000 canecutters who cut five tons of cane daily for a total crop of about five million tons of sugar. In fact the harvest began in January, the peak in sugar content was reached in March, less than 300,000 canecutters were steadily employed, and they cut about two tons of cane daily. If Karol's data were correct, the harvests could have easily reached 20 million tons of sugar.)

One of the problems that Karol faced in preparing his book was his absolute lack of knowledge of Cuba prior to the Revolution, as he himself acknowledges. Furthermore he does not seem to have kept track of Cuban events between his trips, particularly between 1961

and 1967 when he was first in China and then writing about his experience there. Hence it is not surprising that in 1967, Karol was aware neither that there had been an important Cuban debate on alternative models (1962-1966) nor that Guevara was the leader of the Maoist stand. Karol learned these facts through a second-rank Cuban official when the debate was over, three months before Guevara's death. He hurriedly tried to research six crucial years of Cuban socialist history plus the pertinent prerevolutionary antecedents while he was traveling and doing interviews.

The most revealing parts on this book are those dealing with the ideological stand of the Cuban leaders. Karol presents the little-known side of Che Guevara of 1961 as a naive admirer of the USSR, who uncritically accepted the Soviet model and trusted too much in Soviet generosity and the efficiency of the planning "clock." (Che candidly admitted to Karol that every revolution had its share of Stalinism, because of capitalist encirclement, but that this evil would not fully evolve on the island thanks to the Soviet aid.) Then Karol explains how Che gradually turned into a severe critic of the USSR because of Khrushchev's betrayal in the Missile Crisis, and because of the poor quality of Eastern European goods, the failure of the Czech-designed economic plans, the discovery of the technological backwardness of the Soviet bloc (whose technicians were incapable of handling the U.S. equipment), and the disenchantment with the Soviet terms of trade. Che's position closely approached the Maoist model but with an important exception: He still endorsed the Soviet myth that the working class's main interest was production and that the leaders knew best how to interpret the thoughts and needs of the masses.

According to Karol, Castro underwrote Che's latest stand from the very start but, being more pragmatic and diplomatic, he tried to avoid unnecessary frictions with the USSR until 1966 when the clash was inevitable. Castro assured Karol that Cuba was going farther than China because of Cuba's goal of abolishing money altogether. And yet Karol maintains that the Cuban Premier did not agree with the Cultural Revolution (nor with the Czech Spring) fundamentally because he was afraid of changes initiated by the rank and file, in an uncontrolled manner, which could paralyze Cuba's socialist allies, thus leaving her at the mercy of the United States.

Karol's main conclusion is that at the end of the 1960s, Cuba suffered from declining revolutionary fervor, waning interest in politics,

poor labor productivity, and increasing antisocial behavior because the masses were not allowed to participate in the decision-making process reserved to Castro and his inner circle. Facing failure in their attempt to develop a "new man," this elite increasingly resorted to authoritarian and police methods, tightening of labor discipline, and militarization of the economy. Karol's prescription to cure these evils is to apply the Maoist model, at least that of the Cultural Revolution. Thus, presumably Cuba should go back to the "heresy" of 1966-68 but with the crucial addition of rank-and-file initiative and decision-making power. Three years after Karol published his book, the Sovietization of Cuba seems so complete (and the Maoist model has been watered down so much) that the possibility of applying his prescription appears highly improbable if not totally utopian.

CARMELO MESA-LAGO

University of Pittsburgh

The Middle Beat: A Correspondent's View of Mexico, Guatemala, and El Salvador. By Paul P. Kennedy. Edited by Stanley R. Ross. (New York: Teachers College Press, Columbia University, 1971. Pp. 225. \$6.95.)

Paul Kennedy was the chief correspondent for *The New York Times* in Middle America from October, 1954 to December, 1965. He died in 1967 after completing a manuscript covering political events in Mexico, Guatemala, and El Salvador.

As an avid reader of the *Times*, I looked forward to reading his book with the hope that an intelligent journalist, freed of the time, style, and space confines of the newspaper format, would be able to supply the student of Latin American politics with insights that scholars might overlook. These hopes were disappointed: *The Middle Beat* is not a book; it is a 200-page newspaper article, much of which is simply spliced sections of dispatches that Kennedy published in the *Times*. Many important topics are handled in short, choppy, cliché-ridden paragraphs.

This lack of analysis seems to arise because, although Kennedy recognized that economic and political changes were taking place, he had few organizing concepts to help him distinguish between what was important and what was trivial. For example, the book spends a great deal more time on elections in Middle America than in explaining the rise of the Central American

be partially attributed to a lack of familiarity with the scholarly literature concerning this region. A few traditional historians are quoted, but the only political scientists mentioned are Harry Kantor, Robert Scott, and Frank Tannenbaum. Kennedy also shows no awareness of the studies of anthropologists Oscar Lewis and Richard Adams, nor of the growing number of studies by Mexican social scientists. Consequently, Kennedy's book ends up being a description of surface events and phenomena.

Mr. Kennedy also neglects to analyze aspects of newspaper work that would be of interest to readers and especially to scholars. It would be worthwhile to know how Kennedy, who had covered Spain for the *Times* during the early 1950s, prepared for his assignment to Middle America in 1954. What did he read? How difficult was it to make contacts with people who knew the facts of the situation and were willing to explain them to him? What kind of policy guidelines did the *Times* designate about the kind of stories to pursue? How does a reporter seek out the truth in societies with partially-controlled presses and supersensitive politicians who have the power to oust the over-inquisitive foreign reporter?

The biggest disappointment of the book, however, is the manner in which Kennedy deals with the secret training base the C.I.A. set up in Guatemala in order to invade Cuba in April, 1961. On October 30, 1960, the Guatemalan newspaper *La Hora* stated that the C.I.A. had built a training base for Cuban refugees near the mountain town of Retalhuleu. One would have expected a diligent reporter to have investigated this story immediately, but Kennedy did not get anything in print until November 20. In that unsigned dispatch (why unsigned?) Kennedy published excerpts from an interview with Guatemalan President Miguel Ydigoras Fuentes who claimed this base was being used to train Guatemalans in the event Cuba invaded Guatemala. Early in January, 1961, Kennedy finally visited the secret site and published his findings in the January 10 issue of *The Times* under the heading, "U.S. Helps Train an Anti-Castro Force at Secret Guatemalan Air-Ground Base." Kennedy was severely criticized by the Ydigoras government for this story, and he then (voluntarily?) left Guatemala. In the book his final words concerning this investigation were, "For security reasons I did not use sizeable portions of the material

know—that are of great interest to the reader. We would like to know on what grounds Kennedy decided to publish certain facts and to exclude others. Was there any pressure from the hierarchy in the *Times* or the Kennedy Administration not to publish certain information? Was the reporter engaged in an artful compromise between his professional obligations to tell the truth and his support of the Kennedy Administration when he ascribed most of the damaging pieces of information—those that indicated the U.S. and Guatemalan governments were lying—in his January article to “opposition forces” in Guatemala. Were we supposed to believe less in “opposition forces” than in “official sources?”

Paul Kennedy's reporting of these events has been attacked from two opposing points of view. Soon after the Cuban invasion, his revelation regarding the base was attacked by President Kennedy as a premature disclosure of security information. And, in the Fall, 1967 issue of *The Columbia University Forum*, Victor Bernstein and Jesse Gordon attacked him for diluting the truth in the name of national security. Kennedy never mentions the President's complaint in his book, and he died in 1967 before the Bernstein and Gordon essay was published. However, the editor of this book, the historian Stanley R. Ross, does defend the author in an appendix entitled “The Base of Retalhuleu.” Although Ross's defense is well-argued, this book would have been more valuable if Kennedy had been willing to defend himself.

JOHN W. SLOAN

University of Houston

Lesotho 1970: An African Coup under the Microscope. By B. M. Khaketla. Perspectives on Southern Africa, 5. (Berkeley: University of California Press, 1972. Pp. x, 350. \$10.00.)

Since the Kingdom of Lesotho presents a rare African example of an ethnically, linguistically, and culturally homogeneous state, the severe factionalism and acute political instability of its postindependence era seem especially tragic and unnecessary. In seeking “to identify . . . the factors, immediate and remote, which contributed to the crisis into which Lesotho was plunged in 1970” (p. 1), B. M. Khaketla has produced a scathing polemic against the regime of Prime Minister Leabua Jonathan and a somewhat more restrained critique of the behavior of Ntsu Mokhehle, the leading opposition politician. Mr. Khaketla's characterization of the crisis as a “scandal” (p. 1) reflects his basic assumption that the roots of Lesotho's problems can be found in the personal idiosyn-

crasies, opportunistic strategies, and faulty decisions of these key political actors.

Lesotho 1970 is an intensely partisan memoir chronicling the constitutional development of that nation and the constant infighting among the conservative National Party Government, the militant Congress Party Opposition, the royalist Marematlou Freedom Party and the young monarch, King Moshoeshoe II. Mr. Khaketla played a prominent role throughout this period as the outspoken editor of the respected political journal, *Mohlabani*. In addition, he served on various constitutional commissions, as a member of the pre-1965 Executive Council, as founder and later Secretary-General of the MFP, and finally as the select Privy Councillor to the King. Given the extent of his own involvement, Khaketla's overemphasis on personality factors to the virtual exclusion of any more general conception of political behavior, political coups, or political and economic development is understandable. More surprisingly, Mr. Khaketla rarely draws new insights from his intimate linkages with the major political participants and almost never delves into his own aspirations, triumphs, fears and self-evaluations. Thus, the book becomes just another outlet for the endless factional struggles among Basotho leaders and offers an important example of the pathology of politics in the Kingdom of Lesotho rather than an explanation.

Khaketla approaches his task with a veritable legalistic passion for verification of his assertions. To this end, he includes lengthy and often undigested segments from parliamentary debates, public speeches, constitutional documents, party journals, and press reports. Although such materials are intrinsically interesting, they often become unsatisfactory substitutes for the more incisive and reflective analysis which Mr. Khaketla could have provided. The data which he introduces on Prime Minister Jonathan's efforts to negotiate some sort of return to constitutional rule are most welcome since they include verbatim segments of hitherto unpublished party position papers used in the delicate discussions. By contrast, lengthy eye-witness accounts of atrocities occurring during “the reign of terror” (pp. 262–291) following the 1970 coup serve only emotional purposes and may well conjure up the usual stereotypes of African savagery instead of focusing attention on the actual sources of unchecked factionalism. Moreover, one must wonder why Mr. Khaketla draws so heavily on English-language South African newspapers, especially after he has impugned the reliability

and objectivity of their coverage of politics in Lesotho.

The quality of analysis throughout this book is rather uneven. Khaketla is at his best in demonstrating how Chief Jonathan shrewdly manipulated both negotiations with the opposition and Lesotho's perennial agricultural shortfall to secure the restoration of vital British aid without having to yield any substantive concessions. Similarly, he cogently analyzes how the ruling National Party utilized the governmental bureaucracy to augment and eventually supersede its own rudimentary organizational capacities (p. 174). On the other hand, Khaketla allows his marked bias against Indians to flavor his comments on the role which this small minority group has played in politics in Lesotho (pp. 40-41). More importantly, he fails to probe very deeply into the alternative courses of action available to King Moshoeshoe II in his pursuit of executive powers or to the royalist MFP in its efforts to become a major political force. Ironically, Khaketla directs his sole criticism of the King's political judgment (p. 68) to the very issue on which Palmer and Poulter imply that it was the High Court, not the monarch, which erred (*The Legal System of Lesotho*, Mitchie Company, 1972, pp. 300-304). In addition, Mr. Khaketla is all too ready to villify Chief Jonathan as a "lackey of the Republican Government" (p. 31). He neglects to buttress his assault by offering a plausible set of more appropriate policy options open to the leaders of Lesotho in coping with the acute dependence of their enclave state upon its powerful neighbor, racist South Africa.

Despite these weaknesses, the general tenor of *Lesotho 1970* seems appropriate. Certainly, Prime Minister Leabua Jonathan's refusal to surrender power after losing the 1970 general election, and his thorough disregard for democratic norms since that time make him and his regime legitimate targets for severe reproach. Likewise, there is ample need for an effective rebuttal of Chief Jonathan's facile contention that authoritarian expedients were essential to promote national solidarity. B. M. Khaketla's multi-faceted professional career as a university-trained scholar, linguist, novelist, poet, and teacher provided him with the optimal background for accomplishing these objectives. While his passionate involvement in contemporary political events makes for interesting reading, Khaketla's inability to achieve some detachment from his subject matter would seem to have compromised his overall effectiveness. In attempting to score points against his political enemies, Mr. Khaketla has undercut his

broader effort to demonstrate that the people of Lesotho have a long and distinguished national heritage and "do not need Chief Leabua Jonathan to make them what they already are, have been and always will be" (p. 334).

RICHARD F. WEISFELDER

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The Politics of Military Revolution in Korea.

By Se-Jin Kim. (Chapel Hill: The University of North Carolina Press, 1971. Pp. xiv, 239. \$9.25.)

There are, in general, two schools of thought on military involvement in civil politics in developing nations. One is essentially a negative view, which is shared by Morris Janowitz, Manfred Halpern, Edward Shils, and S. E. Finer. It is the view that the military should not intrude into civil politics because, in the long run, the military would hamper the growth of a democratic political system. It is quite conceivable that in the short run the military can restore order and maintain stability, for it emphasizes hierarchy, obedience, and discipline. But the ill effects of the military rule are the silencing of the opposition, the manipulation of the populace, the lack of provision for orderly succession, and the simplistic attitude toward problem solving.

The other view is represented by such scholars as Lucian Pye, Samuel Huntington, and Rupert Emerson. It is essentially a positive one, holding that the military can assume a positive role in promoting modernization while maintaining stability in developing nations. This is said to be particularly true in those nations which have had a long tradition of an administrative authoritarian system in the absence of a firm constitutional tradition of peaceful change. In such countries the military may become the only cohesive and disciplined unit with a modern scientific orientation; consequently, the military coup or revolution should be welcomed as a genuine alternative in bringing the necessary change and progress toward modernization. This was the view subscribed to by many in the 1950s and 1960s in justifying military involvement in the politics of such countries as Turkey, Pakistan, Burma, Indonesia, Vietnam, and Egypt. And it is the view advanced by Kim in this book detailing the role of the military in South Korean politics.

More particularly, Kim advances the thesis that the 1961 military coup, which brought overt military intrusion into civil politics for the first time in South Korea, was inevitable, because the thirteen-year civil rule by the two previous regimes—Rhee's First Republic

(1948–60) and Chang's Second Republic (1960–61)—failed to accomplish the two most important tasks of nation building. Their failures were, first, to create "an efficient political institution to articulate the public's demands and the nation's needs" and, second, to conjure up "a political ideology to capture and stir the imagination of the population." Instead, they succumbed to their own inabilities and petty political desires. Under these circumstances, the military, which "possessed the ultimate means of political power and subscribed to a more modern and pragmatic outlook . . . became a new political force . . . [and staged the coup] in the name of restoring constitutional democracy—a goal which, to some extent, has been achieved" (pp. viii–ix).

This is too rationalized and oversimplified a view of the process of political change in South Korea. It is difficult for the reviewer to conceptualize the military takeover of the civil rule in such a dialectic manner. For example, as the author has charged, the Rhee regime was guilty of political corruption and economic neglect, and the Chang government during its short period of rule was unable to correct the prevalent social and economic ills of the country. The military in the meantime became dysfunctional because of growing factional struggles among the officers' corps along the lines of geography, military service, school, and age. Social friction also occurred between the "new" modernized military (which was becoming too big to manage) and the "old" conservative civilian elites. Even assuming that all of these "facts" were correct, however, one cannot conclude (as the author does) that they would necessarily lead to the kind of military coup and its successful seizure of power of the country's political system at that particular time. The coup was a result of a conspiracy by a dozen key discontented officers and was implemented by less than 5,000 soldiers out of the total of 600,000 armed forces. It could very well have failed for a number of reasons if, for example, the regular military high command was determined to crush it. It succeeded because the Chang regime was either incapable of using or unwilling to use coercive power to prevent a small *illegitimate* group of soldiers from taking over its *legitimate* government.

As to the effects of the military takeover, the author seems to give too much credit to the military regime for peace and prosperity in Korea. He, for example, states that "the successful implementation of the entire developmental program could not have been achieved without the stability assured by these political organiza-

tions" [the Central Intelligence Agency and the Democratic Republican Party have been developed by Kim Jong-Pil, the master mind of the coup and the present Prime Minister] (p. 175). Another example of his almost "blind" faith in the present quasi-military regime is his statement that:

What can be concluded from this almost fortuitous success of the present leadership is that modernization is not really as complex and difficult as has been supposed. It can occur if there is a determined leadership with organizational skill . . . committed to such a goal (p. 175).

He goes on to say further that South Korea has experienced great progress in its economy and public administration, because "former military officers have in the last ten years become managerial experts in both private and public sectors" (p. 175). This is not only an oversimplified view of the modernization process but also an exaggeration of the military regime's success. One cannot give the military the total credit for the success of the First Five Year Plan, because much of its foundation has been built over the years during the precoup civil regimes.

Throughout the book, the author is preoccupied with rationalizing what he considers to be "the causes of the military coup." Hence, he has failed to deal with the future of the military involvement in civil politics, especially its effect on the development of liberal political democracy in Korea. There is, of course, no need to assume that the establishment of liberal democracy is the ultimate objective or ideology of the present quasi-military leadership, for Korea throughout most of its history has been governed as an administrative authoritarian system.

In spite of these criticisms, the book is well written and well documented. It is the first book written in English which has dealt almost exclusively with the cause and role of the military coup in South Korea. It therefore deserves serious attention from students of Asian politics.

JOHN C. H. OH

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Rebellion and Its Enemies in Late Imperial China: Militarization and Social Structure, 1796–1864. By Philip A. Kuhn. (Cambridge, Mass.: Harvard University Press, 1970. Pp. 254. \$8.50.)

Any student of modern Chinese history soon learns to pay attention to the role of the military in Chinese domestic politics and power dynamics. In this book, Philip Kuhn examines the

role of military growth in the social setting of China during the 19th century. Building on the studies of the foremost Chinese historians, such as Lo Erh-kang, and on some earlier analytical efforts by American authors, including this reviewer, the author has produced a superb, detailed study of that period and of the emergence of local military-political organizations during that time. In systematizing the complex institutional growth through case studies as well as through general analytical groupings, Professor Kuhn has made exhaustive use of a great variety of Chinese material, from local gazetteers to individual biographies and collected writings of both major and lesser participants, to trace the story of the militarization of Chinese society during the late Ch'ing period.

In close approximation to this reviewer's earlier analysis, Philip Kuhn draws two theoretical distinctions: one, the distinction between "natural" and state-imposed local military organizations; and the other, that between "orthodox" and "heterodox" leadership of the "natural" local military community organizations. The "natural" military organizations, in the author's terminology, are those that grow out of the village and town communities below the state administrative structure. The state is always attempting to tie this "local defense to the network of bureaucratic accountability," but without much success. The basis of "orthodox" natural military community organizations is the *t'uan*. Combined with the term *lien* (training in arms), *t'uan-lien* means "to prepare the community to bear arms." In times of crisis and of decline of the regular government troops, this community self-defense is a local necessity. Local officials, of course, always attempt to retain control over these locally established defense units. The effectiveness of these units, however, depends on leadership by the local elite—the gentry, which, while loyal to the existing order, is primarily interested in the defense of its own communities and social positions more than in the defense of the state. The interplay of this struggle for authority in the establishment and management of the *t'uan-lien* in the 19th century is brilliantly described by the author in numerous case studies, clarified by several maps indicating the location of the gentry lineages and of the *t'uan* organizations in key areas in which this local military growth occurred.

From the description of the *t'uan*, the author moves up to personal armies, demonstrating how local military forces, beginning with a predominantly local perspective, move outward from home districts to home provinces and

eventually to the affairs of the empire, a development in which personal armies, based on recruitment from the *t'uan-lien*, provide forces for the regional commanders of the period. Professor Kuhn traces in considerable detail the history of these armies and their commanders, the highest tier of the three layers of local military development.

These "orthodox" forces of a "militarized" social structure were paralleled, as Kuhn recounts, by a similar growth of "heterodox" military hierarchies of forces that rebelled against traditional society and state. When the rebellions failed, the system of "orthodox" local military leadership survived throughout the remainder of imperial times and into the republican period.

In his concluding section, Professor Kuhn raises several interesting speculative questions regarding the role of this local military leadership under the impact of the major institutional changes that occurred with the birth of the republic. The author believes that a "certain degree of social continuity" existed, even though the end of degree status, privileges and sources of income of the old order would force the rural gentry "to forge new links to the formal structure of state power" (p. 222). The question of the decline of Confucianism, of the vain efforts of the last Confucian philosopher K'ang Yu-wei to transform Confucian concepts for modern use, may be outside the scope of this institutional study; but the issue, at least, could have been mentioned in the author's broad analytical framework. In its institutional analysis, however, this is an excellent book to be highly recommended to any student of modern Chinese history.

FRANZ MICHAEL

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Latin American University Students: A Six Nation Study. By Arthur Liebman, Kenneth N. Walker, and Myron Glazer. (Cambridge, Mass.: Harvard University Press, 1972. Pp. 296. \$11.75.)

More than twenty years ago there was widespread recognition of the political importance of university students in Latin America, but virtually no useful research on this subject. Through the mid-1960s evidences of student political activity outran the rather spotty beginnings of an analytical literature, and only during the past five years have the results of reasonably sophisticated empirical field research on a number of the area's countries begun to find their way into print. Now the systematic comparative work catalyzed by Seymour Mar-

tin Lipset nearly a decade ago has finally resulted in this important study by a group of his former disciples. (Since they were brought into the project after the fieldwork had been completed, responsibility to that point rests in Lipset's hands.) Grateful as we must be for the hard data provided, much of it has aged quite rapidly, and the authors at times assume a continued relevance which is not demonstrated. They argue cogently for the utility of concentrating upon Mexico, Colombia, Uruguay, Paraguay, Panama, and Puerto Rico as including "[s]harply different versions of competitive party systems" as well as traditional and leftist variants of authoritarianism. Yet each appears dissatisfied with the limitations of this sample and to varying degrees reaches out to include developments, often more significant, in other countries. Rigorous as is much of their work, the authors somewhat disconcertingly pass *obiter dicta* judgments on countries excluded from their research design. Hence, for maximum usefulness this study should be read along with Frank Bonilla and Myron Glazer's *Student Politics in Chile* (1970), Richard Walter's *Student Politics in Argentina* (1968), Jaime Suchlicki's *University Students and Revolution in Cuba, 1920-1968* (1969), and Orlando Albórniz's works on Venezuelan students in order to have a sufficiently broad Hispanic American comparative dimension. As is too often the case, this book has little proven validity for the other two-fifths of Latin America whose heritage is Portuguese, French, or British rather than Spanish.

The authors find the period of their survey work, 1964-1965, to have been one of "reassessment and a search for new and better ways of dealing with old and seemingly lasting national and university problems." This temporal limitation should also be kept in mind while considering their profile and analysis of the Latin American university student. Lipset's brief introduction aids in this task by placing the question of student political activism in historical perspective: The project's intellectual godfather asserts that "By concentrating on students in countries with similar religious, political, and cultural traditions whose universities derive from a common model, the authors have been able to narrow their focus to the sources of variation in student behavior which emanate from different political systems and levels of development." He defines the central inquiry of the study as "What is there about the interaction between the children of the Latin American elite and an archaic, socially insulated institution—the Latin American university—that

regularly produces a significant number of students opposed to both their governments and the social structures that have and will so richly benefit them?" (pp. xxiii-xxiv) In the first chapter Liebman and Walker then furnish a concise historical overview of the manner in which student attitudes and behavior in the political realm have been "profoundly affected by the values and forces coexisting within their universities and societies as well as by the tensions emanating from the interrelationship between these two overlapping spheres" (p. 1). Here the scope is regionwide, with particular attention given to Argentina, Peru, Cuba, Venezuela, and even Brazil in passing, as well as Colombia as the only one of the countries included in the survey work to figure in this background discussion of basic trends.

The empirical body of the study begins with an analysis by Glazer and Liebman of social characteristics and career orientations using data of the mid-1960s and embracing all of Latin America. With university enrollment in the area low, but rapidly rising, they find a sharp urban-rural discrepancy and a high degree of selectivity by social class accompanied by underrepresentation of females. Similarly, conventional wisdom is borne out concerning relative overconcentration on law and medicine as well as the tendency of higher status students to plan careers in business while lower status classmates look to the employment opportunities afforded by the government. Glazer's exploration of "University Problems and Students' Attitudes Toward Higher Education" paints a picture of discontinuous professional socialization in an environment of inadequate facilities, low academic standards, and persistence of traditional teaching methods. Advanced students in particular express a longing for more courses stressing practical application of their professional skills. Centrality of professors to the students' evaluation of the total university experience and the dependency of the universities upon national governments are perceptively discussed. Blame for the politicization of the universities is transferred largely from the students to the system, and evidence of serious student concern for educational quality is introduced.

Variations between countries and among universities are emphasized in Walker's analysis of "Family Background, University Experience, and Student Politics" (pp. 89-126). Level of satisfaction with university life is not seen as an accurate predictor of student political activism or radicalism. On the question of the relative significance of recruitment and socialization

factors, he finds strong family background influence on student political orientations, with choice of career chiefly ratifying political predispositions. Liebman treats conservative students at length, having found the left to be a "small minority" whose political successes within the university reflect both the apathy of the centrist plurality and the reluctance of the rightists to work as intensely at politics as their ideological rivals. He also perceptively compares the national context of student politics in Mexico and Puerto Rico, providing excellent appreciations of the 1968 riots in the former and the 1969-1970 disturbances at the University of Puerto Rico. Indeed, the richness of this very contemporary case material highlights several of the study's weaknesses, while the concluding chapter brings the reader up to date on student politics in Cuba, Chile, Argentina, and in a sense Brazil rather than drawing upon the six nations empirically studied.

While my overall reaction to this volume is a very positive one, I do lament the failure to include more significant cases than its relatively limited range: three of the smallest and least typical countries of the region along with the second and fourth most populous and Puerto Rico, formally still a noncountry albeit of particular current concern to the United States. The omission of Brazil and Argentina is to be regretted the most, since useful efforts to conceptualize Latin American politics must include these two major nations. (As indicated by the questionnaire preface on page 237, Brazil was included in the original research design but subsequently dropped.) In their repeated references to Brazil the authors have chosen to rely upon only one of the three or more North American dissertations available on student politics there and to ignore non-English language materials on the subject. With very rapid changes having taken place in Brazilian higher education since 1969, they mar their study by continuing to view the situation in the early 1970s as a linear projection of policies of an administration which was out of office a year before Allende came to power in Chile.

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Israel's Parliament: The Law of the Knesset.

By Eliahu Likhovski. (Oxford: Oxford University Press, 1971. Pp. 236. \$13.75.)

Anyone who has observed the Israeli political scene knows only too well that it is not necessary to become theoretically involved in order to appreciate the intense centralization of polit-

ical power in Israel. Indeed, the more astute would remark that viewing the entire Knesset, rather than the more exclusive Cabinet, as supreme, gives too broad a base to the ultimate source of political power.

Likhovski is, however, most assuredly aware of these all too easy and frequent observations about the cliquish nature of Israel's political leadership. The fact that the present government leaders were the founders of the state in 1948, and the founders of all important institutions of the Israeli state and society since the early 1920s, cannot escape his consciousness.

It is, in fact, this very intimate connection between Israel's present and its own past foundations that provides the threads with which behavior and intentions can be woven into this closely argued treatment of constitutional development. Since the authors and the practitioners are nearly identical, we have a rare opportunity to gauge intentions and actions against each other in a contemporary setting.

Likhovski finds constitutional-legal principles in two streams of influence in the developing Israeli State. The first of these is the Zionist practice, which was the prestatehood manifestation of the political relationships that obtain to this day in Israel. Although appearing to be a consequence of the second stream of experience, the British Constitution, these political relationships are uniquely and directly a consequence of Zionist practice; Likhovski maintains that, in fact, the Zionist experience established Israel's particular set of common laws which, when subjected to proper exegesis, reveal a richness of precedent and an internal consistency and logic that allow Israeli practice to represent itself.

The desire to continue the Zionist modes in the new state was a matter of pride, identification, and practicality. Viewing Israel as the fulfillment of their own definition of Jewish statehood, those present at the founding were not likely to deny the heritage they had themselves produced. Equally important, of course, was the ambition of the founders to maintain their positions of formal leadership while effecting a rapid transition from mandate to independence under the conditions prevailing in 1948.

However unique and self-satisfying this Zionist heritage may be, it produced the more static, definitional aspects of the Israeli constitution. More dynamic is the legal system and the as yet unsettled relationship between the legislature and the judiciary.

The prestatehood legal system was mostly British, having been imposed during the time of

the Mandate: it was adopted wholesale at the time of Israel's independence. With this adoption, the Israeli Court inherited the right to review administrative action. But an unwillingness to adopt the whole of British constitutional practice, as well as to formalize relationships in a written constitution, left the Court with some latitude to find its own place; ultimately, this could include the judicial review of legislation.

Thus, Likhovski implies that the supremacy of the Knesset need not remain absolute, and he finds the *Bergman* case (1969) to be, possibly, Israel's *Marbury vs. Madison*. Implicitly, the American experience meets the Zionist and the British in the unfolding of Israel's unwritten constitution.

In his treatment of this meeting of streams, Likhovski brings a subtleness and sophistication that makes his subject almost dramatic. While remaining objective, the analysis offers a useful handle for those who are concerned about the high concentration of power in Israel. Although, as Likhovski points out, the Court has shied away from the full implications of *Bergman*, its potential remains.

In the confines of its restricted mandate as a constitutional-legal exegesis, *Israel's Parliament* is an excellent book. We may only fault Likhovski for providing too narrow a framework for his analysis, which is merely to suggest that the limits he set may not be the true limits of the topic. Thus, while we can appreciate his desire to avoid the strictly political, Likhovski can be expected to test his skills in those areas where his interests meet the more expressive elements which have affected Israel's constitution.

In particular, it is surprising that the Jewish religious-legal tradition was not treated as an explicit stream of influence, for it is certainly the wish to avoid an outright civil-religious clash, as well as the legislative-judicial issue, that prevents the writing of an Israeli constitution. Equally true may be the notion that judicial unwillingness to be fully drawn into the question of legislative review is based on the fear of becoming too involved in questions of personal status and the representation of the state in religious doctrine.

Here we discover that our questions require more explicit behavioral treatment. Although most obviously evident in the sphere of religion, it is also in matters of political representation and access to decision makers that we become aware that constitutional development involves the relationship of the State to its constituents. If Likhovski's analysis falls short of

our desires in this regard, it is possibly because the basic developmental mappings of Israeli political behavior remain to be completed.

MICHAEL KAHAN

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Classes, Crises and Coups: Themes in the Sociology of Developing Countries. By Peter C. Lloyd. (London: MacGibbon & Kee, 1971. Pp. 223. £2.95.)

As befits a volume in a series entitled "Sociology and the Modern World," this is a book primarily for sociologists and restricted, as the author acknowledges on page 199, "to a narrow framework of analysis, focusing the discussion on the emergent patterns of social stratification in the poorer nations." But Dr. Lloyd's earlier books have shown that he is also a highly sophisticated political scientist despite his professional label as a social anthropologist.

The main title of this book is somewhat misleading as there is relatively little about crises and coups, and class is discussed mainly in connection with the differentiation between elites and the masses. Even though there are only eight chapters in total, the structure of the book falls into three distinct sections: four brief chapters discuss the impact of the West on traditional societies; three more discuss social stratification with respect to the changing rural scene, among urban workers, and within "the men at the top"; finally a long last chapter entitled "crises and coups" deals briefly with revolutionary change and the role of the military and then provides extended examples in the form of case studies of Nigeria, Columbia, Cuba, and Tanzania.

There is much that is valuable in each of the three sections. Dr. Lloyd writes well, and his succinct style is unencumbered by academic apparatus, with a bare minimum of footnotes to distract the reader. Although the last two sections contain little that is original, the general reader will find a great deal of material which might otherwise have remained buried in the scholarly monographs of the specialists. On Latin America and India, there will doubtless be points where Lloyd's uncritical acceptance of the conclusions of another writer (like Dix on Columbia) will be challenged by others; on his home ground of tropical Africa, Lloyd's judgment seems impeccable. The only major criticism would seem to be the continuing assumption that "the masses" are capable of exerting pressure on the various ruling elites through the use of the franchise. "The masses" are never given a satisfactory definition, nor is

sufficient account taken of the capacity of elites to maintain themselves in power by controlling key groups rather than relying upon majority support.

One of the reasons for this analytical weakness is that Lloyd's "men at the top" in chapter seven are members of the civilian political elite and distinct from the military leaders discussed in chapter eight. And this final chapter is perhaps the weakest in the whole book. It points out but does not resolve any of the contradictions and limitations which result from the imperfect state of knowledge about why coups occur and how they are related to other factors in producing violent crises within new states. The book tails off in the last thirty pages because the case studies never get back to grips with the dominant themes enunciated earlier. The data are interesting in themselves but don't add up to much or lead anywhere in particular.

This sense of anticlimax is largely a product of declining tension because the first section of the book reached such heights. The first four chapters are densely packed with interesting information, almost all critical of the "conventional wisdom" on the subject of the process of modernization. Lloyd sets out to justify his skepticism of attempts to describe modernization as "stages through which all societies pass by processes of unilinear evolution" (p. 18). Against these universalist, evolutionary assumptions, Lloyd argues that "social change results from the continual interaction between individuals and groups seeking, through their use of existing resources and new opportunities, to improve their position in the social hierarchy" (p. 18).

He attacks theorists of modernization from Lerner to Hagen and McClelland because their notion of a single process implies evolution toward a common goal by a simple, inevitable route. Exposing a number of fallacies in this approach, Lloyd is especially critical of the ethnocentric assumptions which underlay it:

Implicit in the evolutionary approach is the belief in the increasing transferability of social institutions; as countries adopted the technology of the western nations so they adapt their own social structures to resemble those of the West. This widely held viewpoint is far more deterministic than anything postulated by Marx. . . . It seems to be confidently assumed that the rest of the world will not only become like the United States but that it wishes to do so (pp. 68-69).

In building up his own approach, Lloyd denies that societies progress along a predetermined continuum because change is seen to relate to the activities of various individuals, each

pursuing a conception of his own interest. The most suitable theoretical framework is seen to be that of games theory:

Individual interaction is seen as a game in which the basic elements in decision-making are the resources of the individual: in terms of wealth, education, skill, personality, etc., and his expectations of the reactions of others, in terms of the sanctions of reward or punishment that they can or will wield over him . . . in this context, values are not a determinant of behaviour but a weapon used by contestants . . . the social process, defined in these terms as a game, or series of games, between individuals does not predict any specific direction of change, nor indeed does it predict that change will occur (pp. 76-77).

Lloyd is doing a valuable service to all students of political science by casting doubt upon the utility of grand theories and arguing strongly for an approach which requires first an increase in the basic ingredient of any food for thought—empirical data—before any general intellectual advancement is possible.

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The State of the Nations: Constraints on Development in Independent Africa. Edited by Michael F. Lofchie. (Berkeley and Los Angeles: University of California Press, 1971. Pp. 305. \$10.00.)

This collection of essays puts forward the organizing concept of "constraint" in order to evaluate political change in Africa. The idea is to point to overall conditions and circumstances—largely inauspicious—in which Africans assumed responsibility for national political units. Rather than dwelling on conceptualizations of development or decay, which can lead to false hopes of generalized categories of explanation, the effort here is to assume a more modest stance, to "illuminate processes and problems" (p. 4). The editor and several contributors agree with the suggestion, familiar to Africanists in political science, that researchers in Africa in the late 1950s and early 1960s created a sense of purposive design in political transition, built around parties, "mobilization," and democracy. Professor Lofchie articulates the contemporary mood when he expresses "qualified pessimism" (p. 261) regarding the present outlook for policies of social and institutional change in Africa. (While one hopes for some cumulative knowledge based on scientific criticism in political studies, one can't help suspecting that the political analysis of Africa in the past decade or so has had as much to do

with the state of American political science as with African politics. Although this volume reflects the present climate of retrenchment, Lofchie's own commentary is so reasonable and even wise, that the reader wishes it appeared earlier so as to save us much confusion.)

Like most collections, this one is marked by unevenness in the quality of contributions and discontinuity in some of the themes pursued. Rather than catalogue probably inevitable differences of scope and level of analysis among the articles, I will merely note apparent contradictions that strike me as fundamental. For instance, Immanuel Wallerstein summarizes the differences in the political character of African regimes in terms of the conservative-radical split, while Henry Bienen on parties argues that these distinctions are unhelpful. Rupert Emerson on the prospects for democracy avoids the use of these terms. Barbara Callaway and Emily Card, writing on Ghanaian development, conclude that Nkrumah wasn't socialist enough and the CPP wasn't disciplined enough to chart new directions. But Bienen, working with the concept of the political machine as a regime model, undermines the notion that leadership and parties in Africa have the resources to make and implement these kinds of choices.

Perhaps more than in most collections, the editor's introductory and concluding essays seem most rewarding in this one. Although Lofchie makes an admirable effort to incorporate the discussion of his contributors into his own presentation, his major analytical themes do not especially depend on the articles in the book. That is not to say that the six case studies in the section called "parameters" are without value to the reader. Jonathan Barker's use of Senegalese politics to illustrate what he calls "the paradox of development" seems cogently suggestive: a regime that presses hard with a policy of economic development will undermine the basis of its initial support. Cranford Pratt's description of the workings of the core of the Tanzanian executive during the first six years of independence seems genuinely revealing of administrative and political problems from the inside. But the significance of Lofchie's overall supervision here is to put the question of Africa's political development into a comparative historical context and then to suggest some connections between major social forces and public policy.

Lofchie starts by discussing the "timing of technological change" (p. 15) rather than colonialism. As a latecomer to industrialization and modernization, Africa is subject, he suggests, to certain economic and social dependencies

which make it impossible for a given country or even the entire continent to influence change decisively under present conditions. Political choices available to European countries several hundred years ago are simply ruled out for Africa. Basic issues that were managed "in series" in Europe—constitutional matters, the state and the economy, popular participation in government, social welfare—now occur simultaneously in Africa. The political expectations and moral understandings of today do not permit the sort of deprivations endured by the lower classes a century ago. To these general points, derived from the theorists of constitutionalism and comparative politics of the postwar decades, Lofchie adds the observation of political economists that sophisticated technology is a poor instrument for stimulating employment. So the process of growth itself widens the gap between elite and mass.

Most of the contributions to the book reflect Lofchie's view that the major questions for policy and study revolve around development planning and the emergent social structure. Some interesting dilemmas appear. Most actual policies rely on foreign aid, so the debate between advocates of equality and advocates of growth may take on an artificial quality, especially when planning experiences are investigated. In addition, there is an assumed need for the state in development and there is an obvious weakness in state institutions. This political context is vulnerable to military intervention, which may overcome some new cleavages, but which may also intensify older ones. In terms of policy problems, Lofchie summarizes two basic contradictions. First, there is the attempt to transform the society at the same time as to represent all elements within it. This suggests the second, the attempt to create an egalitarian society in the face of the inequalitarian consequences of modernization.

The major theoretical suggestion that emerges is that political conflict is best understood as reflecting social rather than cultural tensions. Lofchie sees development strategies linked to the effort to redress "proportionate inequality" (p. 274), i.e., disproportional economic advantages for regional-ethnic groups. But such efforts are seen as mistaking apparent conflict for real cleavages, class differences that promise to be more severe than in previous Western history, because of the timing and conditions of social change, already mentioned.

Lofchie's contribution deserves expansion, refinement and reprinting on its own, as an effort in political theory in a tradition somewhat overlooked in comparative politics in recent

years. It suggests a new look at the constitutionalism literature, tempered by neo-Marxian sociology and perhaps measured by the concerns that animated the casebooks in "public administration and policy development." For research in Africa, it would appear that political economy is yet another new frontier. Equally important, the thesis sketched here seems an exciting tool with which to teach—viewing African political change in the context of historical sociology.

HARVEY GLICKMAN

Haverford College

Guatemala: The Politics of Land Ownership.

By Thomas and Marjorie Melville. (New York: The Free Press, 1971. Pp. 320. \$9.00.)

In January, 1967, readers of *The New York Times* were, I suppose, at least mildly shocked by the report that a Maryknoll priest and a nun were expelled from their order and subsequently from Guatemala for involvement in a plot to smuggle arms to Castroite guerrilla organizations there. In the ensuing years the ex-missionaries have led extremely active lives in the United States. They married, were convicted (with the Berrigan brothers) of burning draft board files, pursued graduate studies and produced the book which is the subject of this review.

Being aware of the writers' background and having first seen the book under its British title, *Guatemala: Another Vietnam?* I was anticipating yet another in the long list of polemical treatments of the Latin American scene. What I found, however, was that, whatever their disclaimers, Thomas and Marjorie Melville have written a remarkably straightforward, well-researched book on the last twenty-five years of Guatemalan politics.

The book recounts in detail the attempts by Presidents Arevalo (1945–1950) and Arbenz (1950–1954) to create a more equitable and productive land holding system for the country. The basic purpose of their policies was not to socialize agricultural production but to realize an objective sought by various Guatemalan regimes since *La Reforma* (1871), namely, to force the owners of *latifundios* to manage their properties as capitalistic enterprises.

Although definitely favoring the infamous Decree 900—the Arbenz agrarian reform law—the writers do not hesitate to point out its defects: vagueness in its definition of uncultivated land, the lack of representation by landowners on the local agrarian reform committees, the lack of adequate appeal procedures, and the

failure of the government to control illegal peasant invasions. The authors point out that those owners who had been most generous to their workers and had provided them with larger and better plots stood to lose the most from expropriation since the law stipulated that such property was to go to the workers (p. 67).

The Melvilles see Castillo Armas (1954–1957) as a tragic figure. A true Cold Warrior, he saw in the Arbenz approach to agrarian reform the destruction of private property, the dissolution of social peace, a decrease in agricultural productivity and the "sovietization" of Guatemalan society. Yet they also document the fact that he was not totally oblivious of the need to provide social justice for the peasant. However, it was to be in the form of colonization rather than land redistribution.

The remainder of the book describes the abortive attempts of successive administrations to deal with the agrarian problem in the face of increased politicization of both the left and the right. The social forces set in motion by the Arevalo-Arbenz reformist regimes produced most of the problems usually associated with policy making in underdeveloped societies: problems such as coalition maintenance, the "motivation-outruns-understanding" style of problem solving, hyperpoliticization, etc. Thus it was that, although "under Mendez Montenegro (1966–1970) the number of labor syndicates and rural cooperatives increased impressively" (p. 243), his government had "the dubious distinction of awarding lands of insufficient acreage to 5,000 out of the 80,000 landless families that [had] proliferated during its four year regime" (p. 230). And all of this accompanied by "over a hundred political murders a month . . ." (p. 247).

As political history and as a case study of regime performance in a specific policy area, the study is a very useful addition to the literature on contemporary Guatemala. But, although passing reference is made to the political fragmentation of the military and the growing social consciousness of the clergy, the analysis is clearly based on the traditional oligarchic model of the Latin American polity. While reasonably accurate for Guatemala, the oligarchic model does not provide a totally valid perspective. What the authors have failed to do is to relate the land tenure problem to such phenomena as industrialization, urbanization and internal migration, all of which have the effect of redistributing power and wealth. It is in these areas that considerable social change is taking place and can ultimately be looked upon to

produce political change as well. Thus, although the Melvilles believe that land tenure is the country's major problem, Guatemala is largely past the point at which agrarian reform can be viewed as a panacea.

ROLAND H. EBEL

Tulane University

Plan and Market in Yugoslav Economic Thought. By Deborah D. Milenkovitch. (New Haven: Yale University Press, 1971. Pp. 323. \$10.00.)

Quite a few books have been published on Yugoslav economic reforms, but this one by Mrs. Milenkovitch, Assistant Professor of Economics at Barnard College, is the best overall treatment and evaluation of the theoretical issues involved. It roughly follows the historical sequence of the reforms from the late 1940s to 1970. While it is based on primary sources, Marxist concepts are adapted to Western terminology, and it is scholarly and objective without being either excessively critical or unduly enthusiastic. The study is a useful contribution to the systematization of our knowledge of what undoubtedly is one of the most intriguing experiments in social history. Those who have been yearning for an ideal economic-political system—which neither capitalism nor socialism on its own has yet produced—will find the book most stimulating.

In contrast to the traditional Marxist approach, plan *versus* market, the Yugoslav economic version of socialism is the complementarity of plan and market. In the author's words,

Plan is used here to mean central determination of what and how to produce. In physical analogy, planned decisions are made from the mountain top. From the peak it is possible to survey the entire area. Market here means that decisions about what and how to produce are made from the viewpoint of the primary economic unit. Following the analogy, market decisions are made in the valley (pp. 2–3).

The reasons for the reforms are discussed at length. On the economic side, the author points out that centralized, directive planning and management may be desirable for an underdeveloped country seeking rapid development, but in higher stages of economic development the market mechanism works more efficiently. However, it is political circumstances—the strength of the Tito regime and the expulsion of Yugoslavia from Cominform (in June 1948)—that made the reforms possible, as well as necessary.

In the process of the reforms, Professor Mi-

lenkovitch tells us, the three elements normally associated with the socialist economic system—production planning, investment planning and the social ownership of the means of production with the distribution of income according to work have been virtually abandoned one by one. Yet most Yugoslavs believe that the reforms do not represent retreat but rather advance on the road to communism.

In the reviewer's opinion, the feature of greatest interest to economists and political scientists alike is the workers' management of enterprises combined with the exclusive rights of use of productive public property. Although this feature will, no doubt, please many supporters of the syndicalist movement, the Yugoslavs maintain that the resemblance is misleading and they deny any connection with syndicalist concepts or dreams. The Yugoslav answer to the workers-management-owner problem brings to mind the Japanese approach, hinging on paternalism and loyalty for its success. Many observers believe that Yugoslavia and Japan have evolved better solutions to the problems of industrial relations than other socialist or capitalist countries have. One is tempted to speculate if the Japanese approach is tenable in the long run, when the economy reaches a more mature stage, the power of traditionalism weakens and trade unions become more powerful and militant.

Although Yugoslavia may have produced a lasting answer to intra-enterprise industrial relations, has she made much progress in solving broader issues of social justice? Not according to the author, who highlights a growing division between the privileged and the underprivileged, and she believes that "the have-nots, if brought together, could form a particularly powerful alliance against the Establishment . . . The Belgrade student uprising of June 1968 may have been a harbinger . . ." (p. 289). The author concludes the book with the following sentence: "The ultimate consequences of these reforms are not yet known but the Yugoslav experience suggests that the results may be more far-reaching than the reformers either intended or anticipated" (p. 300).

The book reads well, with arguments clearly and smoothly developed, but one cannot escape the impression of repetition. The description of the Soviet system could be omitted, and the account of the Soviet reforms is rather trite. Many a reader would instead prefer at least some brief comparative references to the ideas put forward by such Soviet and Eastern European writers on market socialism as F. Behrens, W. Brus, P. Erdős, G. S. Lisichkin, G. Petrov

and O. Šik. This does not detract from the main object of the study and it will be welcomed by many social scientists as an example of mature scholarship.

JOZEF WILCZYNSKI

University of New South Wales

The New Order and the French Economy. By Alan S. Milward. (Oxford: Clarendon Press, 1970. Pp. 320. \$12.00.)

The organization of conquered economies for the German war effort during World War II was largely the work of Hitler's brilliant favorite, Albert Speer, the Minister for Armaments and War Production from 1942. Although Milward has not used Speer's memoirs, he has sifted the available ministry records in the Foreign Documents Centre and also consulted the materials in the Bundesarchiv which received the Speer files captured by American forces. Most of this material was also microfilmed and the films are available in the U.S. National Archives. As I edited the material for filming I can add to Milward's exemplary bibliographic note that the unfiled photostats were not sufficiently legible and that these and materials used for the Nuernberg Trials are still in the U.S. National Archives. None of it, however, would affect his excellent account of the French economy in World War II.

Milward's history emphasizes some of the salient points made by others of the few authors who have tried their hand at World War II economic history and policy. Like Homze (*Foreign Labor in Nazi Germany*, 1967), Milward makes clear the very deep division between on the one hand the new technocrats epitomized by Speer and the generation of young men educated in the Nazi era—and on the other, old party hacks like Gauleiter Sauckel (chief of labor conscription), who held to the raw revolutionary ideals of early Nazism and its attack on capitalism, Jewish capitalists, interest rates, etc., as summarized in the party platform of 1920. Milward's view that the National Socialist revolution was a "last stand" against capitalism and industrialization is a point which is perhaps exaggerated. Hitler was himself a great admirer, almost in a Social Darwinist sense, of the successful modern entrepreneur, the captain of industry. The hostility to big business had of course characterized the Storm Trooper (S.A.) wing of the party which Hitler had subdued in the Blood Purge of 1934. It also reflects the provincialism of German society as German industrialization was primarily decentralized and Germany lacked the cultural strength of modern urbanization.

Like B. Carroll (*Design for Total War*, 1968), Milward devotes a long introduction to the problem of war—Carroll to "total war" and Milward to the "idea of conquest." Introductions of this sort usually assume too much tradition for the subject at hand. Milward also finds a long tradition of "liberal" antagonism to conquest in the political thought of the sixteenth through nineteenth centuries but describes only those authors who agree with his thesis. Most of the leading political writers of these centuries from Machiavelli on were not at all opposed to conquest and war. The rights derived from conquest were even an integral part of the prevailing international law of the seventeenth and eighteenth centuries. The liberal dislike of war and the argument of the high cost of conquest arises in twentieth-century sociology as well as in Adam Smith, but its strength derives from the public reaction against World War I. The prevailing tradition was favorable to ideals of war and conquest and the limited education of the Nazi leadership provided no opportunity for imbibing any but the older traditions. And if, as Milward points out, Göring believed that the object of conquest was booty and did in fact order the systematic looting of France, he was consciously following ideals of warfare which had been accepted in Europe for centuries. He sounds little different from Wallenstein and Gustavus Adolphus whose conception of warfare was similar. Unlike Wallenstein, however, Göring resisted the idea that a conquered area could be made more prosperous and could serve the ends of future wars. Speer seems to have been able to persuade Hitler of the need to utilize the French economy for the effort of total war which the failure of the Blitzkrieg in Russia made necessary. Surprisingly, Hitler and the party faithful like Sauckel continued to resist the full implications of Speer's conception of rationalizing production. During 1942 and 1943, however, some production was concentrated in France, where labor productivity rose despite sabotage and despite Sauckel's heavy labor draft.

Although the French economy was not systematically plundered, it was despoiled. Inflation, the Black Market, shortages of fuel, agricultural deficits, marginal mining, the heavy occupation costs left their mark. Nonetheless, as Milward points out, Germany became increasingly dependent on France and the Benelux countries, and the pattern of her trade which had been oriented towards eastern and south-eastern Europe was now shifted to the west. Here, then, was the foundation of the present common market.

German cartels, particularly I. G. Farben, also began to buy into French industry, thus seeding some of the roots of the greater integration which emerged after the war; but the account is rather too skimpy, and Milward ought to have researched the microfilm of records of captured firms and the archives of the major West German firms, which are also accessible. The N.S. government certainly encouraged German firms to acquire control of French and Belgian cartels, particularly after 1941. Whatever one's criticisms, however, this monograph establishes Milward as the leading English-language historian of National Socialist economic history.

HELEN P. LIEBEL

University of Alberta, Canada

Studies in Nigerian Administration. Edited by D. J. Murray. (London: Hutchinson Educational Ltd., 1970. Pp. ix, 324. \$11.50.)

This collection of essays is one in a series of publications sponsored by the Institute of Administration at the University of Ife in Nigeria. The series, which also includes various monographs and the *Quarterly Journal of Administration*, is inspired by legitimate concerns and pursues laudable objectives. As the Institute's Director, Dr. Adebayo Adedeji, makes clear in a prefatory note, public administration—a key factor in the country's development process—has too long been neglected by local and especially by foreign scholars. What researches have been undertaken thus far are usually buried in unpublished doctoral dissertations or in foreign journals—out of sight of Nigerian policy-makers who might (should?) derive some benefit from them.

Adedeji has touched a nerve here, one which impinges on the whole range of social research in Africa. Addressing the matter more generally, a former graduate student of mine (from Nigeria, coincidentally) observed as follows in a seminar paper:

It is foolish to search for the culprit in a single academic discipline. Anthropology, for example, as is now the fashion. Or in particular subjects of inquiry . . . tribal lore or ethnography, as cases in point. Or even in the national origins of research scholars. The problem transcends all these, and particular territories as well; it is continental in scope, perhaps universal. And it is at once moral, philosophical, and practical. The 'social research enterprise' in Africa, still dominated (but, it should be noted, not monopolized) by foreign institutions and operatives, is essentially an extractive industry, along with tin, gold, petroleum, etc. At least the latter three can lay claim to creating jobs, wages, and consumer goods

to use. But what of social research in Africa, whether pursued by African scholars or by foreigners? What does that enterprise create, and what does it diffuse? Knowledge? Of what? To whom? And in whose interest?

A raw nerve, indeed!

Commendably, each contributor to *Studies in Nigerian Administration* has accepted the challenge to tailor his research to African needs and aspirations—particularly with regard to public administration in Nigeria at the national and the state (formerly regional) levels. Each has undertaken to examine critically selected aspects of that country's administrative life—during the colonial era and in the first decade of independence. Sadly, their efforts here have not been very impressive.

Eight essays range widely across the administrative landscape in Nigeria, from national apex to village base—albeit without coherence around either a theoretical perspective or a set of organizing policy questions. The editor, Dr. Murray, has refrained from "putting the whip" to his colleagues and to himself; his introductory essay, a superficial discussion of the evolution of administrative policy in Nigeria, anticipates the disjointedness of what follows: a "comparative analysis" of high ministerial organization in that country and the Ivory Coast (by L. Adamolekun); a discussion of accountability and public control of statutory corporations in Western Nigeria (R. O. Teriba); a "comparative analysis" of administration at the field level (D. J. Murray); reflections on the executive class in Nigerian administration (F. J. Fletcher); a survey of the public service commissions (O. Nwanwene); an empirical study of work habits and motivation among village level agricultural extension workers (R. K. Harrison); a review of personal income-tax administration in Nigeria (G. M. Walker, Jr.); and finally, a "role analysis" of Nigeria's higher public servants from the vantage-point of that country's Western-educated elite (R. L. Harris). With the exception of Harrison's piece, which draws on the literature of organization theory and industrial psychology, the essays are dry, tedious, and often banal.

If there is any single theme (mood?) which may fairly be said to unite most of these contributions, it is this one: that Nigerian public administration in practice has widely failed to mirror its English counterpart in theory. Various essays manifest a preoccupation, either implicit or explicit, with that "reality": Nigeria's executive class, beset by role ambiguity, is disappointingly unlike that found in the United Kingdom; the public corporations are exces-

sively politicized; higher public servants are, unfortunately, more loyal to family and tribal constellations than to the civil service ethos; and so forth. The inappropriateness of *selected aspects* of the English model under Nigerian social, economic, and political conditions is observed by some contributors, hinted at by others. But nowhere in the volume is there to be found a critical examination of the entire model as such or any consideration of alternatives to the Weberian rational-legal perspective. The lesson may be a hard one for many Western and Western-trained scholars to learn, but learn they must: that alternative models for organizational life are not only conceivable but already available, and that these models may be more appropriate to conditions in Nigeria and the so-called "Third World" generally. The Soviet and Chinese alternatives come quickly to mind, the former suggesting, for example, that role ambiguity, to the extent that it places a premium on adaptive administrative behavior, may be highly functional to the attainment of organizational and national development goals. (In this regard, see Jerry Hough, *The Soviet Prefects: The Local Party Organizations in Industrial Decision-making*, Harvard, 1969.)

Since independence in 1960, Nigerian political life has been dominated successively by the party and the military factors—political parties prevailing up to 1965, the military thereafter. Those realities, it can be argued, are at base incompatible with key assumptions in both the English administrative model and the Weberian ethos. Except to vindicate the transcendent merit of Nigeria's colonial legacy, it seems pointless to belabor, as have most contributors to *Studies in Nigerian Administration*, the hiatus between theory and practice. Policy-oriented research organized in the future under the auspices of the Institute of Administration at Ife might contemplate that observation as it seeks to assist the country's development process.

ALVIN MAGID

SUNY at Albany

**West African States and European Expansion:
The Dahomey-Niger Hinterland, 1885–1898.**

By Boniface I. Obichere. (New Haven: Yale University Press, 1971. Pp. x, 400. \$15.00.)

Colonial expansion in Africa has spawned divergent explanations. Mixed motives marked the acquisition of territory—economic gain, both as markets and as sources of raw materials; philanthropic and cultural objectives, such as the "civilizing mission," Christian proselytization, or stamping out the slave trade the Eu-

ropeans themselves had earlier fostered; and transference of continental rivalries to the checkerboard of tropical Africa.

Where does Obichere fall in the still inconclusive debate among historians about the causes of imperial conquest in West Africa? To him, acquisition resulted primarily from "the desire to ensure the economic viability of those coastal possessions whose commerce and prosperity depended on an uninterrupted flow of trade with the hinterland" (p. 3). He seeks to explain not the initial European implantation along the coast, but the frenzied period of treaty-signing and border-delimiting of the 1890s. This viewpoint represents a clear twist on the monistic, economic determinist arguments strongly represented in the literature.

My disappointment with this book certainly does not arise from the assiduity of research and the extent of documentation: 250 pages of text account for no less than 1,561 footnotes. But the citations come from standard archival sources—the French Ministère des Affaires Étrangères, the British Foreign Office, the private papers of Lord X and Monsieur Y. The author, an African as well as an Africanist and historian, has followed the well-trodden scholarly paths too faithfully. The story of the impact of colonialism remains untold, or viewed through the lens of Europeans, for rarely do the sources Obichere cites provide a rounded view of the initial effects of conquest.

The thesis linking expansion with the economic viability of coastal comptoirs is provocative, but not fully sustained by the evidence cited. It does accord with the well-known reluctance of the British government to undertake new colonial responsibilities; the flag followed trade, and the Colonial Office refused to accept any new protectorates until chamber of commerce pressure became "irresistible" (p. 142). The main competition for territory was carried out by hyperambitious captains and lieutenants, not easily restrained by instructions from London or Paris, nor, it can be surmised, by careful consideration of the economic viability of the kingdoms they were bringing under European aegis. The key to the frenetic treaty-signing lay not on the West African coast, but in European capitals. The negotiations that settled the major Anglo-French disputes testified to "reasons of European diplomacy and balance of power" (p. 219); "The consideration of the European balance of power as well as other international issues were also important in deciding the questions in west Africa" (p. 241).

The historian of Africa will thus find *West African States and European Expansion* a guide to cabinet perceptions and military ad-

ventures in the closing years of the nineteenth century. Obichere has carried out an extraordinary amount of archival work; it will be the task of other historians to determine how the information he provides accords with the various explanations of the scramble for Africa.

CLAUDE E. WELCH, JR.

SUNY at Buffalo

Political Development in Changing Societies.

Edited by Monte Palmer and Larry Stern.
(Lexington, Mass.: D. C. Heath and Co.,
1971. Pp. 179. \$12.50.)

Subtitled "an analysis of modernization," this volume consists of eight essays by such distinguished scholars as Manning Nash, Leonard W. Doob, Joseph LaPalombara, W. J. Siffin, and Joseph Spengler, among others. The essays were originally delivered as lectures at Florida State University. The editors have contributed a short introductory chapter.

Unfortunately, this collection suffers from many of the weaknesses of co-authored works, while it has almost none of the advantages. Each of the essays stands by itself, with little or no apparent relationship to the others. This is true even of the editors' introduction, which singularly fails to provide a unifying theme that might pull the disparate efforts of the contributors together. The essays, moreover, are highly uneven in nature. The first, by Manning Nash, describes the survival of traditional attitudes in the politics of the state of Kelantan on the East Coast of Malaya. Those who are familiar with Nash's work will not be surprised to learn that it is incisive and lively, although perhaps too brief. The second essay, by Doob, also excessively brief, presents some hitherto unpublished survey data from among highly educated Africans "who have experienced the disruption of the traditional patterns or who will be helping to disrupt them" (p. 17). Doob presents six hypotheses about change. They are of a very high order of generality and in themselves unexceptionable. Following brief elaboration on each of these, he presents a series of statements reflecting modern or "nontraditional" attitudes and the level of agreement or disagreement on each of these among various groups of his African subjects. He does not, however, correlate the responses of these subjects, nor does he, in fact, incorporate them into the body of his discussion at all. Moreover, the "modernity" of some of the statements is merely assumed or asserted without further elaboration. But surely such sweeping statements as "People are naturally good," or "The world is so dreary, that I feel sorry for children who are born into it" require some explanation, particularly in

terms of their relation to the phenomenon of modernization.

With one exception, the remaining essays do not deal with specific communities or research projects. LaPalombara presents a full-scale review of the state of the art of political science as it relates to modernization. In terms of comprehensiveness, this essay is on a par with Huntington's recent survey article ("The Change to Change," *Comparative Politics*, 3 [April, 1971]), though the perspective is somewhat different. Part of LaPalombara's discussion summarizes the essence of the seventh volume in the SSRC-Princeton University Press series on political development. Siffin's chapter similarly summarizes how the study of public administration has contributed to an understanding of the politics of development. There is some overlap between Siffin and LaPalombara; indeed, Siffin extensively reviews his co-author's thoughts on structural-functionalism, but without once referring to LaPalombara's contribution to this volume!

William Flanigan and Edwin Fogelman next present some preliminary results from an ongoing research project in which they correlate attempts to establish or maintain democratic regimes with such indicators as proportion of the population engaged in agriculture, and incidence of violence. Although they describe an Index of Democracy, they provide no elaboration. For example, such items as "no significant political suppression," and "no widespread electoral suppression" are not defined. More important, they do not indicate how they determined whether a particular democratic regime was "successful" or "unsuccessful," although the correlation of success with other indicators is a key element in their research. Finally, Flanigan and Fogelman apparently have not read LaPalombara's contribution to this volume either, for they show no awareness of his vigorous criticism of research based solely on aggregate data. This is not to say that no response to the criticism is possible. It is, however, jarring to move from such a critique almost directly to a prime example of what has just been debunked—with no explanation, much less rebuttal.

Joseph Spengler next examines the relative advantages of democratic regimes for purposes of economic development. Political scientists familiar with prevailing trends in the third world will note his emphasis on the desirability of "private foreign investment, especially direct investment" as an instrumentality; his admonition that "restrictions of foreign enterprise need to be minimized"; and that "especially useful to an underdeveloped country is the multinational

corporation" (p. 118).

Finally, John Armstrong reviews the experience of the Soviet Union as a pattern of modernization distinct from that of the West, and John Lovell summarizes the accepted wisdom on the role of the military in political development, with references both to policy making in Washington and to the evolution of theory among social scientists.

Aside from the total failure of the contributors to respond to one another, this collection also suffers from sloppy editorial work, as evidenced by a number of typographical errors. Nevertheless, several individual essays (notably those of LaPalombara and Siffin) may serve to introduce students to the subject.

FRANK TACHAU

University of Illinois at Chicago Circle

The Vietnamese in Thailand: A Historical Perspective. By Peter A. Poole. (Ithaca: Cornell University Press, 1970. Pp. ix, 180. \$8.00.)

This all-too-brief description of the history of the Vietnamese minority in Thailand includes considerable new factual information while at the same time leading the reader to wish that the author had expanded upon his material. Professor Poole discusses knowledgeably the two main groups that now compose the 70,000 to 80,000 Vietnamese in the Kingdom, the "old Vietnamese" who arrived at the end of the eighteenth century and have tended to assimilate into Thai society, and the "new refugees" who arrived after World War II and are themselves divided among first- and second-generation members. He notes the differences between the two groups in location, tradition, patterns of assimilation, and legal position. In this area he has broken new ground. To a great extent this book is concerned with a traditional tracing of the two peoples' histories and international contacts from the founding of their respective states to 1968. Emphasis is given to the efforts to repatriate refugees in the early 1960s and the failure to maintain the program because of growing tension arising out of Thailand's decision to support the United States in Vietnam after the Gulf of Tonkin incident.

The weaknesses of the book develop from its brevity and the difficulties Professor Poole had in getting into the newer Vietnamese community. In the first instance, one would wish for more information about and analysis of the social and political composition of this important minority and perhaps less discussion of the history and international relations of the area, much of it covered elsewhere. The very infor-

mative footnotes and equally fascinating material in the appendices could well have been expanded and included within the body of the text. These gaps in content and analysis may in part be explained by the author's self-acknowledged inability to talk to representatives of the refugees and Democratic Republic of Vietnam (he is to be congratulated for carefully listing the people he did interview, including Thai and South Vietnamese officials, Catholic priests, and a select group of old and new Vietnamese settlers and refugees). In-depth interviews with North Vietnamese and extended surveys of the Vietnamese community were apparently not politically possible, and the author himself notes that their presence would have strengthened the book.

In spite of these shortcomings, this work is the best contribution to our knowledge of the Vietnamese in Thailand since General Chan's narrowly circulated study a decade earlier. Professor Poole's tables and analysis of efforts to repatriate the refugees are of particular interest, and it is hoped that he will follow up this brief survey with a more extended analysis of a group important both to Thai domestic politics and the international peace of the area.

FRED R. VON DER MEHDEN

Rice University

Nationalism and Capitalism in Peru: A Study in Neo-Imperialism. By Aníbal Quijano. Translated by Helen R. Lane. (New York and London: Monthly Review Press, 1971. Pp. 122. \$6.50.)

The four years that have elapsed since the overthrow of Peru's President Fernando Belaúnde Terry have generated much speculation about the character and meaning of the revolutionary government which replaced Belaúnde in 1968. No small amount of the confusion about the significance of the new government's policies derived from the apparent contradictions in the government's actions. Thus the government expropriated the agricultural and cattle-raising estates of the Cerro de Pasco Corporation and the W. R. Grace and Company plantations, at the same time that it permitted the Southern Peruvian Copper Corporation to receive new mining rights and agreed to heavy new investments. Similarly, the installations of the International Petroleum Company were expropriated, while other important foreign companies were increasing investments and extending their concessions.

What do such tangled policies mean when considered in the context of change in Peru under the Velasco government, and in the broader

context of radical change in Bolivia, Chile, Cuba, and the rest of Latin America? Aníbal Quijano, a Peruvian economist who has worked with the Economic Commission for Latin America, attempts to explain these policies as a form of neoimperialism. While the military junta operates against the traditional patterns of foreign control of agricultural exports and enclaves in mining and petroleum, it strengthens the foreign role in mining and the urban-industrial sector. Thus it appears that the 80 per cent of industrial investment in Peru that is foreign will simply undergo a change in sectorial emphasis. Dependency will continue; but the state has determined to play a stronger, more efficient role in structuring the form of that dependency. The Peruvian government intends, as Quijano sees it, to strengthen its ability to intervene in and control economic events. Under the tutelage of the state, Peru will develop a regulated economy combining private, state, and cooperative enterprise. It is clear to Quijano (and the record bears him out) that the military junta has no intention of eliminating foreign investment, but it does intend to supervise it, and to insist on state hegemony in the new economic alliance.

Quijano observes also, despite apparent concessions granted to workers in the so-called "Industrial Community," that the new regime is strongly authoritarian-paternalistic in nature, and that many of the concessions are illusory, posing no real threat to capital. He concludes that the regime is guided by an ideology of "limited nationalism within the imperialist order" and also by an aim of class reconciliation.

Aníbal Quijano has provided a useful service in reviewing the economic policies and programs of the military government of Peru and in finding coherence in those policies. It is unfortunate that he lapses so frequently into ideological jargon which through overgeneralization obscures rather than clarifies his meaning. (e.g., "dominated groups," "dependent bourgeoisie," "popular masses," etc.) Nevertheless, this short book is a well-written, important analysis of a new style of military regime in Latin America, and it deserves careful reading.

JACK W. HOPKINS

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Scandinavian Political Studies. Volume 6/71.

A Yearbook Published by the Political Science Associations in Denmark, Norway, and Sweden. Edited by Erik Rasmussen and Erik Damgaard. (New York: Columbia University Press, 1971. Pp. 260. \$10.00.)

The 6th annual volume of *Scandinavian Po-*

litical Studies underlines the strong position of behavioral studies, focused primarily on electorates and elected bodies. But even if the revolution in American political science of yesterday remains the dominating style in Scandinavia today, the impression may be exaggerated, and for two important reasons. First, contributions to the *Studies* emanate mainly from those who regard themselves as members of a wide, international intellectual community. In this category, behaviorists hold a prominent place. Second, research in international politics is so far not represented in the *Studies*, except in its exhaustive annual bibliography. Since 1965 this field has been covered in another joint Scandinavian publication, the quarterly journal *Cooperation and Conflict*. Together, the two mentioned publications represent a significant part of the "international profile" of political studies in the "Nordic" area. It would be reasonable to argue that such a profile should be evaluated by an outside observer who cannot be regarded as part of that very profile. Since this is not the case, the reviewer is intensely aware of the difficulty and delicacy of his assignment.

The largest single political science research project so far in Scandinavia is the Swedish local government research project, started in 1965 and currently approaching its completion. (Cf. J. Westerståhl: "Swedish Local Government Research," *Scandinavian Political Studies*, 2, 1967, 276-80.) The project deals with what is probably the most sweeping local boundary reform in the Western world. One of the participants in this research venture, Lennart Brantgärde, has made an ambitious and sophisticated study on the behavior of the local councils facing local district amalgamations. It is an experiment in method; and as such at least a partial failure in view of the model's "relatively moderate power of explanation in terms of variance explained" (p. 82). Its ambition of offering information about the reform itself is very modest.

In this respect Brantgärde's study differs from that of three Danish authors (Mogens N. Pedersen, Erik Damgaard, and P. Nannestad Olsen) whose "Party Distance in the Danish Folketing 1945-1968" is not only a belated attack on the outmoded "undimensional model" of the party system but also is packed with information on Danish parliamentary party politics. The other Danish contribution, on "Saliency and Political Attitudes," deals with the old problem of using survey techniques for predicting future political behavior. The main preoccupation of the authors, K. W.

Redder and Ole Tonsgaard, is however to emphasize the need for a discussion on saliency as a neglected problem and to identify operational methods of measuring saliency. The authors are realistic enough to recognize that they are a long way from a satisfactory solution of this problem.

Phillip E. Converse and Henry Valen have collaborated on a major study on "Cleavage and Perceived Party Distances in Norwegian voting." Multidimensionality is taken for granted, but the number of important dimensions at any given time is limited, "if for no other reason than that of the limits of attention on the part of either the governors or the governed" (p. 209). The "left-right" and the "religious-moral" conflicts seem to emerge as the most salient cleavages in current Norwegian politics (p. 148). A historical comparison reveals further that the recently successful anti-EEC combination between remote rural interests and city radicals had a counterpart in a similar cleavage structure almost a hundred years ago (p. 150). Another Norwegian study on "Political Awareness and the Distribution of Other Social Resources," centers around the problem of structural opportunities for political participation and motivation for political activity. Willy Martinussen tests two hypotheses: that political interest, information, and activity in expressing opinions vary strongly among voters, and that as indicators of political awareness or competence they are interrelated (p. 154). His critical conclusions on citizens' opportunities to use their political rights are made, however, without any reference to the role played on their behalf by numerous interest groups, whose strength and competence may well balance the disadvantage of being less informed as individuals.

Risto Sänkiäho introduces a model of "populism," primarily as a tool for studying the Finnish Rural Party. Its usefulness is admittedly limited; "it cannot be used to explain the rise of populist movements in the United States" or in the Third World (p. 44). A not very successful attempt is also made to apply the model to the French Poujadist movement. Ilkka Heiskanen discusses "educative propaganda," defined as "any asymmetric, legitimate communication that is politically neutral (does not aim at change in the power relations) and aims at value changes that are corrective and complementary to the value system produced by the primary socialization" (p. 173). It is a controversial paper, finally posing the fundamental ideological problem of the communicator's

right to define "correct" change in values and behavior (p. 180-81).

Two contributions fall outside the major pattern of the *Studies*. Dankwart A. Rustow's thought-provoking study on "Sweden's Transformation to a Democracy; Some Notes toward a Genetic Theory" deals with the basic problem of what "brings a democracy into existence" (p. 12). To Rustow, "the Swedish experience suggests two major factors. The first is a systematic attack on the old system by well-organized forces—the attack including at least implied threats of violence. The other are a number of considerations of prudence that weaken the old elite's will to resist" (p. 21). Gunnar Heckscher represents in the retrospect at least as much as any other individual scholar the changeover from traditionalism to modernism in Swedish political science. Many years ago he abandoned the profession of political scientist for applied politics and public service. In the 1971 *Studies* the former ambassador in Delhi pursues his old interest in public administration in a shrewd and sympathetic essay on "The Indian Civil Servant and Modern Politics."

Reviews of recent elections in Denmark, Finland, Iceland, and Sweden, and the annual "Bibliography of Scandinavian political Science" by Helen V. Aareskjold add to the usefulness of the volume, also (and not least) inside Scandinavia.

NILS ANDRÉN

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Tension Areas in India's Federal System. By

Amal Ray. (Calcutta: The World Press Private Ltd., 1970. Pp. ix, 160. Rs. 15.00.)

In this brief volume Dr. Amal Ray develops some of the ideas which he first outlined in an earlier work, *Inter-Governmental Relations in India: A Study of Indian Federalism* (Bombay: Asia Publishing House, 1966). He is concerned with studying Indian federalism "not as a legal or constitutional structure, but eminently as a political process" (p. 21). It is particularly important to consider Indian federalism within its political environment, and that environment has changed considerably since the height of the Nehru era. "India's federal structure was conceived and planned in a unitary political environment" (p. 5), characterized by a "one-party dominance system," the "lop-sided dominance of Nehru within the Congress" (p. 23), a centralized planning apparatus, and, in general, so many centralizing trends as to threaten the balance of the federal system. In fact, the Centre was so powerful that no serious Centre-

State crises arose.

In the last years of Nehru's life a significant shift in Centre-State relations set in. Power began to move slowly from the Centre to the States. This trend was more manifest after Nehru's death. It was evidenced by the role of the Chief Ministers and political bosses in the selection of Nehru's successors in 1964 and 1966, by the trends toward localism, by the upsurge of regional forces and movements, and by the weakened position of the Congress Party, both at the Centre and in the States, as a result of the fourth general elections in 1967 (reconfirmed in the midterm elections in four important States in 1969) and of the split in the Congress Party in late 1969.

Dr. Ray's study reflects the impact of the changed political environment in India, and the consequent strengthening of the States in relation to the Centre. He concludes, as did most other observers, that the fourth general elections "put an end to the era of one-party dominance" (p. 29), that "sooner or later a coalition government shall be formed at the Centre" (p. 34), that "at the State level the unstable coalition is likely to remain the dominant style of politics in the foreseeable future" (p. 35), and that "no orderly and effective operation of Centre-State relations seems likely in [the] 1970's" (p. 35).

Quite obviously all of these predictions have proved to be wrong. A third important change in the Indian political environment has occurred since Dr. Ray completed his book. In the language of the psephologist, the general election of 1967 proved to be a "deviating" rather than a "realigning" election. As a result of the mandate which Mrs. Gandhi and her wing of the Congress Party received in the fifth general elections in 1971 (a mandate confirmed in the State Assembly elections of 1972), and of Mrs. Gandhi's astute handling of the crisis with Pakistan in 1971, a "one-party dominance system," under a leader of unquestioned dominance, has been restored at the Centre, in a new and perhaps stronger form, and in most of the States, and the centralizing trends of Indian federalism, apparently reversed in the years following Nehru's death, have again become manifest. Perhaps Dr. Ray will write a third volume on Indian federalism, to explain where and why he went wrong in his second study and to analyze the effects of the Indira Gandhi era in Indian politics.

While his overall predictions have not been borne out, the tensions which he discusses in this study still remain largely unresolved, and

his two chapters on the functional role of the President of India and the Governors in easing tensions are still highly relevant and interesting. Among the tensions which he analyzes are those resulting from constitutional strains; from the conflicts between planning and federalism, from controversies over law, order, and property—"new areas of tension" (p. 50)—and from continuing differences over questions of language.

In considering the proper roles of the President and the Governors in the Indian federal system he seems to want both more autonomous actions by these officials and more curbs on them. He holds that "the Constitution itself has enjoined upon the President an autonomous role in the sector of Centre-State relations" (p. 70), but he emphasizes "the need for important constitutional amendments in order that the subsidiary presidential features are not turned into dominant ones" (p. 81). He believes that the Governor too should always remember that he is both the head of the State government and the representative of the Centre, but he argues that "it is desirable to spell out the Governor's discretionary powers in minimum terms" (p. 87). He also mentions "the need for creating channels of communication between the civil servants and the people" (p. 115), although he is obviously not very hopeful that this will happen; and in a curious insertion in the Epilogue he refers to "the Judiciary's insufficient awareness of its crucial role either as a federal balancer or as [a] tension easer in the overall political system" (p. 149).

In spite of the many false predictions of the shape of things to come, Dr. Ray's essay is a concise, clearly-written, and suggestive interpretation of India's experience with "cooperative federalism" in a changing political environment.

NORMAN D. PALMER

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Developing the Third World: The Experience of the Nineteen-Sixties. Edited by Ronald Robinson. (Cambridge: Cambridge University Press, 1971. Pp. 289. \$13.50.)

In this skillfully edited book, Ronald Robinson makes a virtue of the fact that it contains papers written by fifteen different authors (plus six conference reports), generated by Cambridge Conferences on Development which took place between 1964 and 1969. His opening essay ("Practical Politics of Economic Development") catches the changes in mood which occurred during the latter years of the

decade and in the issues on which the practitioners of development (and the ardent chorus of commentators) came to focus. The authors are familiar and distinguished: Balogh, Busia, Chenery, Figueiredo, Flores, Hutchinson, Ikram, Jacoby, Meier, Please, Ramaswami, Schumacher, Singh.

The papers are grouped under five headings: general perspective; industrialization; rural development; the role of government; and aid.

Some quite striking shifts occurred between 1964 and 1969. In terms of growth rates, there was fear in the mid-1960s that development was "petering out." By the end of the decade, the central question was that a 5 per cent per annum real increase in GNP was not sufficient to avoid dangerous levels of unemployment. In the mid-1960s deep concern was expressed about the sluggishness of agriculture, the innate conservatism of the farmer, and the prospect of imminent Malthusian crises. By the end of the decade, the central question was the effect of the Green Revolution in polarizing the rural population. In the mid-1960s there could still be rather sharp debate on the primacy of industry versus agriculture in development. By the end of the decade, the interrelationships were much better understood, and the case for reasonably balanced development, on economic as well as on political and social grounds, was almost universally accepted. Less dramatically, there was a gradual refinement of perception on the links between economic and political development.

Aside from its value in reflecting the perceptions and debates of the 1960s, the book anticipates some, at least, of the critical issues of the 1970s:

—What should be done about high levels of unemployment and partial employment? Here Schumacher's case for "intermediate technology" (Chapter 7) is at the center of the debate. There is some reflection of the possibilities of two-tier industry, with labor-intensive as well as capital-intensive sectors, as pioneered in Japan; but the possibilities are not fully developed. And there is no exploration of the potentialities of two- or three-shift use of industrial plant.

—What criteria should govern the balance between public and private enterprise? Here Kaldor's argument in favor of enlarged public outlays is the centerpiece (quoted at some length, pp. 176-77) with its evocative but incomplete posing of the issue in terms of "the sixteenth Cadillac of a Latin American millionaire" versus rice subsidies. Others get at the question of the efficient and equitable use of resources in a more satisfactory way. But the great unresolved issue of the appropriate rules of the game for private foreign investment is not explored.

—How shall we generate an adequate flow of official aid from the richer nations? There are five useful papers on aid, almost wholly exhorting and instructing the aid givers. And, having been in the exhortation business for some twenty years, I do not object. But, surely, in the 1970s, the developing nations have responsibilities, too, as they exert more vigorously the prerogatives of independence and sovereignty. The responsibility of settling regional quarrels rather than exacerbating them; the responsibility of intensified regional co-operation which might generate attractive joint projects; the responsibility of working to produce stable and mutually satisfactory criteria for international private capital movements rather than using foreign investment as a convenient whipping-boy to generate unifying nationalist reactions when things are, otherwise, not going too well. This book does not treat the international politics of aid as the two-way street it will have to become if we are to generate the level of aid most of us would like to see.

And there are two very large gaps,—or virtual gaps: birth control; and the need for and possibilities of regionalism in Latin America, Africa, and Asia. Clearly, at best, the Green Revolution has only bought some time for effective population control policies to take hold. Without them, massive Malthusian crises are as certain as anything can be in an uncertain world, capable in some critical areas of overwhelming all the other matters before us. As for regionalism, the medium and smaller countries of Latin America, Africa, and Asia face severely inhibited prospects for industrialization and growth unless much more intense procedures for cooperation emerge.

Nevertheless, this volume is a valuable addition to the literature of economic development, relevant, on the whole, to the 1970s as well as to the First Development Decade.

A final word is required on three pages of Robinson's introductory chapter (pp. 15-17). In a terse, brilliant summation he catches the essential tensions, political and social, that economic growth creates as it moves forward. He concludes:

Unavoidably it seems, economic growth in the Third World tends to destroy employment faster than it creates it, to deepen social cleavages faster than the political system can contain and integrate them. Inequality in income distribution tends to outstrip the equalizing power of government; while old and new social and regional divisions tend to widen more swiftly than national sentiment and common economic interest extend to span them. The practical politics of economic development is to contain these negative trends and reverse them in the direction of economic growth and political integration (p. 16).

These, he notes, are the tensions which bring to the center of the economic development agenda recommendations for "intermediate technology, two-tier strategy, land reform, employment, and a balance between industrial and agricultural growth."

I would observe that the agenda is incomplete and will remain incomplete until social scientists concerned with development are prepared to turn more explicitly to the political process itself. There are cases in which the political process in developing nations has managed, against a background of rapid and steady growth, to contain these inevitable tensions, for some periods at least; and there are many cases of failure and acute political crisis. I have tried to isolate the lessons of these success stories and failures in a preliminary way in *Politics and the Stages of Growth* (Cambridge, 1971, Chapter 7). I do not believe we shall be successful in understanding and mastering this range of problems unless we are prepared—the intellectual and political leaders of the developing world, above all—to make the 1970s the Decade of Political Development, while carrying forward the Second Decade of Economic Development.

W. W. Rostow

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Economics, Politics and Administration in Government Lending: The Regional Loans Boards of Nigeria. By Sayre P. Schatz. (Ibadan: Oxford University Press, 1970. Pp. 146. \$3.75.)

Political science has suffered from unbalanced growth: far greater attention has been paid to the determinants of public policy than to the content, administration, and societal impact of policy. In the field of American politics the effort to correct this imbalance has increased noticeably in recent years. Studies in comparative politics, however, still tend to ask how power is won and decisions made rather than how these decisions are (or are not) implemented, or what effect government programs have on economic development, social structure, and subsequent inputs to the political system. In this brief, meticulously documented case study of the Regional Loans Boards of Nigeria, Sayre Schatz demonstrates the value of a balanced and comprehensive treatment of public policy. Students of comparative politics would do well to heed this economist's general approach as well as his specific conclusions.

The study, based on four years of research in Nigeria, deals with the efforts of the country's regional governments to aid indigenous busi-

ness by lending public funds for productive enterprises; special attention is paid to the years immediately preceding and following independence in 1960, when the Regional Loans Boards were particularly active. Schatz illustrates the influence of political factors on the loans allocation process, a phenomenon related to the newly-acquired regional power base of the major political parties. He then shows how administrative problems—including staff shortages, rapid turnover, the absence of reliable data, a limited capacity for project viability studies, and high civil service salaries—contributed to the rather dismal cost-benefit record of the Loans Boards. Schatz is insightful in showing how political and administrative factors had mutually reinforcing negative effects on Loans Board performance: political manipulation fostered low morale among bureaucrats, while "The dearth of competent personnel for appraising loan applications almost ensured the potency of political considerations" (p. 124).

The author argues, however, that poor performance—measured by the high failure rate among assisted businesses and the "incredibly high" (p. 128) cost of financing the few projects that were successful—is due far more to the economic environment in which African private enterprise operates than to the factors just discussed. Governments can try to alter the economic environment by providing capital where needed. But Schatz contends that the major economic constraint in a country like Nigeria is the shortage not of capital but of commercially viable projects. The latter shortage, in turn, is presumably due to a low level and unequal distribution of income which it is extremely difficult for government policies rapidly and peacefully to alter. The author might have discussed, along with domestic constraints, the considerable impact of external economic forces—and of large expatriate firms operating within Nigeria—on the failure of so many Nigerian manufacturing, processing, and transport enterprises.

This book may help to moderate, as well as provide empirical data for, the frequently emotional debate over capitalist and socialist paths to economic development. That debate concerns in large measure the economic roles to be played by the public and private sectors. Schatz implies that the public/private distinction may be exceedingly hard to draw—and not terribly significant at that—in cases where (1) a rising political class uses public offices and resources to strengthen its position as the economic elite in the private sector; (2) government intervenes actively in the economy to aid, rather

than compete with, private business; and (3) nationalist sentiment puts the government of an independent country in the same camp as the indigenous business class in the struggle against foreign-based economic exploitation. If, moreover, the economic environment has the profound effect on government's economic performance attributed to it by Schatz, the kind of public/private organizational mix a country chooses may have little practical consequence. The author, concluding that business-assistance programs are not necessarily low-cost users of public funds and administrative talent, hints that Nigerian governments should become more directly involved in administering economically productive activities. But whether a more socialist option would improve matters, in view of the negative effects on decision making of the political and administrative realities so carefully noted in this book, remains an open question.

DAVID B. ABERNETHY

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The Awkward Class, Political Sociology of Peasantry in Developing Society: Russia 1910-1925. By Theodor Shanin. (Oxford: Clarendon Press, 1972. Pp. 253. \$15.25.)

The controversy over Russian peasants started more than a century ago. Those among Russian intellectuals who developed a leaning toward Western theories, and especially Marxists, argued that industrialization and development of capitalism would affect the peasants in Russia as they had affected peasants in other parts of Europe. The rural population would dwindle. Capitalist relations would appear in the village, diversifying the peasants into rich and poor. A class struggle would ensue. As a result the socioeconomic structure and culture of the *obshchina*, the traditional rural community, would decline, if not disappear altogether. Originally the Narodniks, later the Populists, voiced a different opinion. They believed that Russian peasants represented a unique historical case. These people had retained their socioeconomic order and value system virtually unchanged throughout centuries. Left undisturbed, they argued, the peasant socioeconomic order would persist and perhaps even would generate a new sense of social justice and healthy social order encompassing the entire Russian society. Shanin does not claim that the *obshchina* constituted a natural source of socialism, as Narodniks and some of the Populists had done in earlier times, yet he is definitely on their side.

His book is an important contribution in the

field of Russian history and the outlined debate. It is, however, even more important perhaps for the analysis of problems that it does not tackle. It provides a formidable analytical perspective for the analysis of the problems of modernization and socioeconomic development of peasant societies in the Third World nations. In a most provocative way it inspires the reader to pose questions about theories of African socialism, contemporary peasant revolutions, the possibility of modern societies composed of small communes, and other current issues. Furthermore, it is valuable as a work in social theory which especially accentuates the distinction between modern populism and Marxism. It is therefore a most illuminating work in rural sociology with far-reaching implications for political analysis.

Shanin begins the discussion by sketching a picture of Russia as a society entering the era of industrialization with 84 per cent of its people being peasants. He then focuses on the characteristics of the peasant society as a whole, and subsequently on the most elementary units of its structure, the *obshchina* and the family. The second part of the book begins with an outline of the debate concerning the development of the Russian peasantry. Following Chayanov, Shanin believes that the Russian peasantry has been in a process of perpetual multidirectional and cyclical mobility. Centripetal tendencies in this process have balanced centrifugal trends. Thus the society retained its character as an aggregation of middle-level peasant family units, neither too poor nor wealthy. Indeed, on the whole the peasant society was poor, illiterate, and ignorant. The average wealth and acreage of the land-holding of the household was usually proportional to the number of mature male members of the family. The writer points out several factors which maintained the Russian village in this state: periodic redistributions of the land by the community; an egalitarian distribution of land in the course of the Revolution; constant partitioning of more wealthy households by mature male members; migration, and so on. In substantiation he produces a wealth of data meticulously extracted from the relevant literature, most of it published in the Soviet Union in the 'twenties. His theses are also supported by analyses of various studies pertaining to the subject and organized around diverse methods. Among these are budget studies; research data of the Central Statistic Bureau; and works on "substantive changes" or transmutation of peasant households in Russia. Finally he considers the impact of biological and economic factors on

the stability of the rural economy. The third part of the book is concerned with the period from the Bolshevik Revolution until 1925, when the Party began to make preparations for pervasive collectivization. Social stratification in rural areas, and conflict and interaction among diverse centers of local power are discussed here. The writer demonstrates that the major conflict in rural areas after the Revolution was not so much between the poor peasantry and the kulak minority as between the peasantry as a whole and the external structure of Soviet authority.

The penetrating analysis of the literature on the subject and of the Russian reality splendidly supports Shanin's basic conclusions. Nonetheless, doubts may be raised about whether this disproves the argument of the writers from the opposite camp who claimed that left undisturbed, the awkward peasant society would have turned into a "normal," progressively more diversified and declining peasant social structure and culture. These arguments assert: (1) All peasant societies, whether in Western Europe in the past or in other parts of the world, have perpetuated themselves in cyclical and differentiating processes taking place within a more general tendency toward transmutation and decline; and (2) peasant society in Russia never existed as an undisturbed entity. The writer discusses a relatively short period of twenty years. Even in "normal conditions," the process of differentiation within a peasantry comprising 84 per cent of the population would have not been so overwhelming as to eliminate cyclical mobility. This was an exceptional period, and all factors at that time worked toward leveling the differences within the Russian village. The policies of the Soviet government directed against the kulaks, against the rich, and to a degree against any entrepreneurial activity reinforced the encapsulation and perpetuation of cyclical mobility. Thus accelerated differentiation was halted in such conditions. Besides, after the Revolution industries were destroyed and could not provide either an outlet for the surplus of labor in rural areas or an adequate volume of products to serve as an incentive for the peasants to increase their production and engage in entrepreneurial activities. There was little in the market to buy for the money that they could have earned. Peasants had to abstain from entrepreneurial activities in order not to be conspicuous so as to avert repressions directed against the wealthy. It is therefore not astonishing that differentiation in the Russian village was not too pronounced under such conditions.

These comments are made not to diminish the importance of Shanin's impressive work, which I very much admire, but rather to assert its provocative character. They are made to ask again what would have happened had there been less interference by Soviet authorities in the spontaneous development of the peasant society of Russia. With the highest regard for Shanin's excellent work, I have to say that that question remains and probably has to remain open.

SZYMON CHODAK

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The Military in Politics: Changing Patterns in Brazil. By Alfred Stepan. (Princeton, N.J.: Princeton University Press, 1971. Pp. 314. \$10.00.)

The literature on the military in politics in the Third World has dealt almost exclusively with three questions: Why do military coups occur? How stable is military government? How much a force for "modernization" and socioeconomic reform is the military? Alfred Stepan deals with all three questions, but mainly the first two, in this fine study of Brazil in particular and of the military in politics in general.

Stepan's main thesis is that to understand the propensity for and success of military coups, one needs to consider the military as a subsystem of the larger political system. One needs especially to know the attitudes of civilian groups in the political process. The military acts the way it does in politics, Stepan contends, not simply or even principally because of its size, financial resources, organizational strength, institutional interests, or socioeconomic backgrounds and characteristics (although these factors can play a part), but mainly because of the pressures placed upon the armed forces by other groups in the political process. At least this is the case, Stepan maintains, when the "moderator pattern" of civil-military relations obtains.

This part of the book constitutes much more than half the study, and it is very persuasive. Stepan's main thesis is well argued, illuminating, theoretically significant, and amply documented. Especially important in these chapters is an analysis of editorial opinion in upper- and middle-class newspapers during five coups and coup attempts in 1945, 1954, 1955, 1961, and 1964. The openness and intensity of civilian appeals for military intervention in these instances are astounding and very revealing of the reasons for the coups.

Stepan deals with the second question, re-

garding the military's capacity to govern, in three chapters on the military in power in Brazil from 1964 to 1968. He concludes that governance creates tensions and conflicts within the armed forces which in turn lead to instability and crises of succession. Moreover, the military desire for unity has led, in the Brazilian case, to repression of dissent (including widespread use of torture) and isolation from civilians. Such repression has its roots, according to Stepan, less in the internal contradictions of capitalism or the wily machinations of the United States (although Stepan has much to say about U.S. policies) than in the nature of the military as a governing institution. Stepan thinks that the often-presumed stability, unity, and fixity of purpose of the Brazilian military is an illusion. He anticipates more internal divisions and (possibly) coups and perhaps a return to at least a modified form of the old moderator model.

Stepan devotes least attention to the third question; however, his answer is essentially that although the military in Brazil has not yet been a force for socioeconomic reform and egalitarianism, its role may well shift at any time from one of "system maintenance" to one of "system-transformation" (p. 267). He sees the military as "above class conflicts." It is "above all a situational elite, not a class elite," and therefore the policies of the military in Brazil and elsewhere will probably shift "from far left to far right, and back again" (pp. 269-270).

Against Stepan's answer to the third question stands that of Samuel P. Huntington (*Political Order in Changing Societies*, pp. 219 ff.). Huntington suggests that the modernizing and reformist impact of the armed forces varies depending on the socioeconomic conditions of the country at the time the military is in power. In a traditional or transitional society, says Huntington, the military can play a radical role; in more modern societies, the military's socioeconomic role becomes more conservative. Stepan considers briefly, and rejects, Huntington's hypotheses (and those of José Nun, which are similar). But it is not clear that Stepan's representation of their analyses is entirely accurate.

Moreover, Stepan himself notes that in Brazil, "the continuing middle-class fears of the lower class remains [sic] a source of their support for the military's antimobilization policies" (p. 48), and that "military 'radicalism' [is] much more improbable in Brazil than in Peru," which is less modernized and complex sociologically (p. 271). These conclusions, which seem sound, flow much more logically from the

Huntington-Nun perspective than from Stepan's conception of the military as a free-floating "situational" elite oscillating between the far left and the far right. The notion of oscillation is probably useful within countries over short periods of time; but Huntington's hypotheses seem necessary to explain the differences among countries, and within countries over longer periods of time. Thus, oscillations can occur anywhere; but on the whole they tend to have more radical consequences in a relatively backward country (Peru) than in a partially modern one (Brazil)—or in Brazil from 1870 to 1930 than in Brazil in the 1960s or 1970s.

This study is rich in original data. Stepan had formal interviews with sixty officers; he talked informally with more than two hundred military personnel; and he visited military installations in five states. He also used files of the Brazilian military academy, newspaper editorials, and a wide range of secondary sources in English and Portuguese. The techniques and measures Stepan used are sensible and innovative. A former journalist, Stepan writes extremely clearly and briskly, mixing quantitative materials and qualitative insights nicely, without belaboring the former. Finally, as an exile (in the United States) who researched and wrote this study while employed by the Rand Corporation, he also had extraordinary access in Brazil. But his achievement is also a personal one: before this book, many experts claimed that an intensive empirical study of the Brazilian military was impossible, yet Stepan did it.

In sum, this book's contributions are both substantive and methodological, and it should serve as a point of departure for comparable studies in other countries. Stepan's *The Military in Politics* is probably the best study we have of civil-military relations in a poor country; it is also, along with Philippe Schmitter's volume on interest associations, one of the two best books by a North American political scientist on contemporary politics in Brazil.

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Approaches to Politics. By Pierre Elliott Trudeau. (Toronto: Oxford University Press, 1970. Pp. 89. \$1.95, paper.)

Trudeau in Power. By Walter Stewart. (New York: Outerbridge and Dienstfrey, 1972. Pp. 239. \$6.95.)

These two short books encapsulate the ideal and the reality of P. E. Trudeau, Canada's present Prime Minister. The former is an edited

collection of Trudeau's essays, written for a small circulation journal of opinion in Montreal in the mid-fifties. They are directed against the crude, repressive, boss politics of Premier Maurice Duplessis, and his use of nationalism as an instrument of political manipulation, along with old-fashioned Catholic attitudes of deference to authority. The result is a strongly argued if legalistic defense of the rights of the individual against abuses of power by the state and its agents. It shows no awareness, however, of the problems posed by modern structures and concentrations of power. And nationalism becomes Duplessis' crude, hate-filled racism, preached for ulterior motives—and nothing more. In reading this collection of essays, one has the feeling of reading the writers of the Enlightenment against the *ancien régime* (except for the recent Canadian references); and in a sense this is what it is. The Quebec of Duplessis was a kind of *ancien régime*, and Trudeau was a courageous defender of liberty in the face of reaction. His writing is studded with references to Rousseau, Austin, the *Vindicae Contra Tyrannos*, Saint Thomas More, the papal encyclicals, and lesser-known Catholic (usually Quebec) authorities, with whom he takes issue. His problems are the old ones: How does one man gain authority over his fellows, under what conditions is regicide justified, what are the bases of the social contract, what means can be used to achieve the greatest happiness of the greatest number. These articles reveal the categories in which this solitary man thinks, and, along with his *Federalism and the French Canadians*, indicate his approach to politics. He is both a vigorous defender of the rights of the individual, and a rigid legalist, tenaciously defending the established constitutional structure. He is a tough-minded rationalist who does not fail to act on his conclusions, which he draws simply and clearly from his evidence.

Having passed his heroic years in the battle against the Duplessis régime, Trudeau assumed attitudes suited to this situation; so he emerged with a simplistic approach to nationalism, little awareness of the powers and ways of modern, large-scale organization, and an excessively individualistic and legalistic approach to political problems.

Stewart's book, *Trudeau in Power* is a critical journalistic account of the Prime Minister in action; the treatment is factually accurate, but the author tends to draw more conclusions than his evidence supports. Canada's divisions and economic dilemmas were serious before Trudeau came along, and the author, like most

Canadians, expected this oversold leader to resolve these problems (notably unemployment, the independence question and relations between English- and French-speaking people). It is true to say that these particular questions are now more serious than in 1968. His analysis of the new governing structure under what he calls the Supergroup, supported by two larger circles drawn from the public service and political recruitment is well presented, and his conclusion (which he admits is not original) is significant, that Trudeau is replacing the essentials of parliamentary democracy with power concentrated in his own staff, like the American president. The book presents the most important issues of Trudeau's Prime Ministership in lurid, but basically honest colours, and, while his criticism is severe, the essentials are sound.

H. G. THORBURN

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Chinese Political Thought in the Twentieth Century. By Chester C. Tan. (Garden City, New York: Doubleday and Co., 1971. Pp. 387. \$7.95.)

World history seems to indicate a positive relationship between political instability and intellectual vitality. In China for instance, one of the greatest periods of intellectual activity, which produced Confucianism, Taoism, and other philosophies of the "hundred schools," was the chaotic time of the late Chou and the Warring States. Given the political disintegration and institutional uncertainty of late nineteenth- and early twentieth-century China, it should be no surprise that this period also reflects an intense level of questioning and analysis, system building, and the hybridization of ideas. It is this phenomenon which Professor Tan chronicles for us in *Chinese Political Thought in the Twentieth Century*.

Unfortunately Professor Tan does not attempt to explain the relationship between political instability and intellectual activity, although he does explicitly recognize the close association that political thought has with contemporary political phenomena. Indeed his account alternates between event and idea rather consistently, and he is successful in placing ideas in meaningful historical contexts.

The work is organized more or less chronologically and treats both individual thinkers and movements. It begins with a section on "Moral Conservatism and Liberal Democracy" (K'ang Yu-wei, Liang Ch'i-ch'ao and Chang Shih-chao), which is followed by sections tracing the development of anti-Confucianism and the ap-

peals of democracy and such new "isms" as socialism, anarchism, and Marxism. Professor Tan's discussion highlights the powerful hold Confucianism had on the minds of China, even among the most radical anti-Confucianists. It also illustrates how Confucian prohibition against heterodoxy persisted in new forms into the twentieth century. As the discussion of Chiang K'ang-hu's socialism shows, the first step in the diffusion of new political thought was not the full articulation of a new doctrine, but was rather the propagation of ideas so as to establish the legitimacy of considering them.

The middle section of the book is perhaps the most interesting and useful. It includes a long chapter on the thought of Sun Yat-sen which attempts to establish Sun's thinking as the central alternative of Marxism. The following chapter on the political thought of Kuomintang leaders (Chiang Kai-shek, Tai Chi-t'ao, Chu Chih-hsin, Hu Han-min, and Wang Ching-wei) continues to play on the theme of the centrality of Sun. Two additional chapters, one on "Human Rights and Democracy" (Hu Shih, etc.) and one on the "Third Force" (Chang Chun-mai, Chang Tung-sun and Liang Shao-ming), complete the middle section of the book. Professor Tan concludes his study with two chapters on Chinese Communism, one devoted to the thought of the pre-Mao Communist leadership and one on Mao Tse-tung.

A few of its characteristics may rob Professor Tan's book of the impact it might otherwise have had. First it would have been helpful at the outset if the author had said explicitly what he meant by "political thought." The implied meaning of the term seems to include ideas which shaped national policy and initiated social trends, and includes philosophy, ideology, and strategy and tactics.

Most significantly the work has no clearly stated thesis and draws no conclusions. While this characteristic might be attributed to understandable scholarly prudence, it is regrettable in this case since one senses that the author has gotten to know his subject rather intimately. I would have welcomed the author's more general reflections on political thought in modern China. Some of the big questions remain unasked, although ironically, not completely unanswered. Why do some ideas, and not others, achieve a level of saliency to become the bases for an integrative doctrine (e.g., Sun's thoughts, Marxism)? At what point, and under what conditions does integrative doctrine become a weapon of ideological conflict and the object of disputes over orthodoxy (e.g., the

varying interpretations of Sun by Tai Chi-t'ao and Hu Han-min)? How did the influence of modern empirical analysis affect the character of Chinese political thought?

Finally, there is a particularly curious omission in the book: Many of the men discussed by Professor Tan have been treated in depth in English language doctoral dissertations and published monographs (for instance, K'ang Yu-wei, Liang Ch'i-ch'ao, Li Ta-chao, Sun Yat-sen, Ch'en Tu-hsiu, Mao Tse-tung). Strangely, there is little reference to this secondary literature. Professor Tan's strict adherence to primary sources is laudable, but one can't help feeling that this would have been a better book if it had come to grips with the interpretations of the secondary literature.

Although one might find analyses in greater depth of individual thinkers and movements elsewhere, the unique value of *Chinese Political Thought in the Twentieth Century* is that it gives us for the first time a synoptic view of modern Chinese political thinking from K'ang Yu-wei to Mao Tse-tung. For the scholar, this synoptic view offers a new perspective on the relationship between ideas and action in modern China. For the teacher, this volume read in conjunction with a book like *Sources of Chinese Tradition* would be useful for advanced undergraduate courses.

RICHARD P. SUTTMEIER

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Social Class and Social Change in Puerto Rico.

By Melvin M. Tumin, with Arnold S. Feldman. (Indianapolis: Bobbs-Merrill, 1971, 2nd ed. Pp. 525. \$9.95.)

We, The Puerto Rican People: A Story of Oppression and Resistance. By Juan Angel Silén. (New York and London: Monthly Review Press, 1971. Pp. 134. \$5.00.)

Two views of Puerto Rican society could hardly differ more than those of Melvin Tumin and Juan Angel Silén. Between these two authors exist generational, ideological, cultural, and disciplinary gaps which can be explained but not reconciled. Tumin's membership in sociology's establishment is reinforced by the painstaking manner of research and presentation in evidence in *Social Class and Social Change in Puerto Rico*. His approach and hardly earth-shaking conclusions are those of a detached behavioral scientist coming across an intriguing community. Silén, a young Puerto Rican scholar and activist, on the other hand, writes from the heart rather than the printout. His revisionist essays on Puerto Rican history

and historiography lambaste what he calls the "pseudo-scientific" findings that Puerto Ricans are a sad and docile people (pp. 15, 36-45).

Tumin's book, which originally appeared in 1961, carefully, almost tediously, analyzes data from extensive questionnaires applied in the late 1950s to an area probability sample of one thousand Puerto Rican family heads. Aside from a very brief new preface, Tumin has neither revised his study nor conducted what would have been an extremely valuable longitudinal analysis. Given the past decade's surprising political developments, republication now would be unjustifiable were it not for the book's attempt at rigorous quantification and focus on significant theoretical issues. The book provides extraordinarily detailed, but frequently sterile, empirical findings on the consequences of social change on classes.

For Tumin, "Puerto Rico is a class-structured society that is undergoing rapid change" (p. 3). Such change, one learns, operates within a dualism which provides both successful adaptation to new urban and industrial relations and stubborn retention of most significant family and ethical norms. Parallel literature remains sparse and rarely deals with an entire society. Oscar Lewis developed provocative themes on the "culture of poverty" by focusing on representative social types. Manning Nash and Sol Tax studied communities' response to changing national and local conditions. The literature on social development has also uncovered ethical and structural inhibitions to change as seen, for example, in the work of Banfield and Lerner. Tumin's broad approach illuminates social change with a diffuse but dim beacon while the others use sharply focused flashlights.

The book explores two critical dimensions, education and residence. Halfway through the book, reliance on geographical differences (San Juan, other cities, and the countryside) are all but dropped, so that for most purposes, he defines classes in terms of years of formal education. Other variables (income, influence, professional status, and self-esteem) are found to be inferior predictors of class differentiation, but of course the argument is more circular than real. Surprisingly, Tumin makes no serious attempt to factor analyze competing indices to support his case. Using formal educational attainment as a blunt tool to distinguish three basic classes, Tumin explores class differentiations on attitudes toward racial issues, economic prospects, use of leisure, propensity to join voluntary organizations, reference groups,

self-esteem, and social mobility. The reader expecting discussion of educational programs or, for that matter, any social program will be sadly disappointed. Quantity of formal education is taken to explain just about every facet of life in Puerto Rico, a point which will strike some as patently ridiculous and others as profound.

Far and away, the most absorbing, difficult, and probing analysis concerns measurements of social mobility. Tumin employs two, the Generational Occupational Mobility and Career Pattern Mobility scores. Although some minor differences arise, these inter- and intragenerational measures, respectively, are almost mutually supporting. A brief chapter analyzing these scores in Puerto Rico in the light of results of similar measures of representative nations might be read profitably by comparativists.

Some fascinating social questions are treated carefully and intelligently. The most crucial is whether "... the economic tasks of development and distribution (can) be carried out to increase the standard of living throughout the Island without developing, to any much greater degree than is now present, a cultural emphasis on ranking and on invidious distinctions based on occupation and income" (p. 483). Tumin's answer is couched in conditions. To retain economic growth without social cleavage will require (he claims) acceleration of the 1950s' impressive social mobility, increased use of education as an instrument of mobility as well as of socialization, common allegiance to symbols of national identity, and survival of cementing traditions regarding family and religious life and intergroup tolerance. This type of answer, almost a self-parody of academia, is not likely to satisfy anyone concerned with the relative significance of these often competing social changes and traditions. The dualism Tumin sees in Puerto Rican society may well be the result of his failure to employ multivariate analysis and the consequent significance he gives to salience and nonsalience of highly operational but dubious definition of classes.

Significantly, Tumin's work is bereft of any substantive evaluation of such key institutions as education, religion, voluntary association, or partisan politics. In such a lengthy and apparently comprehensive study this omission is serious. Social scientists are thus likely to find the book especially frustrating. While Tumin recognizes "... that economic decisions involve key issues of politics, social organization, and psychological motivation" (p. 21), these issues are neither studied nor defined. The momen-

tous political events in post-1960 Puerto Rico—which may be seen largely in terms of rapid polarization—are not foreseen in Tumin's study. Education, regional planning, family life, and political status continue to generate dramatic issues on the island, but as issues these receive no attention.

An exciting but onesided account of them does appear in Silén's book which intentionally is a primer on Puerto Rican political and cultural history. It begins with a provocative but hardly scholarly historiographic essay which rightly points out the false images perpetuated by "colonial" academics; the stereotype of Puerto Ricans as "sad," passive, and docile people is ably rebutted. Moreover, as part of the genre of contemporary history written by "losers," *We, The Puerto Rican People* is best at dispelling old myths and worst at defending new ones. The role of radical movements is exaggerated in Silén's work, although his effort to cast them in the same terms as other Third World revolutions is not without some basis in fact. As Tumin would agree, Puerto Ricans are not merely poor Americans. Their resistance to assimilation and domination despite "carrot and stick" policies is what preserves Puerto Rican nationhood in the absence of political independence.

Silén argues that the reemergence of the draft issue (during the Korean War Puerto Ricans suffered casualties at a rate 35 times greater than the mainland population's) has increased revolutionary action. Also, the failure of capital-intensive industrialization to solve employment problems, the misguided and unscientific attempts at making English dominant in commerce and education, and the continued conservatism of much organized religion on the island are blamed for what Silén sees as the desperate plight of today's Puerto Ricans. His revolutionary prescription, however, is a mixture of organization and selective violence, and the results thus far are not impressive.

The irony of reading these books in tandem ought to be obvious. The stabilizing and complacent effect of education according to Tumin is lost on many Puerto Rican intellectuals. Silén represents a growing number of Puerto Ricans, well educated, of "good family," "cosmopolitan," who do not behave as Tumin tells us they should. Their disenchantment with Puerto Rico's political and social changes of the past decade are just as real as Tumin's social diagnosis.

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Iran Faces the Seventies. Edited by Ehsan Yar-Shater. (New York: Praeger Publishers, 1971. Pp. xx, 391. \$12.50.)

With the increasing emphasis in the social sciences upon the issue of modernization, the contemporary experience of Third World societies challenged by change becomes manifestly relevant. Iran is a dramatic case of a non-Western society confronted by the need to modernize. The Iranian case is an especially important one because of the political elite's calculated program to address the developmental challenge. The Shah of Iran, who rules as one of the last absolute monarchs in the world and whose power is flimsily dressed up in garments of modern representative institutions, is the major force shaping Iranian developmental policy. With the assistance of vast petroleum resources, the Shah emphasizes economic and military development, considers piecemeal social development, and ignores effective political development. In so doing, he consistently manages to remain one step ahead of social upheaval and personal disaster. The Iranian political elite's qualified success in instituting a program of selective reform from above deserves the careful consideration of developmental scholars everywhere.

Iran Faces the Seventies is a volume consisting of 15 articles first prepared as papers for a conference at Columbia University in 1968. The selections range all the way from discussions concerning religion and politics to writings focusing upon painting and music. There is no unifying intellectual theme other than the fact that all the articles have something to do with modern Iran. The result is a highly uneven and disjointed collection of essays, most of which have little to do with political analysis. Yet, this hotchpotch of articles contains a number of valuable sections that are well worth the exercise of mining them. These explicitly treat interpersonal relations, educational patterns, literary trends, and land reform programs. Implicitly, there is very occasional commentary that touches directly upon power and authority structure.

This volume contains three kinds of useful articles in descending order of significance: (1) those that provide new analytic material important to the student of Iranian and developmental politics; (2) those that provide competent presentations of subject matter also discussed elsewhere; and (3) those that provide useful descriptive material bearing on the political system. The articles falling outside of this classification are generally irrelevant to serious scholars of politics.

In the first category, I include the following three contributions: Brian Spooner's discussion of religion and society; Gustav Thaiss's piece on social change, religion, and the bazaar; and Ehsan Yar-Shater's analysis of modern literary trends. The Spooner article, although turgidly written and sprawlingly organized, contains a number of perceptive and fascinating ideas. The most important of these is the focus upon the dialectic between corporate and noncorporate groups that animates Iranian religious and social institutions. The emphasis upon the informal *hey'at* groupings and the decisional networks composed of such collectivities is crucial to one's understanding of power and change in the Iranian context. Such insights make the necessity of wading through 74-word sentences worthwhile.

Gustav Thaiss's study is a thoughtful analysis of the relationship between Shi'a Islam and social change. Besides capably discussing the struggle between tradition and modernity in the bazaar, Thaiss analyzes the amorphousness of political discussion and opposition among commercial and religious groups in Iran. Also, to my knowledge, this is the first time that the crucial sociopolitical role of institutions such as the Hosseiniyeh Ershad has been discussed in Western scholarship. More critically, there is perhaps a misplaced emphasis upon the asserted fissure that is declared to be occurring between the bazaaris on the one hand and the clerics on the other hand. A general common opposition to the regime in Iran continues to galvanize the traditional alliance between these two classes.

The article by Ehsan Yar-Shater on "The Modern Literary Idiom" is perhaps the most important selection in this volume. It is virtually impossible to understand Iranian politics without some knowledge of contemporary Iranian literature. It is in the literature that one finds revealed the unvarnished political orientations of significant sectors of the Iranian people. Yar-Shater's well-written article is the best available study in English of modern Iranian writers and their works. In Iran, literature and politics are intertwined; change in one area is immediately reflected by change in the other. Social scientists interested in Iranian society and politics must read Yar-Shater's study. Its only major defect is an occasional proclivity on the author's part to back away from political discussion by stressing form over content.

In the second category of contributions, we include the studies by A. K. S. Lambton, Marvin Zonis, and J. C. Hurewitz on land reform, education, and foreign policy respectively. Miss

Lambton has written elsewhere at greater length on the same topic. The Hurewitz contribution is a good overview of Iran's place in regional and world affairs; it is less informed when it touches on domestic politics. Zonis's piece is one of the best available summaries of the relationship between higher education, politics, and change in Iran. It is well researched, well organized, and well written.

The final positive contribution in this volume is the article by Hafez Farmayan on political events in the 1960s. Relying heavily upon Persian sources, Farmayan provides a useful account that is long on description but short on analysis. All in all, this collection should serve scholars of contemporary Iran as a useful reference work. It is a volume, however, that political scientists must consult with a discriminating eye.

JAMES A. BILL

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The Flame of Freedom: The German Struggle Against Hitler. By Eberhard Zeller. Translated by R. P. Heller and D. R. Masters. (Miami: University of Miami Press, 1969. Pp. 471. \$15.00.)

Most writings on "German resistance to Hitler" have a biographical or autobiographical character and are entangled in concerns about the political reputation of the Germans or some particular German individual or group ("the military," "the proletariat," "the bureaucracy," Catholics or Protestants, Bavarians, Prussians or Rhinelanders, and so on). They lack analytical distance from the subject matter. They are frequently silent or unconvincing about relationships between internal opposition to Hitler in Germany and modern German politics as a whole.

Eberhard Zeller's book is of this genre—indeed, one of its leading examples. This, however, is hardly to say that the book is without value. On the contrary, even in this abridged English translation (based on the 4th German edition published in 1963), Zeller's work is relatively comprehensive on the background and activities of those military and civilian officials (or ex-officials) who discussed a *coup d'état* against Hitler at various times and with varied intensity between 1938 and 1944. General Ludwig Beck, Mayor Carl Goerdeler, Count Helmuth von Moltke, and Count Claus von Stauffenberg are the best-known of these personalities, but Zeller gives considerable detail about many others whose names are less familiar.

In its essentials, Zeller's book is more than two decades old, the first German edition hav-

ing been prepared in the immediate postwar years. His account of the military-civilian conspiratorial discussions is convincing in the sense that few serious persons would wish to deny the impressive personal stature of many of the conspirators. The materials which have come to light since the first edition—or since the last German edition of 1963—have not diminished the fact that there were oppositional Germans who engaged in efforts at undermining Hitler's rule, and who suffered grievously after the failure of the assassination attempt on July 20, 1944.

Moreover, these materials have continued to confirm Zeller's point that it is a gross oversimplification to attribute the oppositional sentiments and resistance activities of the conspirators solely to narrow national interests, resentment at Hitler's military bungling after 1943, or aristocratic "class interests." Most of the conspirators were considerably more complex personalities than such purported explanations would have us believe, and so were the facts.

Just because of these complexities, however, Zeller's work is a hopelessly inadequate guide to an understanding of the facts or significance of German resistance to Hitler. This is not just because it concentrates on a relatively limited group of conspirators or would-be conspirators and ignores broader manifestations of opposition in the populace or "the working class." The book defines its scope as that of the 1938–1944 conspiracy; moreover, except for the existence of the *Rote Kapelle* group with its links to Moscow, no widespread opposition to the regime has yet been demonstrated among the general populace or particular classes. Nor do the limitations of Zeller's account derive primarily from the clumsiness of parts of the translation or the elimination in this edition of portions of the German text. Very little improvement could be made in the translation of some of Zeller's more mystical and ambiguous passages.

The difficulty lies in the author's failure to grapple with the question of the extent of resistance activity directed to an overthrow of the regime (as distinct from complaints about its blunders) and in his blindness to the connection between the hesitations of the conspirators and their uncertainties about a viable and desirable political alternative to the Nazi system.

That system had institutionalized a vulgarized and caricatured version of "revolutionary conservative" objections to Western "bourgeois" society. Most of the anti-Nazi conspirators between 1938 and 1944 also rejected

"bourgeois" society and had participated in one version or another of movements of dissent and resistance against the Weimar Republic. Their attitudes on themes such as "the need for renewal" are rather faithfully—if often murky—reflected in Zeller's own book. This is not surprising, since Zeller, a physician by profession, maintained ties with the Stefan George Circle, particularly in association with Claus von Stauffenberg's brother, Berthold von Stauffenberg. He was a rather representative figure of the "critical conservative" intelligentsia of the Weimar Republic, which maintained connections with aristocratic families (of pre-industrial lineage), and rejected "mass democracy" and "party politics," yet also despised the vulgarity of German economic and social development after 1870, together with its new men of power. In effect, Zeller's book is a testimonial to the differences between political romanticism and the Nazi criminality and vulgarity which used many similar slogans. At the same time, in its apolitical attitude Zeller's book reflects the desperate and isolated position of the political romantics when confronted by a regime which simply gave words and "ideas" the meaning it wished through sheer power.

Zeller's book seems to rest on the assumption that politics should consist in the assertion of principles or "spirit" by the individual. A more up-to-date term for this understanding of politics might be the expression "commitment." In West Germany today, both the young and men of letters and education seem increasingly ready to contrast "commitment," "dissent," and the need for a "true community" with bourgeois self-satisfaction—although now from a purportedly "left" perspective. The fate of the revolutionary conservatives in the world of political practice may have an unexpected contemporary relevance if it illustrates that passionate commitment is not the same as political judgment.

GEORGE K. ROMOSER

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SShA: Problemy vnutrennei politiki (The USA: Domestic Policy Problems). Edited by Valentin S. Zorin. (Moscow: Nauka Publishing House, 1971. Pp. 406. 1.55 rubles.)

Jointly authored by nineteen Soviet Americanists, this volume devotes one chapter to each of the following problems: the economy, urban affairs, mass poverty, crime, youth, the blacks, the far Right, and the military-industrial complex. Since it is impossible to treat each complicated chapter separately in a short re-

view, the book will be considered in the light of the contribution it makes to the advancement of the Soviet study of American politics.

Nowhere between two Soviet covers has such a broad range of problems been studied so comprehensively and topically—most of the material deals with the 1960s with reference to antecedent considerations where necessary. The book is particularly interesting for the unusual mixture of information it presents to the Soviet scholarly community: a rare collection of statistics often giving a good overview of the dimensions and parameters of each problem; a summary of the differing views of American scholars, experts, and publicists on the problem with criticism of those views; a discussion of the policy efforts made to overcome the problem, and the resultant success or failure of the policies. There are also some predictable ideological concerns, especially the periodically reiterated assertion that, of course, these problems cannot be resolved in a capitalist society because it is in the very nature of that society to create just such problems.

The problems are analyzed in two time dimensions. In the long run, as their ideology tells the authors, the problems are insoluble. But then there is the meantime, when the ways in which the problems develop and are handled by the political system, become important concerns for Soviet scholarly analysts and political leaders. So the book concentrates upon the meantime (where ideological considerations do not loom so large) and predicts that the dominating class in the United States will continue its policy of "social maneuvering." That is, necessary concessions will be made to the disadvantaged elements of society in order to produce those incremental changes which ensure that the problems will not assume such proportions as to result in revolution. In the short run, therefore, the authors take an uncharacteristically moderate position respecting the capacity of the United States to cope with the problems under study: While the progress made toward their resolution should not be overestimated, the book cautions, neither should it be underestimated.

The moderation also emanates from the authors' analysis of the changing significance of domestic and international factors from the immediate pre-World War II years to the present. Since there were no critical domestic crises in the United States within that period until the mid-1960s, the government was able to concentrate its attention and expend its resources on international problems. But lately domestic

problems have created internal instability and have even had a negative influence on America's international position.

The turning point was the 1968 election in which, although every politician talked about Vietnam, the authors say it was only "*one of the central issues*" (italics mine—R.M.); this fact was attested to after the election by the political analysts who agreed that the decisive role was played by domestic issues. Consequently, the authors predict that domestic politics will influence American foreign policy more in the 1970s than in the preceding decade as policy-making priorities change from foreign to domestic concerns. The obvious implication, which the authors do not make, is that since more attention will be given to "social maneuvering" and ameliorative programs are likely to increase, the system could indeed survive for some time. Within this framework the aforementioned moderation seems to be the most tenable position.

The book maintains that in these matters the American politician has little choice, as is made clear in the discussion of urban problems. In keeping with his "new federalism" concept, President Nixon would prefer to have local government tackle urban problems by giving the broadest scope to private initiative. But that proves impossible, for his conduct and policies are "no less determined by the shortage of funds or fidelity to party ideals than by the very fact that the outstanding urban and other social problems have not been resolved; for in the contemporary, quickly developing and rapidly changing world the accumulation of unsolved problems, the manifestation of conservatism, sluggishness and backwardness, are fraught with more destructive consequences in the future" (p. 119, reviewer's translation).

This volume may itself mark a turning point. It is unusual for Professor Zorin to associate himself with a book that has such a low ideological profile. He had written a series of lengthy books on American domestic politics focusing on how the monopolies control politics. He then directed the compilation of another jointly authored and highly polemical popularization of some of the problems discussed in the volume presently under review entitled, *The USA: From "Great" to Sick* (Moscow, 1969. In Russian). But now he has apparently switched his interest to the problems which, so to speak, came to control the monopolies in the sense that the monopolies were forced to make concessions by the dissatisfactions caused by the problems.

It is welcome indeed to find Professor Zorin involved in producing a volume that goes beyond the narrowest confines of ideology. The book is characterized by a breadth of treatment which can be illustrated in some remarks made about the Negro problem:

In analyzing the anatomy of protest of the Negro people it is necessary to consider the peculiarities of their historical development, the racial, nationality and class aspects of the Negro problem, the marked changes in the socio-political situation and the psychic make-up of black Americans, the ties between the white and black Americans and the policies of America's rulers in relation to the Negro people (p. 275 reviewer's translation).

Although these factors may seem obvious, they have been too often subordinated to ideological considerations in previous Soviet writing on the topic.

One hopes that this book, written under the sponsorship of the USA Institute, is a sign of change for the better in Soviet analyses of American politics, a change which may well have been engendered by the Institute's recent founding in 1968. The fact that the volume contains a large number of typographical errors in the English language citations does not detract from the contribution made toward the development of Soviet American studies.

RICHARD M. MILLS

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Twentieth-Century American Foreign Policy.

Edited by John Braeman, Robert H. Bremner, and David Brody. (Columbus: Ohio State University Press, 1971. Pp. ix, 567. \$10.00.)

This volume is the third in the series entitled, "Modern America," the purpose of which is to examine the problems of change and continuity in twentieth-century United States history. The book comprises eleven essays. Two are of a historiographical nature. "The Changing Interpretative Structure of American Foreign Policy" by Charles E. Neu analyzes the schools of thought and changing assumptions of American diplomatic history since the 1920s. "Writings on American Foreign Relations: 1957 to the Present" by David F. Trask provides a systematic survey of scholarly works on American foreign policy. The remaining nine essays explore facets of American foreign policy and diplomacy.

The variety of topics and multiplicity of approaches make impossible a proper appraisal of each individual paper in a short review, but a few comments are in order. Waldo H. Hein-

richs, Jr. in a comprehensive essay, "Bureaucracy and Professionalism in the Development of American Career Diplomacy," correctly emphasizes the triumph of careerism over amateurism. From the point of view of growth and quality this is a success story. But the coin has another side. Although American diplomats have a respected place in the competitive environment of diplomacy, the rapid progress of careerism has not increased the influence of American diplomats in the domestic political scene. While in most democratic countries the functions of foreign offices and the roles of diplomatic missions abroad have become increasingly important for a continuous and consistent foreign policy, regrettably this is not the case in the United States. The multiplication of offices and agencies dealing with foreign affairs in Washington does not facilitate the task of career diplomats. Scores of agencies have resident personnel in foreign countries who report directly to the home offices and try to influence decisions in the capital according to their special interests. The spoils system still exists. In addition, the role of some congressional committees and maintenance of a super-Foreign Ministry in the White House are symbols of the State Department's weakness in the formulation and execution of foreign policy.

Paul A. Varg in a perceptive piece, "The United States [sic] a World Power, 1900-1917: Myth or Reality?", characterizes international affairs after the turn of the century and the limited part the United States took in them. Even after acquisition of the Philippines, he notes the United States had only small interests in China, took a passive role in European affairs, but dominated the Caribbean states. It would have been interesting to appraise Theodore Roosevelt's passionate participation in international politics and the meaning of America's emergent economic power, dramatically demonstrated during and after the First World War.

The chapter on "The United States and the Failure of Collective Security in the 1930's" by Manfred Jonas is a useful contribution. The author examines the failure from an American point of view. Still, this reviewer believes that he overemphasizes the impact of neutrality legislation, especially on the Italo-Ethiopian conflict. Although this legislation might have influenced British and French attitudes, the influence was at best marginal. The decisive factor was that neither Britain nor France was willing to resist aggression. The British fleet, crowded into Alexandria harbor, had ammunition for about twenty minutes. Stanley Baldwin stated

n 1936 that he did not dare to propose rearmament because this would have ensured his electoral defeat. Since the leading democratic countries were reluctant to consider forceful action, the resolutions of the League of Nations were exercises in futility.

In a valuable study on "The United States and the Atlantic Alliance," Lawrence L. Kaplan correctly indicates some of the reasons for U.S. policy changes toward the Soviet Union after World War II. In my opinion it would have been worthwhile to point out that Soviet refusal to evacuate Azerbaijan and Stalin's speech of February 9, 1946, exacerbated Soviet-United States relations probably more than Soviet policies in Eastern Europe. The sweeping statement that "a series of status-of-forces agreements effectively placed Americans abroad on military and civilian missions under the authority of foreign courts" (p. 323) would need some qualification in view of the many variations of status-of-forces agreements and the coexistence of jurisdictions of the receiving and sending states. Contrary to the author's suggestion (pp. 329-330), one could argue that Khrushchev's ultimatum of November 1958, solidified rather than divided NATO. The task force established in Washington worked out a united policy concerning Berlin and has become a model in interallied contingency planning.

The short and excellent essay on Canada by Robert Craig Brown could have served its purpose better if the author had indicated the stages in the emergence of Canada as an independent state. The chapter dealing with recent United States-Mexican relations by Lyle C. Brown and James W. Wilkie presents events in a narrative form but sometimes overlooks the diplomatically important aspect of political acts. The authors fail to point out that President Wilson changed the traditional recognition policy of the United States when he refused to recognize the Huerta Government in 1913. The change had broader implications than using recognition "as a means of seeking protection for the lives and properties of the United States citizen in Mexico" (p. 379). Wilson's policy was an enlarged application of the Tobar doctrine of 1907. The well-written chapter on "The United States and Cuba: The Uncomfortable 'Abrazo,' 1898-1968," by Allan R. Millett is rich in historical details and source references, but it is regrettable that out of the 50 pages of narrative there are less than three pages on Fidel Castro's regime. While most chapters in the volume are conscientious research pieces by competent historians, A. E.

Campbell writes a concise analytical essay on relations between the United States and Britain, the "Uneasy Allies." The volume closes with a contribution by Warren I. Cohen, "From Contempt to Containment: Cycles in American Attitudes toward China," which offers good background material but now needs updating.

The apparent lack of a unifying theme is a shortcoming of this large volume. Probably a comprehensive introductory chapter about trends and problems of U.S. foreign policy in the twentieth century could have explained the rationale of the chapters, putting them into focus.

STEPHEN D. KERTESZ

University of Notre Dame

The Foreign Policy System of Israel: Setting, Images, Process. By Michael Brecher. (New Haven: Yale University Press, 1972. Pp. 693. \$17.50.)

This is a seminal work by an outstanding political scientist who has constructed an exquisite model and tested it in the study of the experience of a small new state with an unusually pervasive foreign policy system and baffling problems of external relations that stubbornly resist positive normalization. If sound, the model should have general application; and whether sound or not, it cannot help but excite foreign-policy theorizers. Nothing like it has ever been attempted for Israel, or indeed for any other new or old state, large or small. The model will doubtless spark other case studies. Yet in my judgment, Michael Brecher, formerly of McGill and now a visiting professor at the Hebrew University in Jerusalem, has blundered by attempting to verify in a massive book a theory that was first published in 1969 as part of an article and would have been more convincingly presented at modest monographic length.

The first of a two-part study of foreign policy making in Israel, the present work sets forth the model and seeks to substantiate it with available evidence in the record of the state's first twenty years. The second part, which will come later, promises to assess major policies on the basis of the theory. Brecher tells us (p. vii) that the book is the product

of a decade of grappling with questions of theory and methods in foreign policy analysis. The choice of "microlevel" research—the behaviour of a single actor—arose from dissatisfaction with two trends in the study of International Politics: an extensive concentration on the system as a whole, that is, "macro-analysis"; and traditional historical methods of surveying *foreign relations*, rather than analysing *foreign policy* [*italics in original*].

Existing case studies of foreign policy, Brecher argues (p. 1), tend to "amass empirical data but are usually devoid of theoretical value." Systemic inquiry "denigrates factual detail and posits a high level of mechanistic response in state behavior." And theory-based studies of foreign policy, relatively rare to begin with, "are essentially of the taxonomic type" which "develop categories for a mass of undifferentiated data," largely failing to "distinguish the crucial variables of the foreign policy process" or to "establish relationships among them." The work under review is designed to correct these inadequacies.

The author lists as the components of a foreign policy system: the operational or real environment (establishing the limits of possible choice), the communication network (chiefly diplomatic cables and the mass media), the decision-making elite, and psychological or perceived environment (describing desired choice), and the processes of policy formulation and implementation resulting from the interplay of actors and factors within and between the two environments and among many systems. Intrasytemic and intersystemic bilateral relations are also considered. Each component of the two environments and of the process receives detailed consideration. Brecher leaves no doubt from the outset that he is interested in the dynamics of the foreign policy system no less than in its structure.

The evaluation of the psychological environment includes a remarkably concise and meaningful assessment of the personalities and the socialization of Israel's high-policy elite, the eight men and one woman who in the author's view have shaped the foreign policies. The thumbnail biographies are truly brilliant. The process of foreign policy formulation and execution is also elucidated with enviable precision and attention to the involved technical elites, institutions, and methods, giving the quantifiers a rich feast of tables. By contrast, the analysis of the operational environment is too mechanistic and makes no allowance for irrationality and chance.

That environment, as Brecher sees it, consists of the global system and the Middle East subordinate system (with only passing reference to other subordinate systems) linked by the communication network. The global system, as he sees it, passed through three phases: from tri-polarity in 1948 (with Britain and its Commonwealth-Empire as the third quasi superpower) to bipolarity in 1956 and polycentrism after 1962. Alongside it, he has posited a subordinate system that never stirs from its nearly

moribund state. Its only assumed motion is the occasional movement of states from the outer ring to the periphery and from both to the core in a regional system, as viewed from Jerusalem that "comprises three concentric circles of states" (p. 48). For a scholar who is so meticulous about the calculus of foreign policy making, Brecher treats the subordinate system and its constituents much too cavalierly. Between 1948 and 1968 the Middle East system, if anything, was more dynamic than the global system, since at the start only eight of the author's twenty states were wholly sovereign, and of the later expansion and its explosive systemic consequences he is wholly oblivious. Furthermore he grossly oversimplifies the political diversity of the Middle East in cultures, structures, and ways of socialization. Bewilderingly detailed though the Brecher model is, it thus falls short of and distorts the reality.

For analytical purposes, Brecher has construed the Middle East subordinate system so as to fit the Israel case. So far, so good. But the system as seen from Cairo would look different. From there, for instance, the core circle might well include all Arab states instead of only Israel's immediate neighbors plus Iraq. As early as 1955, Egypt's Gamal 'Abd al-Nasir had become the embodiment of the Arab hope for unity, and his charisma in times of Arab-Israeli crisis (1956, 1964, 1967) tended to unify the Arab response to Israel. In the Six Day War of June 1967 even Tunisia, Morocco, and Lebanon, to name but three Arab states that have inclined toward variant positions on Israel, had to join the pack. In trying to cope with Israel, the Arab states have faced the strict alternative of multilateral or bilateral diplomacy. Indeed, Arab political terminology distinguishes between multilateralism (which rests on *qawmiyyah*, or Arab unity nationalism), and bilateralism (which rests on *wataniyyah*, or state nationalism). Arab multilateralism in dealing with Israel has been reinforced ironically by the weak and deeply divided Palestine resistance movement. This is Israel's dilemma, for the final success of its Arab policy depends on its ability to break up Arab multilateralism into bilateral components so as to deal with each separately. But so long as Palestine Arab resistance eludes total suppression—indeed, so long as the Palestine problem is not resolved—the memory and the vision of "Palestinism" are likely to keep multilateralism alive. Here again, Brecher's simplistic handling of the Middle East subordinate system leaves out of analytical purview Arab multilateral diplomacy toward Israel. What is more, the subordinate system as

seen through the eyes of the Arab foreign policy makers, which would undoubtedly have been part of the Arab psychological environment in an extended Brecher model, must be included in the operational or real environment of Israel's foreign policy makers, and its absence seriously impairs the model.

Brecher has thus created his own dilemma, since the model compels him to go too far while not going far enough. He fails, for instance, to explain how Israel's political system works insofar as it bears on the making of foreign policy. Israel's problem from the start was to find a means of converting inescapable coalitions into organs of decisive governance. This Ben Gurion contrived to do as early as 1949 in forming the first elected government. Ben Gurion's two principles for a cohesive coalition in a pluralist system included cabinet decision by majority vote and the collective responsibility of all coalition partners. It also included the invariable condition that his own Mapai or labor party, which had never returned more than 38 per cent and once as little as 32 per cent of the members to the Kneset or unicameral legislature, be given a majority of the seats in the cabinet, among them such key posts as the foreign, defense, and finance ministries, along with the premiership. By the rule of cohesive coalition government under this scheme, Mapai could, and indeed did, adopt virtually any foreign policy that it wished. To structure such governments necessitated advance negotiation among the partners until the terms of compromise as finally agreed became the immutable code of that government, enshrined as its so-called "Basic Principles" by endorsement of the Kneset. With the formation of the Government of National Unity in mid-1967—that is, in the last year or so of the period under review—Mapai lost its cabinet majority. The "wall-to-wall" coalition remained cohesive but lost its decisiveness, becoming largely immobilist in foreign policy making. If the model can explain the transformation in foreign policy making, the author does not show how.

Indeed, Brecher demonstrates the basic principle: an author's mastery of his invented model varies inversely with its complexity. Or to put it differently, the more complex the model, the more likely is the author to become its captive. Brecher deliberately eschews narrative as purely descriptive of the flow of foreign relations, in favor of developing a theory that rests on supportive evidence. As a result, he has contrived to convert a highly original conception intended for analysis into a machine for cranking out factual detail to uphold that con-

ception. More than that, he runs the risk of confusing most of his readers, except the devotees of model-building and of Israel's internal and external politics. Yet almost all model builders have no critical knowledge of Israel politics, and most of those familiar with Israel's politics, not being sophisticated political scientists, probably cannot understand the model.

Still, despite the serious shortcomings, this is a work that cannot be ignored, and in fact cannot be fully assessed until after the appearance of the second volume that promises to investigate Israel's foreign policies at the strategic level.

J. C. HUREWITZ

Columbia University

Basic Documents on Human Rights. Edited by Ian Brownlie. (Oxford: Clarendon Press, 1971. Pp. x, 531. \$11.25.)

The Human Right to Individual Freedom: A Symposium on World Habeas Corpus. Edited by Luis Kutner, (Coral Gables, Florida: University of Miami Press, 1970. Pp. 249. \$12.50.)

These two books represent the two main approaches to international human rights problems. The first one is pragmatic, the second one programmatic; the first one provides a guide to the existing law, the second one proposes important changes in the law.

Brownlie's book contains some sixty basic documents on human rights, national and international, from 1688 to 1969. In the international field, he includes not only the documents of the United Nations and its specialized agencies, but also those of various regional organizations. The author provides short but pithy introductory notes to each chapter and to each document, with a few bibliographical references. Most of these notes are factual, but some of them are quite passionate (e.g., on Latin America, p. 387). Sometimes the author is too cryptic, and does not refer to some crucial facts. For instance, his note on the Indian Constitution (p. 29) does not alert the reader to the fact that several changes in the Indian Constitution had to be made to reverse judgments of the Supreme Court which tried to uphold rights of individuals against state interference. In discussing the Freedom of Association Convention (p. 279), Brownlie does not mention the many cases which have arisen under it and which are discussed in excellent books by Jenks and Haas. Many documents do not indicate the source from which the text was taken (see, e.g., pp. 8, 14, 29, 46, 56, 339).

These are, however, minor flaws. The collec-

tion is excellent and should be useful to all teachers and practitioners in this rapidly growing field.

The second book is a collection of essays—some reprinted, some new—by Mr. Krutner, an eminent Chicago lawyer, who since 1931 has single-mindedly pursued the idea of a “world habeas corpus,” an international legal remedy against arbitrary detention of individuals. Over the years he has written several books and many articles on the subject. In the current volume, he has put together endorsements of his idea by prominent lawyers from the United States and other countries. Some of these essays—for instance, those by Justice Douglas and Mr. Tran Tam—are only marginally relevant to the main topic of the books. Many of the others are general and superficial. Some of the more thoughtful authors are rather doubtful about the feasibility of the whole project, even though they endorse its desirability (see, e.g., Müller, pp. 44–45; Benattar, p. 56; Q. Wright, p. 169; Sutton, p. 179).

In his concluding essay, accompanied by a draft “treaty-statute,” Mr. Kutner makes a good case for the development of international due process of law and for using the historical writ “habeas corpus” as a means for terminating any violations of the basic rules for protecting individuals against abuse of power by the government. In many respects, the essay is much clearer than the rather confusing “treaty-statute,” which does not reflect the latest evolution in the author’s thinking.

One can hardly disagree with the author’s goal, but in this era of growing rather than declining nationalism it is difficult to expect governments to accept such limitations on their freedom of action. The states which might accept his proposals are likely to be those that already apply the rule of due process in their internal affairs; on the other hand, it cannot be anticipated that the many authoritarian regimes which need such international control most would join a system of international courts for the protection of rights of individuals.

While Mr. Kutner mentions in his essay the existence of the European Commission of Human Rights and the European Court of Human Rights, he has not taken their work into account, though they dealt effectively with many cases of arbitrary detention. In the long run, the best hope seems to lie in following this successful example and establishing similar institutions in other parts of the world, rather than continuing on the path blazed by Mr. Kutner. In a sense, events have overtaken him, and it might therefore be necessary for him to study

more deeply the European experience and to recast his project accordingly.

LOUIS B. SOHN

Harvard Law School

American International Law Cases, Vol. I.

Edited by Francis Deak. (Dobbs Ferry, N.Y.: Oceana Publications, Inc., 1971. Pp. 484. \$40.00.)

This is the first of a projected series of eight volumes of cases bearing on international law, decided in the courts of the United States: federal, state, as well as Claims, Military Appeals, Customs and Patent Appeals. A grant from the Carnegie Endowment for International Peace enabled Dr. Deak, on his retirement from Carnegie, to launch and continue this monumental work up to his untimely recent death.

The important role of a national courts in the development of international law is too well expounded to require adumbration here. (Cf. Falk, *The Role of Domestic Courts in the International Legal Order*, [Syracuse: Syracuse University Press, 1964.]) One need perhaps only whisper to potential readers the warning that national courts are instruments of national government and that the international law they purport to dispense is circumscribed by acts of legislative fiat, manifestation of executive pleasure or displeasure (as in recognition), and judges’ readings of the national interest and public policy. (Cf. Franck, *The Structure of Impartiality* [New York: Macmillan, 1968], espec. chap. 6: Denationalization of International Disputes, pp. 212 ff.). On the other hand, were it not for the decisions of national courts, international case law would be slim pickings for the law student. Perhaps this dilemma may in due course be resolved by permitting appeals or references to the International Court of Justice from national courts in international legal disputes between individuals affecting the jurisprudence of more than one state. This daring suggestion is persuasively made by Judge Philip C. Jessup in a recent work. (Jessup, *The Price of International Justice* [N.Y.: Columbia University Press, 1971.])

Meanwhile, one must be grateful for the diligence, by and large, of the U.S. courts in seeking to apply international law as part of the law of the United States, and in doing so with a tolerable minimum of chauvinism, cultural myopia, or excessive deference to the transient political preferences of the legislative and executive branches. It is this which emerges from Volume I of this series, the topic of which is “International Law in General.”

The book, in sum, maps out the results of

the process by which cases with an international aspect are decided in United States courts. Out of these cases, some quite dramatic, emerge doctrines that have gradually won acceptance among states, as well as others which are "international" only in an approximate sense. Moreover, the cases reveal how significantly international relations and law affect the interests of private American citizens. And, finally, they demonstrate that even quite complex and politically "hot" issues of recognition, nationalization, and sovereignty can be resolved by courts, not always with perfect impartiality but, at least, in such a way as to obviate recourse to violence: a lesson private individuals seem readier to learn than are national leaders. Since the book is in casebook format, with little in the way of formal notes, readers are left to make these deductions for themselves.

The series promises to be a useful counterpart to Professor C. Parry's *British International Law Cases* series, and Marjorie Whiteman's (U.S.) *Digest of International Law*. Nevertheless, this newest entry into the Anglo-American trio engenders minor reservations. The blurred and uneven type ranges from blow-ups of U.S. Supreme Court Reports that resemble a N.Y. Times Edition for the Visually Impaired to microscopic Federal District Court decisions, all rendered photographically. All this might be forgiven in the name of economy. But at \$40.00 for approximately 500 pages, economy can scarcely be the altar at which style has been sacrificed.

THOMAS M. FRANCK

New York

Economic Sanctions and International Enforcement. By Margaret P. Doxey. (London: Oxford University Press, 1971. Pp. ix, 162. \$4.50.)

This brief study presents a readable survey of the structures, applications, and problems of economic sanctions. Professor Doxey begins with an examination of the institutional framework available for international enforcement of sanctions by comparing the League of Nations with the United Nations. She calls attention to several major differences, which include: a difference in universality of membership, a more far-reaching and explicit renunciation of the use of force in the Charter, a tightening of the former freedom of League members under Article 16 to take enforcement action by giving primary responsibility for the maintenance of peace to the U.N. Security Council, broader circumstances under which enforcement measures may be taken under the Charter, and an

attempt to integrate regional agencies into the U.N. system by allowing them controlled enforcement powers. She notes, however, that "in spite of these differences between the League and the United Nations, which appeared to make it easier for the latter to fulfil [sic] its purpose of preserving peace and security, the record of the United Nations reveals the limited role it has played" (p. 12).

Professor Doxey next considers the development and application of economic sanctions at the national, regional, and universal levels from the time of traditional siege and blockade tactics to modern times. Two points are made particularly well in Professor Doxey's discussion. First, she acknowledges the difficult methodological problem in measuring the apparent results of economic sanctions against the actual sanction actions undertaken. Second, she draws a necessary distinction between economic warfare and economic sanctions, although the techniques employed (and perhaps the results obtained!) in each are similar. As a measure undertaken as part of a war effort, the object of economic warfare is "to hasten the enemy's defeat, to reduce or eliminate its capacity to wage war, to undermine morale, and generally to make life for its citizens as difficult as possible" (p. 14). On the other hand, economic measures used as sanctions are intended "to deter or dissuade states from pursuing policies which do not conform to accepted norms of international conduct. Compliance is considered to be in the general interest and the sanctions are penalties which relate specifically to acts which the international body condemns" (p. 14).

The record of a spectrum of economic measures is reviewed, including experience during the two World Wars, the West's strategic embargoes in Europe and the Far East, the Arab boycott of Israel, Soviet and Eastern European boycotts of Yugoslavia and Albania, and OAS and U.S. sanctions against the Dominican Republic and Cuba. The study is particularly strong in discussing and evaluating the two major experiments with economic sanctions at the universal level—the League action against Italy for its efforts in Ethiopia, and the United Nations action following the United Kingdom's difficulties with Rhodesia.

The results of both the League and United Nations actions confirm what other experiences with economic measures had already largely suggested, namely that the use of economics as a political weapon is hardly reliable, owing to a discouraging array of problems. There is the basic difficulty of consensus on the existence of a problem and on the responsibility of a certain

state (or states). Then there are decision-making problems in translating that consensus into action, including, *inter alia*, agreement on goals and selection of measures to be taken.

These difficulties are compounded by the reactions to the sanctions by the target state(s) and its (their) particular economic circumstances. The state may be able to anticipate sanctions and prepare itself, may be economically self-sufficient, may be strengthened internally by the development of a siege mentality, may be able to evade the sanctions, or may even be able to level countersanctions. Finally, there is the danger that sanctions will punish the wrong parties.

The ideal object of an international economic sanctioning effort would be a weak island state with no international friends and a highly vulnerable economy. The author concludes that economic sanctions have not worked as intended, and she suggests that their purely punitive rationalization be reviewed and that greater world attention be paid to the kinds of things that caused a state to undertake policies meriting sanctions.

FOREST L. GRIEVES

University of Montana

The Kennedy Round in American Trade Policy: The Twilight of GATT. By John W. Evans. (Cambridge, Mass.: Harvard University Press, 1971. Pp. 383. \$13.95.)

This volume is a masterly review of the commercial policy of the United States over the past 25 or 30 years, focusing on the Kennedy Round. It is written from the standpoint of a U.S. government official who had a significant role in executing that policy.

The first third of the book is given over to a description of the main issues and problems of commercial policy in the period preceding the passage of the Trade Expansion Act of 1962, which was the basis for U.S. negotiations in the Kennedy Round. Here, and throughout the book, the author shows the major role of "reciprocity" in international negotiations concerning tariffs and related matters. Each action permitting freer access to a country's markets has been regarded as a boon to other countries for which they must compensate by giving freer access to their markets. The guiding principle of the negotiations has always been to achieve a balance of benefits, although it must be done in such a way that each set of negotiators can return to the home country with the claim that they have gotten a better deal for their exports than they have accorded to the exports of the

other countries. The latter objective is aided, as the author states, by the vagueness of scoring in the game, since the technical information necessary in order to assess the effects of any trade concession is simply not available. Nondiscrimination, the other major principle underlying the system of world trading relationships that has emerged in the postwar period, is also treated in this section. The author describes the provisions of GATT and the conflict between nondiscrimination and economic integration as represented by customs unions and free trade areas. Problems of agricultural trade, nontariff barriers, and the trade of less developed countries are also examined.

Most of the rest of the book is given over to the antecedents of the Trade Expansion Act in the United States and the Kennedy Round negotiations. Such familiar controversies as those surrounding the escape clauses and the peril points are briefly reviewed. The protracted and extremely difficult negotiations over the ground rules for the Kennedy Round and then the Kennedy Round negotiations themselves and their outcome are described. The Round's substantial success may be summarized by pointing out that the weighted average reduction in tariffs by the United States on dutiable nonagricultural imports was about 35 per cent and that reductions by the European Economic Community were roughly similar. This may be compared with weighted average U.S. reductions in tariffs of less than 19 per cent in the first round of multilateral negotiations (Geneva, 1947) and from two to four per cent in each of the four following rounds.

The strength of this volume is the author's broad knowledge and understanding of world negotiations on trade questions from the standpoint of an American participant; but whatever weaknesses the volume has also stem from this position. In general, the author provides a skeptical review of issues where skepticism was involved in the U.S. position, as, for example, with respect to the trading claims of the less developed countries about the impact upon them of U.S. tariff actions. There is less inclination to assess critically some of the more basic aspects of the U.S. position. For example, the author reports that item-by-item negotiations for mutual tariff concessions worked well as long as tariffs were high enough to give a comfortable cushion against competition. He describes the results as impressive without stopping to ask what benefits ensued if water were simply being squeezed out of tariffs. This is not to deny that there were some benefits; it is only to

say that it would have been desirable for the author to assess them. Also, in his account of the U.S. violation of GATT through the imposition of restrictions on the imports of dairy products, the author does not suggest what seems to me to have been the traumatic impact of this action on the attitude toward GATT held by the other countries of the world. The United States had used its enormous power and prestige in the early postwar years to obtain worldwide sanction for the new rules of commercial policy embodied in GATT, but in this action it took the lead in demonstrating that the new rules were secondary to internal political requirements for protection.

One can also find other matters to quibble about. For example, the electronics and color television industries, even when they were being established, were hardly appropriate illustrations for what is usually referred to as the "infant industry" argument for protection (p. 25).

Nevertheless, when all these objections and quibbles are entered, the fact remains that this is an extremely useful book that is likely to become one of the standard reference works on the Kennedy Round and on American commercial policy of the period.

IRVING B. KRAVIS

University of Pennsylvania

Die Vierte Welt: Modell einer neuen Wirklichkeit. By Bruno Fritsch. (Stuttgart: Deutsche Verlags-Anstalt, 1970. Pp. 212. 19.80 Dm.)

To gaze into the crystal ball and divine the future has become a profitable venture. Alvin Toffler, of *Future Shock* fame, Herman Kahn, Daniel Bell, George Orwell, and others dabbling in futurology have aroused the public's curiosity about such target dates as 1984 and 2000. Each author has his or her peculiar vision ranging from a blessed utopia to a pending doomsday. Into this motley crew steps Bruno Fritsch, a Swiss academician, who heads the Institute of Economic Research at the Zurich Technical University, has taught at Harvard, and has visited a number of American research institutes.

Fritsch has written a challenging but also disappointing book about the contemporary international system and a possible future system, one bearing little resemblance to the other. The reader will be challenged by a number of provocative ideas of a future society, but will be disappointed by the short space allocated to them. Three of the four chapters deal with present trends, and only one with the "Fourth

World"—the world of the future. Such a proportion seems unobjectionable, except that the three chapters deal with matters often fairly well known to the international relations specialist.

The author first sketches the post-World War II era in which a population explosion has paralleled an explosion of new states, in which the share of exports of developing countries is declining in world trade, in which economic underdevelopment is concentrated in much of the world, and in which social underdevelopment is even more widespread. He explores the pattern of foreign investments, including that of American corporations successfully seeking new markets in which they make use of their technological and organizational superiority. He reminds the reader that corruption, exploitation, inequality, and wars are not restricted to any area of the globe; neither to the First World (the "West"); the Second World (the Communist bloc); nor to the Third World. He surveys the planning which has taken place on the national and supranational levels to overcome some of the world's ills and reduce the gap between rich and poor nations. While a global planning system and international solidarity might be able to narrow the gap, the political difficulties are formidable. Rich nations, beset by their own problems, are unwilling to help significantly the poorer nations; with the result that only a minimum of economic and social progress has taken place among the poorer countries.

But despite these difficulties, Fritsch is convinced that planning is imperative in such fields as population, education, and agriculture if technological breakthroughs are to be registered. There are enough agencies at the regional and United Nations levels to perform these tasks, but they often do not operate efficiently.

The author deals in the last forty pages with a number of "Fourth World" models, ranging from the Marxist conflict model to the convergence model. He assumes quite realistically that in the future it will be nearly impossible to eradicate economic gaps, military conflicts, and resource shortages. These difficulties will be compounded by authority crises within nations and by groups identifying transnationally rather than with their governments. In order to ease potential aggressiveness and conflict situations, he opts for a strange mixture of pleasure pills, teaching computers, participation in public affairs, technological forecasting, satellite communications, and computer-linked "response

systems" which will add up individual values and preferences. He forecasts the decline in national loyalties and the growth of new subsystems based on transnational social experiments (communes, etc.).

Whether the world will move in this technocratic direction and witness an evaporation of the national state system remains to be seen. Fritsch has tantalized the reader with these all-too-brief speculative glimpses into the future. Now he should write a full-length book about the "Fourth World" as he sees it.

GERARD BRAUNTHAL

University of Massachusetts/Amherst

The Legal and Practical Aspects of Trade with the Soviet Union. Revised Edition. By James Henry Giffen. (New York: Praeger Publishers, 1971. Pp. 366. \$19.50.)

With major Soviet-American trade breakthroughs imminent, the updated edition of this 1969 book will be welcomed by American exporters, their legal advisors, and political and social scientists interested in the economic and microlegal aspects of the burgeoning Soviet-American détente. The author, a lawyer (with a B.A. in political science) and a leading private consultant on trading with the U.S.S.R., has had extensive field and negotiating experience in the Soviet Union. Writing in the Introduction, Senator Walter F. Mondale, a leader in liberalizing East-West trade, has quite aptly characterized the book as a "practical guide" (p. xix).

This is a competent survey of U.S. and Soviet civil law relevant to foreign trade, with the emphasis on contract law. In presenting Soviet law, the author has drawn heavily on English translations of Soviet civil law and commentaries and, to a lesser extent, on the standard American authorities on the subject such as Berman, Grzybowski, and Gsovski. The book is organized to take a private American businessman step by step through a trade transaction with a Soviet governmental trading agency. Chapters on U.S. export and import regulations are followed by chapters on the Soviet political and legal system as it bears on foreign trade, the art of negotiating with Soviet trade officials, and the legal procedures of contracting, making payment, and protecting private industrial property rights under Soviet law.

This specific and detailed scenario is further buttressed by numerous appendices, tables, and figures. In particular, there are 43 appendices comprising nearly a third of the book which make plain the author's practical intent. Most of them are photocopies of the relevant U.S.

export and import forms and regulations. The remainder are photocopies of the English language versions of the appropriate Soviet forms, including sample import and export contracts, a standard purchase order, and typical arbitration clauses and licensing agreements used in Soviet international commercial transactions.

Because this is basically a handbook, Mr. Giffen's efforts are mainly descriptive and exegetic and rarely interpretative and evaluative. In chapter 4, however, which deals with proposed U.S. legislation on East-West trade, the author becomes somewhat more venturesome and gives the reader some insight into the underlying issues. He raises and effectively demolishes the standard objections to liberalizing U.S. trade with the Soviet Union. Interstitially and at best schematically, Mr. Giffen also lays out the basis of his own commitment to expanded Soviet-American trade. This rests on the familiar positions of easing "cold war tensions" (p. 97), helping the U.S. balance-of-payments problem (because the U.S.S.R. is expected to import from us more than she exports), taking profits (which other non-Communist countries will take anyway if we don't), and the more elusive notion of availing ourselves of the "opportunity to gain concessions and influence attitudes in the Soviet Union . . ." (p. 91).

The author has made no adequate attempt to examine critically his positions, some of them containing the troublesome snags upon which the ongoing Soviet-American trade negotiations are hung up. His basic argument that trade relaxes tensions is really no more than a hypothesis (as yet untested in the Soviet-American context) masquerading as a cliché of the East-West dialogue. Personally, I hope the cliché becomes reality, but professionally I can't ignore the possibility that substantially increased economic contact between two such disparate systems might also yield misunderstandings, mutual frustration, and irritating diplomatic wrangles over misperceived promises, broken contracts, and failed agreements. Although I hope this alternate hypothesis is not confirmed, I think that even a dedicated integration analyst (whose approach this book implicitly takes) would have to concede that it enjoys the same logical status as Mr. Giffen's hypothesis—until actual experience bears out one or the other.

As for the author's positions on the balance-of-payments problem and profit taking, a series of antecedent questions first have to be raised and resolved before his projections could become operative. For instance, what are the Soviets selling that we need besides certain rare

metals? Assuming that they buy more from us, how large an imbalance-of-payments problem can we expect the Soviet leadership to tolerate and for how long a time? In view of the U.S.S.R.'s chronic shortage of hard foreign exchange, can we expect the larger American firms to accept their invitation to invest in the mineral exploitation of Soviet Far East, helping offset the cost of imports from the U.S.? Even more fundamental, will this administration or the next respond to Soviet requests for long term credits to finance the imports? These are some of the issues still unresolved in the current Soviet-American trade negotiations.

Mr. Giffen's most dubious point is that a trade dependency on the United States may give us some leverage for influencing Soviet politics. As he himself states, foreign trade as a state monopoly is a highly manipulable aspect of Soviet foreign policy. When necessary and expedient, the Soviet leadership has tended to compartmentalize and isolate their foreign economic policy. Witness the 1920s and 1930s, when the U.S.S.R. imported massive amounts of "Western" machinery and technology for industrialization while simultaneously maintaining toward the "West" cool to hostile political postures as well as implacable ideological hostility. Consequently, the broadening Soviet-American economic contacts at this time will not necessarily be accompanied by political spillovers.

As an advocate whose business it is to promote more trade with the Soviet Union, Mr. Giffen has done his job well in this book. He has convincingly demonstrated that one can do business with the Russians, but at the cost of glossing over the inevitable problems of trying to mesh public and private property economies and legal systems, each set within quite distinct political and legal cultures.

ROBERT SHARLET

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The American Law of Sovereign Immunity: An Analysis of Legal Interpretation. By Theodore R. Giuttari. (New York: Praeger Publishers, 1970. Pp. xvii, 438. \$20.00.)

The classic case of *The Exchange*, 11 U.S. (7 Cranch) 116, 136 (1812), in which Chief Justice Marshall spoke of the "exclusive and absolute" jurisdiction of the territorial state and noted that all exceptions to it derived from its consent, has long served as the basis of the doctrine of sovereign immunity in the United States. Hence a foreign state and its instrumentalities could not be sued in the courts, and the principle was regarded as "absolute." In *Berizzi*

Brothers Co. v. S.S. Pesaro, 271 U.S. 562 (1926), a unanimous Supreme Court even dismissed a libel against a public commercial vessel owned and operated by the Italian government.

In the ten chapters into which this book is divided, the author, a practicing attorney, traces the historic and legal evolution of the doctrine. His plan has two basic objectives: first, to show the shift from the "absolute" to a more "restrictive" theory; and, second, to evaluate the interrelationship of the three branches of government on the principle.

After an examination of nineteenth- and early twentieth-century practice during which states engaged in few commercial enterprises, Dr. Giuttari draws attention to the ever-increasing activities following the first world war. And while the Department of State at that time merely served as a conduit and favored the restrictive principle, the issue of immunity was regarded as a legal issue, and the judiciary advanced the absolute doctrine. In the 'thirties the courts on their initiative placed certain restrictions on the doctrine, primarily through the concept of waiver. But the major change which soon emerged was to consider the question political rather than legal. Although Chief Justice Stone based denial of immunity in *Republic of Mexico v. Hoffman*, 324 U.S. 30 (1945), on the absence of possession of the vessel by Mexico, he noted by way of dictum the predominance of an executive determination.

The next significant event was the Tate letter of 1952 in which the Department announced that henceforth it would follow the restrictive theory for private or commercial activities (*acta jure gestionis*) and expressed the hope that the courts would act in a similar manner. Widely hailed when issued, the pronouncement raised more questions than it answered. To be sure, both branches have frequently pursued the restrictive doctrine and the courts have limited political or public acts (*acta jure imperii*) to a few categories. The author concentrates on this modification and emphasizes that the letter "consolidate[d]" judicial trends in terms both of the restrictive theory and of judicial deference (p. 359). This may be something of an oversimplification. Even Dr. Giuttari acknowledges "[o]ccasional" deviations from the former tendency, deviations which he has termed "relatively significant" (p. 359). He also rightly observes that in *National City Bank v. Republic of China*, 348 U.S. 356 (1955), the Bench sustained an independent counterclaim. At the same time the Executive still adheres to absolute immunity from execution, a policy pre-

dominantly but not exclusively pursued by the courts as well.

On the basis of a number of developments—the Tate letter and other executive suggestions, recent treaty practice, the U.S. position before foreign courts, the impact of *Republic of China*, the independent rationalization of lower federal and state courts in support of the restrictive rule, and the “guidelines” in *Banco Nacional de Cuba v. Sabbatino*, 376 U.S. 398, 437 (1964)—the author suggests that “adherence both to the absolute doctrine of immunity and to the principle of complete subjection to the executive may no longer be in vogue” (p. 310).

The view concerning *Sabbatino* is particularly interesting. Dr. Giuttari repeatedly declares that the reasoning of the case may be employed *mutatis mutandis* in relying “less heavily” on the Executive in sovereign immunity suits (p. 366). Now, *Sabbatino* involved the act of state doctrine which the Supreme Court applied because it found the issue of the validity of foreign expropriations a twilight zone of international law unsuitable for its adjudication, and declared that the “national interest” and the international “rule of law” are best served by employing the doctrine and leaving resolution of the matter to the diplomatic sphere. The Bench reached its own determination; it did not hold that the Executive could tell it what to do; it did not rule as it did because of an executive determination. Indeed, there was none. On the other hand, the Solicitor General’s brief on behalf of the United States as *amicus curiae* favored the diplomatic approach and stated that the Court should not hold that “executive silence” concerning the act of state doctrine meant “executive approval of judicial inquiry into the foreign act,” 2 *I.L.M.* 1009, 1014 (1963). And the brief, which this well-researched book has overlooked, contains some of the reasoning and language found in the opinion.

Six years later, however, the Legal Adviser wrote to the Court that the doctrine should not be applied to bar adjudication of a counterclaim or set-off. The Solicitor General in two memoranda filed as *amicus curiae* on writs of certiorari declared that the Court of Appeals’ application of the doctrine under such circumstances “seriously” impaired the Executive’s power over the control of foreign affairs, 10 *I.L.M.* 1191, 1192 (1971); 11 *I.L.M.* 27, 28 (1972). When in *First National City Bank v. Banco Nacional de Cuba*, 406 U.S. 759, 92 S. Ct. 1808, 1813, 1816, 1824 (1972), a closely

divided Bench reversed for a second time, three members of the majority held that where the Executive “expressly represents to the Court” that the doctrine “would not advance the interests of American foreign policy,” the courts should not apply it, and for their part adopted and approved the narrow *Bernstein* exception, 210 F. 2d 375 (2d Cir., 1954). In so doing, these three justices stated that their holding was “in no sense an abdication of the judicial function to the Executive Branch” to which the dissenters responded that “it seems patent that the contrary is true.”

Of course, the doctrine of sovereign immunity is one thing; the act of state doctrine another. Yet through such tendencies the immunity principle may witness further erosion not because of the rationale of *Sabbatino*, but as a result of still greater judicial deference. This could prove most unfortunate for a doctrine which is still subject to political considerations and not necessarily the requirements of international law, and could contribute to the judiciary’s having to “receive,” as Mr. Justice Powell remarked in *First National City Bank*, “the executive’s permission before invoking its jurisdiction.”

If viewed from an overall perspective, the book contains certain inexplicable statements. Among these is the comment that the Constitution has set forth congressional/executive roles with “relative clarity” with respect to “treaty-making or diplomatic appointments” (p. 23, n. 10), which is far from the case. Such defects are minor. Of greater import is the format of the volume. Unfortunately, much of the analysis is found in the notes, and this makes for rather awkward reading, especially for those not schooled in the law. Nonetheless, the study is a most valuable contribution to the literature and of considerable interest from the international and constitutional standpoints.

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The Challenge to U.S. Dominance of the International Corporation. By Rainer Hellmann. (New York: Dunellen Publishing Company, 1970. Pp. 348. \$17.00.)

Sovereignty at Bay: The Multinational Spread of U.S. Enterprises. By Raymond Vernon. (New York: Basic Books, 1971. Pp. 326. \$8.50.)

Fads reign supreme in the world of scholarship and research as they do elsewhere, and

quite recently the multinational corporation has become another fad, producing the usual body of polemics, apologetics, and every now and then some competent and enlightened analysis. Yet despite the popularity of the subject few contributions have been made by political scientists, and the study of the international system seems to reflect the state of its domestic counterpart: private power goes unexplained and unnoticed, and it is extremely difficult to come to grips with.

The two volumes under review raise some questions about the multinational corporation which are of potential interest and importance to political scientists. Ultimately, however, and because they are the product of other disciplines and skills, they only go so far, and it seems a bit unfair to expect others to do what political scientists themselves have so far failed to accomplish. Hellmann's book is written by a German economist with a European emphasis and point of departure. Vernon's is the work of one of the leading scholars in international business, the coordinator of Harvard's Project on Multinational Enterprise, to date the most ambitious and extensive research effort on the multinational corporation. Their high quality notwithstanding—and particularly the quality of the Vernon volume—both books adequately demonstrate some of the pitfalls and problems inherent in the study of the multinational corporation. I shall return to this point after presenting their main arguments and findings.

Despite the rhetoric of multinationality the giant corporations that dominate the world industrial order remain, for the most part, national entities domiciled in the world's centers of wealth and privilege. The U.S. dominates the scene, and that is what makes Hellmann's study both timely and important. In a way it could be seen as another European perspective on the American economic presence in Europe, one more optimistic than Servan-Schrieber's popular argument in *The American Challenge*. The burden on Hellmann is essentially to demonstrate the "pluralist" nature of the multinational corporate phenomenon, or more precisely here, its non-American dimension. He fields useful data carefully assembled and documents what is known to most through common sense and intuition: America is the dominant force here accounting for 60 per cent of direct foreign investment—the principal vehicle of multinationality—while Europe is a distant second with about a third of the total. All others are minor participants in this game or mere spectators. The logic of the analysis and the

findings here lend no support to a major challenge to the U.S. dominance of international business. They only demonstrate that Europe is still viable, and that a European response to the American challenge is still a possibility. Europe's chances here according to Hellmann would depend on four factors:

(1) the ability and the willingness of the European firms to invest in the United States; (2) the readiness of European states not to complicate these investments but moreover to promote them; (3) the investment climate which the European will find in the United States; and (4) the American willingness to dismantle existing administrative barriers to foreign enterprises (pp. 159–160).

Hellmann lends his support to a European body of opinion, which is quite influential in business and government circles, namely that European firms have to emulate their American counterparts if they are to grow and compete successfully in the world economy. The remedy prescribed here is mergers and greater size, and governments are expected to encourage and facilitate giantism and industrial concentration. And reminiscent of the functionalist logic, Hellmann sees in economic integration the answer to Europe's problems. Otherwise, he argues, European firms "will have to rely on national partial markets which are too small a home basis for their international expansion" (p. 314). The choice presented for Europe by Hellmann is rather confining and deterministic: industrial giantism and economic integration or acceptance of American and Japanese dominance of world business. The thought that giantism and bigness in world business might be undesirable and that other alternatives need not be ruled out does not even occur to him.

Giantism in business is also Vernon's point of departure. His study is concerned with the multinational dimension of U.S. enterprises, but his universe is restricted to 187 manufacturing firms. The figure was arrived at by applying two criteria for inclusion: membership in *Fortune's* group of 500, and a significant degree of multinational activity as indicated by having manufacturing subsidiaries in six or more countries. The result is a highly selective and elitist club, but nonetheless a very powerful and important one. Vernon's data bear out their prominence and distinction. The 187 enterprises, when compared to the rest of U.S. manufacturing enterprises, were generally more profitable, dedicated a higher percentage of their sales to R & D efforts and advertising outlays, tended to involve greater use of skilled technical manpower, and were generally more

diversified in their activities. In short, they were according to Vernon a "group of enterprises usually associated with oligopoly" (p. 12). And they are even more prominent if one looks at the linkage between America and the world economy. Vernon demonstrates the heavy involvement of his sample of 187 firms in export-oriented industries and products, their contribution to the U.S. balance-of-payments, and their dominant position in the foreign direct investments of U.S. firms. He estimates that this group alone accounts for as much as 80 per cent of the overseas liquid assets of U.S. firms. The last point is quite suggestive and important: multinationality and foreign investments are the domains and the activities of industrial giants and worldwide concentration is thus bound to become the most distinguishing feature of the world economy unless the current trends are reversed.

Like Hellmann, but in a much more systematic and analytical fashion, Vernon is sensitive to the impact of the multinational corporation upon the nation-state system and to the problems of interface between these two forms of organization. His major assumption is one of a basic asymmetry between them which calls for a degree of global regulation and control. The conflict here, we are told by Vernon and by much of the literature on the multinational corporation, derives from the flexibility of the multinational firm, the territorial nature of the nation-state, and the obvious desire of the latter to maintain a reasonable degree of autonomy in its economic activities. But Vernon ignores other possibilities and realities here, and minimizes the symbiotic relationships that develop between business and governmental elites. One need not probe too far to note the close symbiosis between giant corporations and the political order in most of the advanced industrialized countries. Nor can we ignore the alliances that develop between the subsidiaries of giant multinationals and the established guardians of order in the host countries of the Third World. Vernon is vulnerable here precisely because he sees the multinational corporation as a destabilizing force posing a major threat to established elites and vested interests. National authorities, it could be argued, both in the home and host countries, can coexist with multinational corporations to the mutual benefit of both. Policies can be pursued and regulations made which advance the interests of giant firms and governmental elites in both home and host countries. The major concern here would be the social costs to be borne by the powerless of the world's population. Inflated prices, a deteriorat-

ing environment, and much more centralized and inaccessible structures of power and authority, will accompany and indeed have already accompanied the spread and rise to power of the multinational corporation.

Another topic that Vernon attempts to treat is the cultural impact of the U.S.-based multinational subsidiaries. And here his analysis is excessively sketchy. He sees a potential clash of cultures, and applies this concept to the different cultural styles that characterize the interaction between U.S. firms and several industrialized countries. In so doing he overlooks, in my judgment, not only the cultural impact of the multinationals upon the weak and impressionable societies of the Third World, but also the cultural content and dimension of the multinationals and of their products. Clearly there is a life style imparted by the multinationals, and the desired outcome is the expansion of the world consumer system and the world comfort culture. We must wait for someone else to undertake this kind of analysis, and it is not in this area where Vernon makes his best contributions. He is at his best where he provides us with a painstaking analysis of the U.S. manufacturing subsidiaries and raw material ventures and an excellent discussion of the personality and dynamics of the multinational corporation.

The following general remarks are as much of a commentary on the two books under review as they are upon the major thrust of thinking about the multinational corporation. Both books reveal a certain fascination with corporate giantism and bigness. The costs of such bigness are rarely discussed. It is strange, indeed, that Vernon for example, notes in passing that his sample of multinationals is associated with oligopoly but fails to discuss the features and costs of a world economic order dominated by giant oligopolists. Furthermore, like a good deal of writing on the multinationals, the two studies here devote very little if any attention to the social costs of multinational corporate activity, and the entire notion of social costs and welfare implications does not even seem to have occurred to Hellmann. Vernon makes occasional hints and infrequent references, but that is hardly sufficient. A final criticism already stated but worth repeating is the failure of the Vernon and Hellmann studies and much of the literature on the multinationals to note the culture and ideology imparted and implied in multinational corporate activity. Unless the concern with the multinationals moves in these broad directions it will prove to be another short-lived fad whose scholarly out-

come is as questionable as its normative priorities and commitments.

FOUAD AJAMI

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The Bear at the Gate: Chinese Policymaking under Soviet Pressure. By Harold C. Hinton. (Stanford: Hoover Institution and the American Enterprise Institute, 1971. Pp. 112. \$3.00.)

The developing rapprochement between Washington and Peking is surely one of the more significant international developments of our time. This monograph by an acknowledged specialist on Chinese foreign policy is therefore particularly relevant, for it analyzes the course of events that led the Chinese Communist side to such an important breakthrough. The main Chinese motive was supplied by the increasingly threatening conflict with the USSR, and the need to find a Great Power counterweight through better relations with the United States. Sino-Soviet tension had seemingly reached the boiling point with the serious fighting along the border in March, 1969. In this study Professor Hinton describes admirably the way in which the Chinese leadership coped with this crisis, easing a dangerous military situation into a protracted stalemate at the conference table, with the Nixon invitation as a final counter. Thus we have a study of Chinese Communist behavior in which the Chinese are found to be skillful at crisis management, though occasionally rash in initiating crises (p. 9).

Like the author's previous books, this briefer work demonstrates his gift for lucid synthesis, encyclopedic knowledge of the internal and external politics of the communist world, and flair for ingenious—sometimes overingenious—speculation. For example, on page 5 Hinton has Chou En-lai arguing in "intra-party debate" that Lin Piao was endangering Chinese security both by his unpopularity in Moscow and by his opposition to the opening to the United States. Yet one wonders whether the latter might not mitigate the former; if Lin Piao was a last-ditch opponent of detente with Washington, might he not have been more acceptable to Moscow than the wily Chou En-lai, for just that reason? This argument is strained, of course, but suggests how much guesswork must be involved in either interpretation. Also in evidence is the author's penchant for seeing intended political significance in almost every date as the "anniversary" of something else, a subtechnique of Kremlinology that Professor Hinton has developed into a virtual art form (see pp. 5 and 70 for examples). This idiosyncrasy, however,

does little harm to a book which, in style and execution somewhere between scholarship and an intelligence report, will be indispensable for the specialist and informative for anyone.

The Introduction and first chapter provide an overview describing the development of Sino-Soviet friction through the 1969 border clash and its denouement. Much of this ground has been covered elsewhere (see especially the account of Thomas Robinson in this *Review*, December, 1972). Most interesting are the author's interpretations of the roles of hawks and doves on both sides. Hinton suggests that Maoist elements (hawks) in the Army may have provoked the first fighting on the Ussuri as a way of re-emphasizing the correctness of a hostile line toward both the USSR and the USA. As already noted, he also argues that Lin Piao's mysterious fall in 1971 was the result of Chou En-lai's desire to eliminate this simplistic Maoist on the grounds that Lin had become a liability to China's security. The second chapter interweaves the diplomacy of Peking, Moscow, and Washington to show how the Chinese, in the face of continuing Soviet military pressure, defused an explosive situation into protracted, secret border talks, with Chinese claims still open. Again a main theme is the intransigence of the militant Maoists; one is almost led to conclude quite literally that Chou can make progress for China in foreign affairs only when Mao and his "true believers" are out of town. A third chapter analyzes Peking's internal responses to the crisis, suggesting that both Maoists and non-Maoists were able to make use of the War-Preparedness Campaign for purposes of their own. A following chapter on the current state (1971) of Sino-Soviet relations predicts continuing hostility but little likelihood of war. The author throughout handles a mass of detail in masterful fashion, but not everything falls neatly into place. For example, Hinton reports Mao as believing late in 1968 that the Soviet Union had become the prime enemy and that relations with the West must be improved, even with the United States (p. 32), but two pages later he writes that in early 1969 Mao "evidently did not take the Soviet threat seriously and therefore saw no reason to appease the United States" (p. 34). Even if the Chairman "was in one of his militant moods" (p. 33), one wonders at the apparent inconsistency. On page 20 is a chronological slip: the suppression of the Red Guards by the Army should be dated late 1968, not 1969.

The final chapter on the American role in the Sino-Soviet dispute sensibly concludes that the United States cannot gain from Machiavel-

lian attempts at manipulation and should strive instead for modest improvements in relations with both Communist Powers without threatening either. Other aspects of this chapter are less appealing. It is marked by a Cold Warrior tone and an insistence that the primary interest of the United States in the Far East is to maintain "stability." But these implications for the larger realm of the proper bases, moral and pragmatic, for American foreign policy need not be debated here, nor do Professor Hinton's views in this respect seriously detract from the very real value of the rest of his analysis.

JAMES L. NICHOLS

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Every War Must End. By Fred Charles Iklé.
(New York: Columbia University Press,
1971. Pp. 160. \$8.00.)

It is notoriously easier for nations to get involved in war than to extricate themselves from it, and the human as well as material losses suffered by belligerents sometimes reach monstrous proportions just because they don't know when and how to stop. The First World War offers a particularly telling and tragic recent example of this. When the holocaust finally ended, the thought uppermost in people's minds was that such a catastrophe must not be allowed to happen again. The issue, however, was not approached in terms of avoiding the undue prolongation of hostilities. Only the abolition of war seemed to offer any real hope. The war of 1914-18 had to remain on the record as the last war in history. This was to be guaranteed by the League of Nations, symbol and instrument of a total moral and political reform, changing the course of history once and for all.

After the collapse of the League and another world war, the victorious powers revived the idea of safeguarding peace through a universal supernational authority; the U.N. was organized as an improved version of the old League. Yet the problem constellation of peace and war had radically changed since the League days. The war that had just ended again was said to mark the beginning of a new historical epoch, but this was ominously dubbed the Atomic Age. The new era dawned amidst apocalyptic fears of sudden extinction, fanned by the nuclear armament race, the cold war, incessant international crises and war scares, and a succession of nonnuclear, "peripheral" wars that might escalate to the nuclear level.

The peace-keeping machinery of the U.N. offered little reassurance in the face of these threats. In time, however, another factor lessen-

ing the danger of war emerged, reciprocal nuclear "deterrence." Fear of the consequences of a nuclear exchange had a visible sobering, restraining effect upon the superpowers' behavior: in one cold war crisis after another, nuclear capabilities were held back on both sides. This of course provided no complete reassurance either: it was realized that mutual restraint might snap in certain circumstances. The important thing was, however, that at least in the normal run of things mere rational self-interest could be relied upon to ward off nuclear annihilation, even while the ideal solution, a new world order barring all violence, was not within reach. Accordingly, many policy planners and theorists turned their attention to the stabilization of deterrence, and the limitation of war, as the most important immediate, practical problem.

Every War Must End reflects this pragmatic approach toward peace and war in a nuclear technological environment. The central theme is the termination of war, that is cutting it short in order to avoid crippling losses. Professor Iklé, however, also deals with the initiation and the conduct of wars from the point of view of minimizing losses. He sees the key aspect of the problem in "rational" decision making, meaning, above all, that no move will be made without a realistic appraisal of its immediate and distant consequences.

The book is divided into five chapters dealing with the most important sources of "irrational" decision-making practices: going to war without having any idea of what the aftermath would be; errors in estimating one's own and the enemy's resources; reckless "escalation" when things do not go well; political constraints and inertia; failure to communicate with the enemy. Under each of these headings, striking examples of "irrationality" are presented, taken from the record of twentieth-century conflicts (the two World Wars, Korea, Suez, and Algeria). An epilogue deals with the strengths and weaknesses of the concept of deterrence.

From the historical material, which is treated with considerable virtuosity, we get vivid illustrations of such typical planning errors as letting the choice of one's objectives be dictated by available resources and capabilities (Germany, Japan), or improvising bizarre diversionary schemes to compensate for lack of resources (France). The study also presents very interesting, unfamiliar material about debates between politically weak and strong policy-making groups, the former being reduced to stressing technical side issues such as timing in opposing moves sponsored by the powerful. Fi-

nally, we may mention one more type of irrationality discussed in Prof. Iklé's study: abrupt shifts from one policy extreme to the other (e.g. from appeasement to all-out war). Here as in all other examples, we have to do with complex political strategic problems facing planning systems that lack the capacity to handle them; the book is intended to contribute to improving the problem-solving capability of decision-making systems working in technologically complex political environments.

Every War Must End does indeed make such a contribution by clarifying some significant aspects of "rationality" and "irrationality" in relation to decisions concerned with peace and war. At this point, however, a question of principle arises: Is "rationality" in the conduct of war a legitimate object of study? Should we try to make war more bearable? Why not seek to abolish war altogether?

It seems to me that these two objectives are not mutually exclusive. Abolition, of course, is the ideal. It is only that we cannot neglect "rationality" unless "abolition" is already an accomplished fact, but this means changing the course of history once and for all, and the trouble with this kind of objective is that one can never know for sure whether it has been accomplished. It is vital to be able to fall back upon "rationality" if necessary.

PAUL KECSKEMETI

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The Economic Impact of Commonwealth Immigration. By K. Jones and A. D. Smith. (New York: Cambridge University Press, 1970. Pp. 177. \$6.00.)

The McCarran-Walter Act shut off British West Indian emigration to the United States and switched it across the Atlantic to England. The Negro migrants from the Caribbean were later joined by an almost equal number from India (mainly Sikhs) and from both wings of Pakistan so that today, twenty years after the McCarran Act, colored Commonwealth immigrants, together with the children born to them in Britain, account for some 4 per cent of the population.

The West Indians were attracted to the British labor market by the policies of full employment pursued by all postwar British governments. The curve of their arrivals throughout the 'fifties corresponded almost exactly to the curve of job vacancies with a three months' time-lag. The pull of the market was at least as strong a factor as the push of poverty. For this reason there has not been a colored manpower problem in Britain as there has been in the

United States; the scarce resource in Britain has not been jobs but housing, since London and the industrial cities which attracted colored labor had been bombed during the war and had not had their housing stock replaced or renewed.

Britain's future has never been seen to depend on immigration (she has in fact been traditionally a country of emigration), and this is one of the reasons why no machinery existed to deal with the postwar Commonwealth immigration which, the Irish apart, was to be the largest in British history. It was also black and carried with it the seeds of social and political tensions: yet it was allowed to occur almost in a fit of absence of mind. Successive governments adopted a policy of laissez-faire, passing the responsibility for housing and education to the local authorities who were unable to cope with problems which were political dynamite.

There was no Ministry of Absorption; there was no central plan, and there were no planning instruments. Official information on the progress of immigrants was unavailable except through the decennial census which, unlike the U.S. census, did not include questions on income and expenditure. In 1964 the Home Secretary flirted with the idea of an economic census of the colored population, found it too difficult, and dropped it.

The work under review is particularly valuable as it is one of the very few attempts to assess the economic costs and benefits of the immigration and coming from the authoritative and independent National Institute of Economic and Social Research it had a particular political importance at a time when unscrupulous attacks were being made on the immigrants and on their cost to the country. The authors show that they were largely a replacement population filling the gap caused in the 25 to 44 age group by low British birth rates in the interwar period. Given the preponderance of this age group among the immigrants it is not surprising that their economic activity rate is higher than that of the indigenous population. What is more surprising is that immigrant workers are not highly concentrated in particular occupations. They appear to have provided an across-the-board increment to work forces in expanding manufacturing industries, as well as serving to mitigate the decline of employment in contracting industries.

The age-distribution of immigrants also has important implications for their use of the social services. Because only 2 per cent of the immigrants, compared with 12 per cent of the indigenous population is more than 65 years old

and because of the very high cost of the geriatric services, immigrant families cost the health and welfare services less than other families, and the authors predict that this is likely to remain true at least until 1981. The age-structure factor more than outweighs the cost of special health and educational requirements.

Immigrants make greater demands (per head) on hospitals and schools, but their housing standards are lower. Many of their needs in this sphere have to be met by what the authors refer to as "delayed scrapping"—in housing, for example, slum clearance has been postponed. The authors find that immigrants' social capital requirements are some 19 per cent below that of the indigenous population, but that this is more or less balanced by a higher than average need for industrial capital, in the short term, because modern plants require more units of capital per unit of output than older plants.

However, the inflationary impact of immigrants' capital requirements is only marginal, and in the long term the position is reversed, since the majority of the immigrants are workers who have just entered upon their working life. Thus "a narrowly economic inspection reveals the acquisition of workers who will contribute, like indigenous workers, to capital formation but in whom much capital, social and industrial, has already been embodied, the cost of which the recipient body has been spared" (p. 160). This is particularly true in Britain where for the last 6 or 7 years colored immigration has been confined almost exclusively to men with professional qualifications.

This is a valuable study. The authors have had to rely mainly on the evidence from the decennial census of 1961 and the 10 per cent sample census exceptionally taken in 1966. It is not their fault that other evidence is lacking, but it is hoped that they have made the case for further research.

E. J. B. ROSE

London

Euratlantica: Changing Perspectives of the European Elites. By Daniel Lerner and Morton Gorden. (Cambridge, Massachusetts: The M.I.T. Press, 1969. Pp. xii, 447. \$3.95, paper.)

This book represents the major research report of a most ambitious study: TEEPS (The European Elite Panel Survey). During a crucial decade for the development of Europe—1955 to 1965—five waves of interviews were carried out with a sample of elites in Germany, France and Britain. The elites were drawn from government, politics, business, and communi-

cations. In addition, other relevant groups were studied—officials of EEC, students at the *grandes écoles* in France (to get at future elites) and scientists at CERN. The selection of the elites is never completely described—except that they were chosen by a panel of experts in each country—but it appears clear that they represent a cross-section of the "establishments" in each nation.

The interviews covered many topics, but the main focus was on Europe, its future, and its options. The study is thus a social science enterprise with clear relevance to contemporary problems. (In this connection, it is unfortunate that a book issued in 1969 first reached this reviewer in the fall of 1972.)

The authors provide a lively account of the methodological problems involved in cross-cultural research, particularly with an elite population. And they demonstrate that a lot can be accomplished even when the experts tell one that survey techniques will not work with sophisticated elites. For instance, the authors found that their respondents were willing to answer closed-ended questions and did not resent the simplification that such questions imply.

The repeated surveys allow the authors to trace the evolving attitudes of the three elite groups to alternate patterns of European development: Atlantica (the Atlantic alliance of Europe and America embodied in NATO and the Marshall Plan); Europa (the Monnet or EEC model of European integration); and Euronalism (the Gaullist design).

The surveys reveal a high level of consensus among the various elites groups—consensus in commitment to Europa and Atlantica coupled with rejection of the Gaullist alternative. And this policy consensus is based, so the authors tell us, on a growing pragmatism and a decline of commitment to "ideologizing" and "moralizing." "The underlying sentiment is that European integration/Euratlantica cooperation based on the Euramerican system of relations are historically 'right' [i.e., correct] in the sense that they represent the most rational options open to Europeans in the historical conditions of the post-war world" (p. 242). That these elites are pragmatic is clear in the data, but the authors (who share the policy preferences of their respondents) too often seem to equate "ideological" thinking (rigid, unrealistic, bad) with opposition to the "historically right" movement toward Euratlantica. It is ideological to oppose the European community and the Atlantic alliance, pragmatic to support it. Maybe so. But one man's pragmatism is often another's dogma.

The high level of consensus among the elites leads to a certain blandness in the data. The authors find few meaningful variations within the national samples. With almost no exceptions, the data consist of presentations of marginal results across time for each nation. Thus what is usually most interesting in cross-national research—comparisons across nations of patterns of internal variation within nations—is missing. Little, also, is done with the panel data—i.e., the fact that the same respondents are interviewed a number of times rather than using new replicated samples. We see movement in the marginals from time period to time period, but do not know how the reinterviewed respondents may have changed. Measures of the stability of preferences from one time point to another might have been most interesting.

One senses—particularly from the appendices, in which various attempts to find clusters of types of elites are reported with few substantive payoffs—that the authors searched hard for meaningful within-nation variations but the overriding consensus among the elites doomed most such attempts. That's the way things are with data!

The authors conclude that Europa and Euratlantica will very probably grow and prosper. In the few years since their last wave of interviews much has happened. The dollar crisis and (probably to a lesser extent) Vietnam have strained the Atlantic alliance. De Gaulle has left the scene, but the Gaullist view on Europe has not completely disappeared from the Elysées palace. Britain is in, but not all Britons are happy. And in Norway a movement—inconsistent with the new “pragmatic” Europe—arose to defeat EEC at least for the time being.

Despite all this, the authors' predictions are probably sound. Their interviews allowed them to penetrate beneath the surface events that push Europa and Atlantica temporarily forward and temporarily backward. Their data show a movement forward in the long run. And their judgment—based on a unique and valuable body of data—is probably correct.

SIDNEY VERBA

Harvard University

Victors' Justice: The Tokyo War Crimes Trial.

By Richard H. Minear. (Princeton, N.J.: Princeton University Press, 1971. Pp. 229. \$7.95.)

In his preface, Richard Minear, a legal historian, makes some fashionable charges against contemporary political scientists and area specialists. He has “grave suspicions about the concerns of political science, or at least politi-

cal scientists writing about Vietnam” (p. xii). He holds that it is dangerous to leave recent history to the “tender mercies” of Samuel Huntington, Douglas Pike, Ithiel de Sola Pool, and Frank Trager. He asserts that area specialists should concern themselves at least part of the time with topics of immediate relevance to American policy.

The “wrongness” of the Vietnam War led Minear to doubt that American policy on Japan during World War II and the Occupation could have been enlightened. Thus the author begins by generalizing from the Vietnam War and applies the generalization concerning American policy in the 1960s to arrive at a revisionist interpretation of American policy in the 1930s and 1940s. He tends to assume that the reader already shares his ideological biases. Forewarned of the author's lack of objectivity, the reviewer might be forgiven if he is at first predisposed to dismiss Minear's book as just another antiwar tract.

The usual pacifist contention is that the Nuremberg and Tokyo war crimes trials provide moral and legal justifications to resist American involvement in Vietnam and to indict American policy-makers as war criminals. Minear declares, however, that his major concern is “to demolish the credibility of the Tokyo trials and its verdict” (p. ix). He holds that Tojo was “legally innocent” (p. x). He favors trials for conventional war crimes, but opposes trials for aggressive war itself. Thus he would oppose a trial concerned with the whole course of American policy in Asia (p. xi), which he regards as aggressive.

It is a tribute to Professor Minear's intellectual independence that, notwithstanding his views on Vietnam, he refuses to glorify the Nuremberg and Tokyo Trials and instead attacks them. Like many American military men and conservatives, he is appalled by the idea that a nation's military and political leaders might be held personally responsible and punishable for a war. Minear thus appears to attack the basis of the Vietnam War trials held in Stockholm and in Tokyo in 1967 unless he regards them as primarily concerned with conventional war crimes.

The Princeton University Press has announced that it advanced the publication date of Minear's book in view of the interest created by David Bergamini's massive *Japan's Imperial Conspiracy*, which appeared late in 1971. Bergamini, whose scholarly qualifications have been severely questioned by American experts on Japan, attacks the fairness of the Tokyo War Crimes Trial on the alleged ground that

Emperor Hirohito was a principal architect of Japan's aggressive policies and should have been tried along with Tojo and the rest. Although both books advocate a revisionist interpretation of the trials, Minear goes much farther than Bergamini, urging that the trial of a defeated nation's leaders by the victors lacked both legal sanction and fairness. Thus Minear would oppose the trial of the Emperor as well as the trial of other Japanese leaders for their policy of aggression. Minear even goes so far as to suggest that Japan's war policy was at least partly motivated by a legitimate concern for the national defense. Although the two books have become closely associated in the minds of many readers, they have very little in common, whether in content, method, or philosophy.

Minear criticizes the International Military Tribunal for the Far East and its verdict on nearly every conceivable score: the vagueness of the conspiracy charge, the alleged responsibility of individuals before international law, the dubiousness of the concept of aggression, the retroactivity of the verdict (in which people were convicted for deeds not crimes at the time committed), the concept of negative criminality (the failure of superiors to prevent crimes by subordinates), the selection of judges exclusively from the victor nations, dubious procedures (including the imposition of death sentences by a six to five vote of the justices), the failure to try any Allied leaders (even for such apparent violations of international law as the dropping of atomic bombs on cities and the Soviet Union's declaration of war on Japan while a neutrality treaty between the two countries was in force), inconsistencies in the selection of the Japanese individuals to be tried, the political decision not to try the Emperor, and the very loose and biased application of rules of evidence.

Minear's basic contention is that many of the ideals and preconceptions that lay behind the Tokyo trial "have played a contributing role in the more recent mistakes the United States has made and continues to make in Asia." These preconceptions include "the belief that change in the international arena should come only through peaceful means, that resort to force is unjustifiable whatever the provocation, that aggressive war (assuming we can decide what aggression means) is illegal" (p. 178). Dean Rusk's talk of "aggression" and President Nixon's talk of "a generation of peace," Minear says, echo the Pact of Paris and the Nuremberg and Tokyo verdicts.

Professor Minear, in attacking the legalistic,

moralistic approach to the analysis of international relations is saying nothing new. The realist school led by Hans Morgenthau has been reiterating these ideas for twenty-five years. What is surprising is that Minear, an opponent of America's Vietnam policy, appears to deny the basis of the legalistic-moralistic attack on America's Vietnam policy. He evidently opposes the policy not so much because it is illegal or immoral (concepts inappropriate for foreign policy formulation), but because it is politically and strategically unsound.

The Tokyo trial occurred after the Nuremberg trial, and for the American public it was an anticlimax. Americans were much more familiar with the Nazi leaders and their philosophy than with the Japanese leaders and policies which were on trial in Tokyo. Americans forgot the Tokyo trial after 1952, when Japan became an ally. Twenty years later, troubled by the course of American policy in Asia, Minear has reopened the record of the Tokyo trial and raised many searching and difficult questions. Most of these questions were pretty thoroughly debated in the court and press at the time of the trial, although wartime emotions were still at a fairly high pitch. The answers the author advances sometimes seem onesided. This book is perhaps too short and polemical to be regarded as definitive. However, Minear's firm grasp of both law and history has enabled him to expound, in a masterful fashion, the lessons of the Tokyo trial. We must be grateful to Professor Minear for producing a readable and perceptive book about a much neglected but historically very important topic. His fundamental criticisms of the International Military Tribunal for the Far East are equally relevant to the Nuremberg trial and any trials for aggression that might be held in the future. To the student of international politics, the Minear volume provides corroborative data and analysis in support of the realist approach.

THEODORE McNELLY

University of Maryland

Conflict Analysis. By Michael Nicholson. (New York: Barnes & Noble, 1970. Pp. 168. \$8.00.)

Michael Nicholson's book *Conflict Analysis*, manifests the influence of several peace-oriented groups in England, including the Conflict Studies group at The University of Lancaster; the Center for the Analysis of Conflict at University College, London; and the Richardson Institute for Conflict and Peace Research, also in London. In spite of their peace orientation,

these three groups have retained an analytic approach to conflict research in contrast to the more philosophical orientation of many other peace activists.

Among those activist scholars committed to an analytic approach to peace research, Michael Nicholson stands out as one of the most formal (i.e., deductive), yet one of those who is sympathetic to empirical work. Judged from the perspective of a balance between formal and empirical approaches, *Conflict Analysis* achieves a nice equilibrium. The book, however, does a rather uneven job of integrating findings from empirical studies with deductions from formal analysis.

Consider the chapter contents as a means for evaluating the balance between formal and empirical approaches. There are ten chapters in the book. Three deal with epistemological, definitional, or value problems; three chapters focus on formal approaches—game theory, bargaining, and arms races; three chapters contain empirical descriptions or summaries of the empirical literature regarding war, alliances, and crises, and one chapter deals with experimental methods, in particular, man-machine laboratory simulation. In short, three chapters deal with broad methodological issues, three are relatively formal expositions, three are more or less empirical, and one chapter is experimental.

While *Conflict Analysis* is relatively balanced between formal and empirical approaches, it is somewhat less successful in its attempt at theoretical coherence. That is, the formal approaches are used only rarely to illustrate how formal approaches can generate deductions about which empirical evidence would be relevant. In the relatively formal chapters dealing with game theory, bargaining, and arms races, Nicholson could have demonstrated linkages between formal and empirical studies. Although he acknowledges the gap between game theory and evidence from experimental games, Nicholson does not document the gap or suggest ways of relating insights from game theory either to experimental games or empirical work generally. In his discussion of Richardson's theory of arms races, Nicholson does a good job in evaluating Richardson's formal theory against his own evidence but a less adequate job of evaluating Richardson from the perspective of evidence from other scholars.

The less formal chapters dealing with war, alliance, and crisis contain excellent descriptions of current studies, but no attempt is made to link formal theory with empirical evidence. For example, Nicholson discusses the role of

perception in mediated stimulus-response models of crisis decision making, but the potential link between formal theory and the empirical studies of crises is not pursued. For example, an assumption underlying game theory is that the actors have adequate information, i.e., misperception would not allow actors to be rational. If crisis studies find that misperceptions characterize escalation processes leading to war, such a finding would be evidence opposed to a game-theory assumption of adequate information, where the converse would be true if crisis research shows that perceptions are relatively accurate.

What, in fact, does the evidence suggest regarding the phenomenon of misperception? Some authors, such as Anatol Rapoport, assume that misperception is a principal factor explaining escalation. On the other hand, others such as Dina Zinnes suggest that misperception generally does not account for escalation. For example, Zinnes finds a positive relationship between a nation's expression of hostility and the target state's perception of threat or unfriendliness. In other words, nations correctly perceive threat or unfriendliness and express hostility in return.

In discussing game theory (rational) models of choice, Nicholson also overlooks another link between formal approaches and empirical evidence, namely, the role of organization in placing limits on the value-maximizing behavior assumed. Consider Graham Allison's findings regarding limited rather than comprehensive rationality in his study of crisis decision making in the Cuban Missile Crisis of 1962. Allison stresses, inter alia, organizational prothe crisis more as outputs of large organizations cesses to explain Soviet and U.S. behavior in functioning according to standard operating procedures and search processes rather than as the value-maximizing unitary actors suggested in the game theory assumption of comprehensive rationality. Allison's evidence concerning the limited applicability of comprehensive rationality has implications for the validity of choice models of the game theory type as discussed by Nicholson. In short, while Nicholson considers both formal and empirical approaches, he overlooks their potential interrelationships, thus weakening the book's intellectual coherence.

Nicholson's concern for peace is evident in his discussion of social science and the study of conflict as well as in the chapter on social science and values. A concern for peace, however, is not enough. That is, Nicholson should show

specifically how peace research products might help bring about a more pacific world. Nicholson's experience with institutions which are devoted to the study of conflict and conflict resolution gives him a good perspective on the need for clarifying the relationship between social science research and policy making. Indeed, Nicholson states in the preface of *Conflict Analysis* that his purpose in writing the book is "... to describe ... the current work in the application of methods and tools of the social sciences to further the understanding and to promote an ultimate cure of violent and in particular, international conflicts" (p. 14). Yet he devotes only a few pages to the discussion of the actual relevance of conflict and peace research to questions of policy making and policy outcomes.

Nicholson notes that political strategists such as Machiavelli and Clausewitz have discussed the uses of war as a tool for policy. If the purpose of *Conflict Analysis* is to further the understanding and to promote an ultimate cure of violent international conflicts, more attention could be given to applying the findings of peace research to historical situations as scholars such as John Burton have done.

In spite of my reservations concerning the lack of theoretical coherence between formal and empirical approaches, I believe nevertheless that *Conflict Analysis* should be a useful book. It could serve as an introductory text for upper level undergraduates and beginning graduate students interested in conflict studies. For the general academic reader, the book offers a good descriptive and evaluative view of some existing methods and models of conflict.

RAYMOND TANTER

University of Michigan

A Hundred Years of International Relations.

By F. S. Northedge and M. J. Grieve. (New York: Praeger, 1972. Pp. 397. \$11.50.)

"This book," the authors write,

stems from a sense of the limitations of two types of writing about international relations: orthodox diplomatic history with its heavy emphasis on primary archival material, and general theory of international relations, which seems to us to be principally contemporary international affairs with a semblance of social-science analysis and a heavy ballast of incomprehensible jargon. The former tends to degenerate into a strict day-by-day diary of events divorced from the wider framework of the international political system which helps to shape the external behaviour of states. The latter appears to be too abstract, too

uninformed about the historical dimensions of the subject, to throw much light on the living realities of state behavior (p. ix).

Some will say that Professor Northedge and Miss Grieve have not so much steered a careful course between the Scylla of diplomatic history and the Charybdis of international relations theory as fallen between two stools. For their book is too discursive and too dependent on secondary sources to rate as significant contribution to the former subject, and too loose and unsystematic in its argument to be regarded as an important advance in the latter. They have, however, provided the student and the general reader with an admirable historical introduction to the study of international relations.

There are five chapters on the pre-World War I period, five on the interwar period, and six (counting the Conclusion) on the years since World War II. All of the chapters combine wide reading with a willingness to identify the big questions and make judgments about them. The chapters on imperialism, on the League of Nations, the United Nations and "The Transcendence of Sovereignty" seem to me to be particularly valuable. There are some fine passages of writing. Unity is provided by the authors' concentration on the international political system and the changes it has undergone in the last century.

In handling this theme, which Professor Northedge has masterfully expounded in his lectures at the London School of Economics over a couple of decades, the book gives rise to expectations of a contribution to the theory of international relations that it does not quite fulfil. A concluding chapter identifies the main changes in the international system as the transition from a European to a world international system, the "socialization" of foreign policy and the growth of international organizations. Throughout the book the authors attempt to relate such developments as the growth of population, the advance of technology, the two world wars, the rise and demise of the cold war, and the emergence of new states to the shape and structure of the international system. But nowhere are we given a rigorous and systematic exposition of what the international system is, or how and why it functions as it does. A view of the international system is implicit in many of the chapters, and comes closest to the surface in the chapter on sovereignty. To advance our understanding of this subject we need to approach the historical material with rigorous definitions and carefully formulated hypotheses, i.e., to take the theory of interna-

tional relations more seriously than Professor Northedge and Miss Grieve are prepared to do.

HEDLEY BULL

Australian National University

U.S. Foreign Economic Policy for the 1970s: A New Approach to New Realities. A Policy Report by an NPA Advisory Committee. With supporting papers. (Washington, D.C.: NPA, 1971. \$2.50. 215 pp.)

Committees of the National Planning Association are normally composed of prominent, forward-looking representatives of agriculture, business, labor, and the professions. For many years the NPA could take justifiable pride in the ability of such committees to reach substantial agreement on what ought to be done about various problems of public policy. Skillful drafting on the part of the NPA staff was, of course, a *sine qua non*. So were "footnotes" that enabled signatories to ride their favorite hobbies, calibrate upward or downward their enthusiasm for particular paragraphs of the report, and in other ways get in their personal licks. Because the reports were timely and their general thrust was clear, they were influential documents, whether published with or without supporting essays.

Today the NPA can take pride primarily in the ingenuity of its staff in papering over substantial disagreement among committee members, producing a forward-looking report on foreign economic policy despite the inroads protectionism has made among supporters of liberal trade policies, and fortifying the report's uncertain trumpet with a half-dozen clear and well-written essays on specific issues of importance to the United States and other noncommunist industrial countries.

The book under review is a 215-page paperback published in November, 1971—i.e., in the shadow of the shock of the economic policies unilaterally introduced by the United States on the preceding August 15, and before removal of the 10 per cent import surcharge and negotiation of the so-called Smithsonian agreement on monetary policy late in 1971. It has not in any fundamental sense been overtaken by events and, in these days of soaring book prices, remains an excellent bargain at \$2.50. The book begins with a 40-page policy report by an NPA advisory committee recommending domestic and international policies for the "new realities of the 1970s" (p. 4). For these new realities, the "new guiding principle" (which might not seem so new had it not so recently been flagrantly violated) is that "inso-

far as practicable, changes in trade, monetary, investment, and other national economic policies that significantly shift their costs to other countries or that are likely to generate trade and capital movements seriously disruptive to other countries should not be put into effect unilaterally" (p. 27-28).

To this reviewer, the ensuing catalog of specific recommendations for phasing out tariffs and quotas, reducing agricultural protectionism, attacking nontariff barriers, introducing monetary reform, etc. seems sensible. To the labor members of the committee, however, the recommendations appear "completely inadequate." In 13 pages of small print, 10 of the 21 signers add 40 footnotes (20 by Nat Weinberg of the United Automobile Workers) cogently expressing various degrees of dissent from the consensus.

NPA's Chief of International Studies, Theodore Geiger, who did most of the staff work for the report, adds "A Note on U.S. Comparative Advantages, Productivity and Price Competitiveness" which intelligently sifts the available evidence concerning the supposed narrowing of economic disparities among major trading nations and the resultant adverse effects on the balance of payments and domestic employment of the United States. He also contributes an essay on whether the world is moving toward a system of trading blocs and, if so, what would arrest the trend (freedom of trade and capital movements among the European Community, Japan, and the United States). His wife and NPA colleague, Frances Geiger, closes the book with an analysis of the U.S. adjustment assistance program and analogous programs of other OECD countries. (Only Canada operates a direct counterpart.)

Between the essays of Dr. and Mrs. Geiger are four other "supporting essays." Harald B. Malmgren considers nontariff barriers (NTBs) and recommends negotiations aimed at a two-tiered system: (a) a set of general principles to apply in all cases (e.g., no damage to other countries without a compensatory adjustment), and (b) specific agreements to cover designated NTB problems. C. Fred Bergsten makes international monetary reform comprehensible to the layman. Robert M. Dunn, Jr., in the most academic of the essays, wrestles with the problem that imbalances in payments flows are likely to increase in volume and frequency as barriers to trade and capital flows are reduced. Robert E. Hunter speculates in an informed and undogmatic way on U.S. defense commitments in Europe and Asia in the 1970s and

their probable effects (modest) on the U.S. balance of payments.

All six essayists are as sensitive as sinus-sufferers in winter winds to the politics of economic change. Together they have produced a lucid, informative book.

ROBERT E. ASHER

Washington, D.C.

Partners in Development: Report of the Commission on International Development. By Lester B. Pearson, Chairman. (New York: Praeger, 1969. Pp. 400. \$7.95.)

Ever since the Pearson Commission was requested by the World Bank to review the recent history of international development, evaluate its results, and propose new and better policies for the future, its report has been eagerly awaited by all those concerned with development activities in the less developed countries. The composition of the Commission, the authority vested in it, and the wealth of material at its disposal, gave rise to the hope that its report would conclude an era of unfulfilled expectations and open new roads to the future. The report was expected to have the contents and the ring of the cry, "The King is Dead, Long Live the King!"

It was therefore disappointing to discover that the report covered only part of what it was hoped that the Commission would do. In brief, while the report provides a clear and illuminating analysis of past trends and present problems, it barely touches upon the main issue—what are the *practical steps* to be taken in order to achieve better results in the future.

It is now quite obvious that in the last two decades the developing countries have made only slight progress, their pace of development being much slower than had been expected. This conclusion is stated unequivocally by the Commission on the basis of the extensive evidence it collected. "Of the population in the less developed world (excluding Mainland China) 22 per cent live in countries where per capita income has grown at less than 1 per cent per year, 48 per cent live in countries where it grew between 1 and 2 per cent, and 30 per cent in countries where per capita income grew by more than 2 per cent per year" (p. 29). These figures indicate that most of the developing countries have not yet experienced any meaningful change in their present poverty level. Moreover, their rate of growth is so slow that even the beginning of the road to improvement is not yet in sight.

In view of this evidence, it is difficult to accept the Commission's conclusion that "Eco-

nomical growth in many of the developing countries has proceeded at faster rates than the industrialized countries ever enjoyed at a similar stage in their own history. The fears that economically underdeveloped parts of the world were incapable of growth, or that their political problems would be so great as to preclude any economic advance, have proved to be unfounded" (p. 3).

Even so, the difficulties which the Commission has forecast for the developing countries appear to be enormous; they are aptly summarized on pages 58–61 and include three basic problems: unemployment and underutilization of human resources, rural stagnation, and the unchecked urbanization rush. Unless a practical strategy is proposed to overcome these problems, no change will occur in the coming decade, and the consequences may well be disastrous.

The major part of the Report (Chapters 4–11) is therefore devoted to a detailed proposal of "a strategy" for future action which is addressed "to the developing countries, to the industrialized countries and to the international organizations" (p. 14). The proposal includes ten recommendations, nine of which are concerned exclusively with external factors; namely, the nature, the purpose, and the organization of international trade and aid. Only one recommendation—referring to population, education, and research—deals directly (albeit very generally) with the internal situation of the developing countries.

It seems obvious that any strategy of development must be concerned primarily with what is happening to each country's society, methods of production and organization, and so on. Extraneous factors can only be of secondary importance. By devoting most of its attention to such factors as international trade and aid organization, the Commission missed the main issue—what kind of development programs should be undertaken *within* the developing countries in order to promote their economic growth, and how these programs should be implemented.

One example will illustrate this point. The Commission suggests that "the primary purpose of the additional development aid . . . for the 1970's should be to help bring as many less developed countries as possible to a level of growth of at least 6 per cent per year" (p. 124). The Commission concedes that "a high level of self-sustaining growth cannot be achieved simply by foreign aid, it requires the governments of developing countries to take very positive measures of their own to remove

the obstacles in the way of growth" (p. 127). What kinds of measures? This question unfortunately remains unanswered.

Furthermore, without such a basic program for change it is impossible to evaluate any proposal for improving a trade policy or foreign investment or even the means for more effective aid. For instance, when dealing with more effective aid, in particular regarding projects and programs, how can one recommend an improvement in aid organization when one is in doubt about the nature of the desired projects and the contents of the programs needed in the developing countries?

In conclusion, the Report has described very ably the past trends, current conditions, and probable future difficulties and has systematically and eloquently described the enormity of the task which has to be done. But that is all. It does not propose any new approach, or *any new program which would lead to practical solutions* to the basic problems of underdeveloped countries. The old beliefs are indeed dead, but where are the new ones?

RAANAN WEITZ

Rehovot, Settlement Study Center

US/UN: Foreign Policy and International Organization. By Robert E. Riggs. (New York: Appleton-Century-Crofts, 1971. Pp. 347. \$4.95.)

To bend a familiar phrase slightly, "national interest" is in the eye of the beholder. No work demonstrates this more clearly than the present book, which aims at evaluating the performance of the UN from the perspective of United States national interest. This is a useful and important approach to the subject, but it demonstrates once more what a difficult analytical tool such a subjective concept as the "national interest" is.

All books, especially one this brief, must be selective, and this one chooses to concentrate on two major operational areas: so-called "power" (peace maintenance) and "welfare" (economic and social problems). Within each of these categories, the author examines UN experience in dealing with certain major issues as the basis of his analysis.

As always, the crucial aspect of the work is not the factual grist, although that is difficult enough, but the methodological mill through which the data are processed. At least five methodological questions can be raised about this work:

First, whose interests should guide the evaluation? While the author emphasizes United States national interest in most of the book, he

introduces the interest of the "international community" toward the end when he discusses the general institutional development of the United Nations. On the basis of the general orientation of the book, one would expect the national point of view throughout. If, however, one introduces the international community perspective as a secondary theme, why not use it as a parallel comparative yardstick throughout?

Second, if the United States national interest is to be the dominant frame of reference, how does one determine its substance? In the author's discussion of his methodology, he mentions two obvious approaches: (1) to use official statements of national interest or (2) to substitute the analyst's own formulation of national interest. Either of these, as well as other, approaches can be justified, but they should not be confused.

On page 66, during the discussion of "cold war" disputes, Riggs says, "The effectiveness of American action in the United Nations will thus be judged by how well the policy makers succeed in doing what they were trying to do, not by what, from hindsight, the United States should have done." But later the author raises questions about the validity of United States policy and admits frankly, on page 204, "This line of argument does not attack the utility of the United Nations as an instrument of policy. Rather it questions the wisdom of the policy. . . ." Unhappily the author frequently mixes these two perspectives without adequately differentiating between them.

Third, what functional criteria should be used to evaluate UN performance in relation to either national or community interest? The author identifies three major functions in both the security and welfare fields: negotiation, legitimization, and material support. Granted, these are relevant and important, but they seem too limited and vague. Surely they do not exhaust the list of significant functions performed by the UN in the security and welfare areas. What about fact-gathering inquiries, systematic analysis of problems by staff or other groups, definitive decision making, the termination of hostilities, the peaceful settlement of conflicts, the formulation of advisory or binding standards, and general administration not necessarily involving material support?

Fourth, how does one measure the effectiveness of UN performance with regard to these functions? Admittedly, this is a most difficult question for any scholar to answer, especially in these amorphous areas of human relations where no one can measure activity with abso-

lute precision. Nonetheless, the author does not adequately address this methodological problem, and his analysis seems more subjective, inchoate and unsystematic than some other works in this field.

Fifth, how does this study compare with other relevant investigations of either the UN or other international organizations? Regrettably the author has very little to say on this question.

Within this analytical framework, the author concentrates primarily on evaluating the utility of the UN with regard to two main categories of disputes: those involving Communist states and those not involving Communist states. While this is only one of many possible bases of classification and while such a boundary cannot be drawn so sharply, the distinction has utility and is frequently used.

In the Communist category, the author describes the United States emphasis on "legitimization" of its positions against the Soviet Union, in cases such as the Greek and Korean situations, during the height of the Cold War. During the post-Stalinist thaw, the US took advantage of the improved climate for negotiation in situations such as the Cuban missile crisis. At the same time, the analysis reminds us of the many factors which have limited UN achievement in this area: e.g., continuing and profound hostility between the two superpowers, preference for non-UN negotiations to avoid the multilateral decision-making process involving a growing number of lesser states, and the lack of significant UN resources to affect such issues.

While Riggs indicates that the UN has tended to be more effective in moderating conflicts in the non-Communist category, he concludes that, here also, the US has preferred bilateral or regional negotiation which is likely to be more amenable to US influence. The analysis ends with the statement, "... national interest dictates that the United States should work directly with the disputants whenever possible rather than risking dilution of objectives through UN action or ... the development of a majority consensus that does not include the U.S." (p. 208). This is one of several places in which it is not clear whether the author is simply repeating official doctrine or expressing his own. In any case, though there is much to support this conclusion, it fails to reflect the positive contributions of the UN to which the author has made previous but muted allusions.

The welfare functions of the UN will surely

have more significant long-range impact than its peacekeeping role will have in improving the quality of life in the world. And the US has given substantial support to these functions. Unfortunately, however, the author devotes only a quarter of the space he has given the security function to the welfare role. He limits his discussion entirely to human rights, trade, refugees, relief, and development, in that order, and gives virtually no attention to the specialized agencies. The analysis describes the gradual decline of US influence and interest in the UN as the membership has grown and the balance of votes has shifted in favor of the developing countries. While noting the increasing hostility toward the UN voiced in the US, the author, in this case, injects his own view that "... if the US should conceive its interests broadly ... , a new era of constructive American leadership in the UN could begin, and the UN could become an important instrument of economic and social policy for the US" (p. 256). Unfortunately, his restricted analysis of welfare activities provides little empirical foundation for this conclusion.

H. FIELD HAVILAND

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The Law in Political Integration: The Evolution and Integrative Implications of Regional Legal Processes in the European Community. Harvard University Center for International Affairs. Occasional Papers in International Affairs. No. 27. By Stuart A. Scheingold. (Cambridge, Mass.: The Center, June 1971. Pp. 59. \$2.25, paper.)

Transatlantic Relations in the Prospect of an Enlarged European Community. British-North America Committee, BN-2. By Theodore Geiger. (Washington, D.C.: The National Planning Association, November 1970. Pp. 61. \$1.50, paper.)

Changes in international society during the past twenty-five years have been so momentous and apparently unique that they have strained the imagination and vocabulary of political commentators. This is the case in the relations among the advanced industrialized societies as a whole as well as in the regional grouping that centers around the European Community. Both essays are imaginative efforts to come to grips with some of the problems associated with the description and explanation of these transformations. That neither has accomplished a significant theoretical breakthrough should not detract from their considerable contributions.

Unlike other works on the development of European Community Law, Mr. Scheingold's essay quite consciously sets aside what he calls a "federal teleology." That fallacy of analysis, which assumes a natural goal for the integrative process, impedes the development of descriptive concepts appropriate to the current status of interregional norms in Europe. Mr. Scheingold succinctly argues why the federalist model is inappropriate to the work of the Court of Justice. In fact, he demonstrates that the Court, itself, has avoided policy issues, the direct handling of which would assert federal prerogatives in deed as well as word. Rather than view the legal processes of the Community as fostering federal ends, he argues that they may best be understood in the framework of the consensual system in which they operate.

Prima facie evidence for Mr. Scheingold's hypothesis on the consensual bias of the Court of Justice comes from the Court's persistent refusal to utilize Article 177 in the face of its multiple opportunities to do so. Article 177 could be the most powerful federalizing instrument at the Court's disposal since it gives it competence to deal with the interpretation not only of the Treaty, but also of acts of the Community's institutions and of certain statutes set up by other community bodies; and it establishes the Court as an appellate tribunal of last resort. More fundamental support for his thesis covering consensual processes stems from Mr. Scheingold's interviews and his analysis of litigation before national courts. From these, he concludes that there has been a sort of subterranean promotion of communitywide norms. This has been associated notably with attorneys who have fostered the socialization of these norms in business and interest group leaders. It has also resulted from the Court of Justice itself, insofar as its own validation of executive action reinforces the legitimacy of consensual procedures in the Commission and Council of Ministers.

Mr. Scheingold clearly believes that a consensual process has been developing in the legal system of the European Community, that it is the system's most pronounced characteristic, and that it will continue to be institutionalized as one "which softens rather than sharpens the hard edges of community rules and thus makes them more palatable to all participants" (p. 46). But he recognizes that his own findings are quite modest and do not provide a suitable basis for predicting the eventual level of legal integration in Europe. That, he recognizes, is a function of the political parameter inside of

which the legal processes operate. If this is so, it is doubtful that any of Mr. Scheingold's hypotheses about the nature of the eventual legal patterns in the European Community can now be fully verified. What can be analyzed, however, are norms that develop along with increased levels of interdependence among societies. These change rapidly as a result of the sorts of transformations modernized societies continuously undergo. Mr. Scheingold has made an important effort to come to grips with these norms. If they are to be described and explained satisfactorily, attempts like his own to deal with extrajudicial participants in the legal processes are likely to prove to be the appropriate starting point.

Describing the whole range of changes in norms and processes among the set of industrialized societies is far more difficult than the rather narrow effort to treat legal processes in the European Community setting. Yet, Mr. Geiger does this with such care and eloquence that his essay is a virtual tour de force. Although written before the outbreak of neomercantilist trade and monetary policies in 1971 and the signing of treaties of accession or association to the European Communities by the former EFTA members, it has lost none of its force. In fact, predictions of the former and an analysis of the consequences of the latter are included in the essay.

Mr. Geiger moves with ease between his description of the political history of the European Community and of the transatlantic network and his explanation of more fundamental political and economic trends associated with the high levels of modernization of the societies composing this region. Like Mr. Scheingold, Mr. Geiger refuses seriously to consider any "natural end states" toward which these societies are being driven, whether in terms of a federal or functional Europe, or an institutionalized Atlantic Community. Instead he asks what kinds of institutions are appropriate for handling the levels of interdependence among those relatively modernized societies; and he tries to sketch out the emerging patterns of norms that describe the relations of these societies as their increased interdependence engenders increased tension among them.

The essay concludes with a series of scenarios listed according to Mr. Geiger's views on their probable occurrence. It is a considerable feat that some of his predictions have begun to be validated, especially in light of the rather low level of theory that is now at hand to describe or explain the phenomena with which

the author has dealt. Although part of a larger book-length effort that Mr. Geiger has recently published, this essay provides appropriate and useful materials for both the classroom and for policy formation.

As both monographs point out, there is a great deal of theoretical work to be done on the relations between the advanced industrialized societies if the tensions and crises engendered by their interdependence are to be handled adequately by the appropriate public authorities. The requisite of such theorizing is the avoidance of teleological pitfalls and the rhetoric of outmoded political ideals. Another, of course, is innovative ideas. That both pieces are better in the former regard should not be undervalued in the development of adequate theorizing.

EDWARD L. MORSE

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Confrontation and Intervention in the Modern World. By Urs Schwarz. (Dobbs Ferry, N.Y.: Oceana Publications, Inc., 1970. Pp. 218. \$7.50.)

In seeking its sources of verification, political science is more and more torn between persuasive interpretation that commands the consensus of the experts and rigorous conceptualization and measurement that stands the test of replication. Both aim at prediction, the first more successfully thus far since the second is only an arrogant youngster, a struggling science, whereas the first is a fine art. Yet the older can never really be the same with the youngster around. If the art tries to ignore the science, it is deprecated in the professional literature, and if it tries to make its peace with the science and meet it halfway, it is likely to satisfy no one, not even those who find their own peace and salvation in the middle of the road. In this case, both the reviewer and the reviewed are in the latter position, with all the advantages and disadvantages thereof.

Urs Schwarz, retired editor of the *Neue Zürcher Zeitung* and the *Swiss Review of World Affairs* and author of *American Strategy: A New Perspective*, has written an interpretation of two typical mechanisms of contemporary international relations. The book is interesting but scarcely definitive, one that would make a good college international relations text and could trigger some further investigation by a more rigorous thinker. Both confrontation and intervention are defined as episodic encounters in postwar international relations, the first being a symmetrical limited use of threat of force (pp. 15-16) and the second being an

asymmetrical one (p. 83 *et passim*). The two concepts are rather tightly defined, and are then used to identify a series of events in the contemporary world. One might then have expected an attempt to find typical patterns of action within each concept, either in an effort to identify some inherent process or in the hopes of finding ways in which the reaction can be kept from getting out of hand and peace (or some other value) can be preserved. Unfortunately, for an exercise that began so well, such an end is never attained, and the study concludes with some semantic juggles (p. 206) and some interesting comments on the erstwhile Cold War.

After an initial discussion of the constructive use of limited force and of the concept of confrontation, Schwarz launches into an excellent series of thumbnail sketches of a number of confrontations since the original Berlin blockade. He is particularly deft in portraying the chain of encounters of the Khrushchev era, from the Berlin ultimatum of 1958 to the Cuban missile crisis of 1962, with a fine capacity for summarizing context, options, moves, and outcomes. The chapter moves on to other confrontations and then just ends (p. 82), all trees and no forest. The second half of the book, on intervention, does not show the same skill in its case studies, but the wisdom of a sound and seasoned commentator on international affairs is still evident. The strength in the second part lies therefore not so much in the vignettes of intervention or in the more semantic discussions of the concept itself, but in Schwarz's perception of the evolution of international relations from a hierarchical system of power and responsibility where the stronger policed the weaker and were expected to do so, to a limited conflict system in which nonintervention is expected but intervention occurs, with neither power nor responsibility clearly defined.

Nowhere is this ambiguity more evident than in Schwarz's conclusions themselves. "Confrontation has become the accepted form of antagonistic international intercourse" (p. 208) whereas intervention "is not needed, . . . not safe, . . . [and] not right." Yet the best examples of confrontation come from the bygone Khrushchev era, whereas the lingering malignant cases of intervention in Czechoslovakia and Vietnam are still with us (p. 215), and the confrontational aspects of the latter two episodes have been notoriously mild or ineffectual (neither is discussed by Schwarz under "Confrontation"). Such ambiguities are of course part of reality and are properly reflected in the

author's interpretation. If life is like that, it may be too much to expect more of analysis.

I. WILLIAM ZARTMAN

New York University

Conflict in World Politics. Edited by Steven L. Spiegel and Kenneth N. Waltz. (Cambridge: Winthrop Publishers, 1971. Pp. 474. \$4.50, paper.)

This is a book of original essays dealing with various types of conflict situations—those involving Great Power conflicts, hegemonic conflicts, violent conflicts, muted conflicts, and policy conflicts. As one would expect, the essays are of varying quality and insight, although each contributes to a clearer understanding of the particular situation being discussed.

Aside from examining specific contributions, it is always difficult to evaluate a book of essays except in terms of its overall conceptualization and the way that conceptualization is reflected in the organization and content of the work. In evaluating the present work, then, one must note that the categories are not exclusive: Great Power or hegemonic conflicts may be either violent or muted. Nor are the principles guiding the selection of cases very clear. For example, is the conflict between East and West Germany really (or primarily) violent? If one were to answer in the affirmative, one would have to define violence and distinguish between its latent and actual forms. The larger question is whether all conflicts between divided countries are bound to be violent, as the scheme of the book suggests.

Another example of the questions that arise out of the organization of the work: Internal conflicts are subsumed under the category of "violent" ones, with case studies of South Vietnam and Nigeria/Biafra. No one would question that the categorization applies to these cases. But many internal conflicts (I would argue the great majority of them) are resolved short of violence: were this not the case, any given system would surely break down.

One of the most interesting categories in the book is that of "muted conflicts," with its subdivisions into "three-sided conflicts," "international cooperation and potential conflict," and "social and economic cooperation and conflict." Some particularly fine case studies are presented here, but some difficult questions do linger. For instance, surely we are not asked to assume that all, or nearly all, three-sided conflicts are muted. Also, one can fully accept the proposition that cooperation does not preclude conflict; yet, the inclusion of case studies deal-

ing with conflict between Portugal and South Africa, Great Britain and France, or the United States and Canada requires a very broad conceptualization of conflict. In short, I wonder whether a narrower, tighter, definition of conflict would not be more illuminating. As it stands, the "muted conflict" category appears miscellaneous; one cannot tell just what overriding or underlying principle holds it together.

Are there, then, common elements in conflict? The editors suggest five: every conflict has origins, scope, issues in dispute, methods-instruments-strategies, and outcomes. One cannot fault this scheme, but one must say that it is very minimal—and also that the cases presented are so varied that it is easy to lose sight of the basic organizing principle.

The question, of course, is whether it is possible to generalize more fruitfully about conflict—especially when the term is as broadly conceived as it is here. The editors appear to be somewhat ambivalent on this point. Professor Spiegel, in his introductory comments, seems to argue against generalizing, particularly when generalization is based on cross-cultural, cross-national, and comparative analysis. But he does, nonetheless, go on to make some rather broad statements about causes and consequences of conflicts. His conclusions are sound though not startling: Great Power intervention in local conflicts tends to intensify contention; distance tends to diffuse relations and facilitate conflict resolution; proximity intensifies both cooperation and conflict. One feels that one has known these things all along, though it is always reassuring to have them reconfirmed by case studies. I have the same reaction to the "questions about familiar themes" which, Spiegel writes, the essays raise: e.g., the challenge to the idea that postwar conflicts were ideological; the recognition that the relationship between American capitalism and Russian communism is "largely peripheral and partly irrelevant" (p. 10) to most conflicts being waged. Another point that emerges from the cases, namely that territory still does remain an issue of importance in contemporary conflict, strikes me as more controversial, since others have argued that this is not the case.

Professor Waltz concludes the book with a chapter on "Conflict in World Politics." I consider this the most important contribution to the work. The concept is elusive, as Waltz clearly recognizes. He views international conflict from a systems perspective in the sense that these conflicts do not emerge out of individual behavior but can best be viewed in terms

of "an actual or postulated organization that overarches the units" participating in the conflict (p. 457); that conflicts are best viewed also "in terms of the effect of the environment on them and . . . their effects on the environment . . ." (p. 457). The point is that the structure of an international system at any given time is related both to the inception and the consequences of conflict.

Waltz adopts Kurt Singer's scheme ("The Resolution of Conflict," *Social Research*, 6 [1949]; "The Meaning of Conflict" *Australasian Journal of Philosophy*, 27 [December 1949] and *The Idea of Conflict* [Melbourne University Press, 1949]), which states that there are only a limited number of conflict behaviors: (a) regression, (b) integration, (c) withdrawal, and (d) resolute contention. The characteristics of these behaviors, respectively, are: (a) to stay behind a barrier; (b) to try to remove or modify the barrier; (c) to leave the field; and (d) to resolve to destroy the barrier. Waltz then matches the structure of international systems to these modes of behavior. Thus, he writes, regression appears to be typical in loose associations of states (US-USSR); integration is typical for close and cooperative associations (France-Germany); withdrawal is appropriate to nonassociation of states (a null category); and resolute contention is found when states are closely associated in an oppositional framework (as in East and West Germany). I happen to think the last example could be questioned and evidence adduced for a muting of conflict between the two German states. Similarly, North and South Korea no longer seem to fit into the "close association-oppositional" framework and appear to be moving toward some type of integration.

One could question whether all of this really does tell us a great deal that was not known before—though the scheme is useful in any case. One could worry about definitions of situations: Precisely what, for instance, changed the U.S.-Soviet relationship from "close association in an oppositional framework" to "loose association?" Most important, one might ask why, if this framework had so much to recommend it, it didn't serve as the organizing focus of the book.

Still, Waltz, with his characteristic insight and fine analytic skill, does contribute significantly to our understanding of the linkages between system and behavior in the discussion of the various types of conflicts. His conclusion that conflict is ubiquitous and will remain so,

pending the unattainable condition of the abolition of international relations, is sobering but, in my judgment, entirely valid. In addition to the factual data and analyses presented in the essays that constitute the bulk of this book, Waltz's comments, even though they do not (and don't pretend to) cover the entire range of questions we must ask about conflicts, alone are worth the price of admission.

FRED A. SONDERMANN

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Swords and Plowshares: A Memoir. By General Maxwell D. Taylor. (New York: W. W. Norton, Inc., 1972. Pp. 434. \$10.00.)

Tall, strikingly handsome, an acquirer of languages, holder of a distinguished war record in World War II and in Korea, and the author of a book protesting the Eisenhower Administration's excessive reliance on nuclear weapons, General Maxwell Taylor was conspicuously the kind of man who would captivate the late President Kennedy. The latter, always hungering for what he took to be "excellence," brought this former Army Chief of Staff out of retirement on the morrow of the Bay of Pigs fiasco to begin a second and far more important career than before. First special consultant to the President, then Chairman of the Joint Chiefs of Staff, later Ambassador to Saigon, he then returned to be consultant to the late President's successors. In these posts he played a part in the dismal Vietnam story second to none played by any other military man.

His memoirs are a contribution to history because of what he inadvertently tells us about himself, not for what he says of those important events in which he figured so prominently. He is somewhat casual about these events, about which other sometimes conflicting reports make his own often appear unpersuasive. A noteworthy example is his insistence that the Wheeler-Westmoreland request for 206,000 additional men after the Tet offensive of 1968 was no request at all but reflected merely contingency planning. If so, some very bright officials, including Clark Clifford and Townsend Hoopes, strangely misconstrued it. One notices also with some wonder that the author professes even now inability to understand why the Communists launched that offensive!

One was perhaps forewarned about his views from various passages in *The Uncertain Trumpet*, published in 1959. It read somewhat oddly even then that American intervention at Dien Bien Phu in 1954 was "unfortunately" prevented by lack of the appropriate conventional

forces. He then added: "This event was the first, but not the last, failure of the New Look to keep the peace on our terms" (p. 25). One remembers that Dien Bien Phu occurred only one year after President Eisenhower had finally got an armistice ending the three-year war in Korea.

In the present book we do not read far before we get similar indications of the author's set of mind. One concerns the curious postmortem he did for President Kennedy on the Bay of Pigs, given a chapter here. He does mention among many other things that were apparently done wrong the fact that the invading force was much too small to have the slightest chance of success, but he does not give that point the dominating position it deserves. Against it all other considerations were virtually insignificant—except the question which he does not try to answer: How *could* it have happened? He finally decided, but only as an afterthought following his final briefing on the subject to the President, that the operation should have been cancelled. In a later place he casually observes that surely "one of the prime lessons of the Bay of Pigs" is that if we take another such "first step" we must "be prepared to go all the way" (p. 248)! According to Arthur Schlesinger's account, President Kennedy reached a very different and far sounder conclusion about the same affair and the relevance to it of considerations of prestige. To Kennedy a loss of face or prestige was of only temporary moment, not worth paying too high a price to avoid or redeem. To Taylor it is all-important; for him a country like the United States must suffer a near-fatal blow in accepting any such loss.

We are thus prepared, though not sufficiently, for the author's concluding chapters dealing mainly with Vietnam. What one has increasingly suspected becomes in these last chapters firmly established: To this man, reared in the society and the schools of Kansas City before he ever went to West Point, democracy has no real value. In a chapter entitled "Lessons from Vietnam" he asserts that when a president in the future again decides on an intervention requiring the use of armed force, "he will be well advised to obtain Congressional approval before committing himself and then to seek a declaration of war or emergency to silence future critics of war by executive order" (pp. 405f.). One hardly knows what to be more amazed at, the spirit of this proposal or its author's ignorance of his country's laws. The final chapter is one long declamation against criticism, not omitting the now customary at-

tack on "the media," which he feels should be at special pains to find the "wholesome" side of American life as newsworthy as the Calley trial.

Well, here is a soldier who does not understand or cherish democracy; might he still not be a good top-ranking soldier? Perhaps, but this officer cannot be evaluated simply as a commander in the field. It fell to him to play in a quite special and notable way the role of the soldier-diplomatist, not only as an ambassador but as a direct adviser to presidents.

To Taylor, war or any other major use of military force appears to have only one purpose, and that is self-engendered. The goal of fighting is to win. To accept anything short of that is "humiliating." The consequences in Taylor's mind of what he regards as "humiliation" are unclear, but they are obviously unspeakable.

Most other senior generals of our own and of previous generations have felt this way. But one somehow expected more of Taylor. His urbanity and facility with languages are no doubt exceptional, but intellectually and spiritually there is much less here than one had been encouraged to believe.

BERNARD BRODIE

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The Unique Partnership: Britain and the United States. By Arthur Campbell Turner. (New York: The Bobbs-Merrill Company, Inc., 1971. Pp. 216. \$6.95.)

Arthur Campbell Turner, who is Professor of Political Science at the University of California, Riverside, has written a well-informed, understanding, highly readable and gently amusing book about Anglo-American relations. It is agreeable to have explained in a footnote the difference between "swithering" and "haver-ing." It is not quite clear why the book is called "The Unique Partnership," a title evidently suggested by Mrs. Turner. No one can doubt that the partnership is "genuinely unique: demography, language, literature and the carryover of political institutions have made it so," as the dustjacket says. But can Professor Turner think of the relations between any two states which are not unique? International relations are *sui generis* because states are *sui generis*.

Anyhow, there are certain special things about this "special relationship." There was the migration of over 15 million people from Britain to the United States between 1815 and 1940, with all that it meant in lingering family connections and cross-fertilization of ideas and

social customs: the consanguinity of political thought and values, however different their institutional expression on either side of the Atlantic. There was, and still is, the common language, the political implications of which Bismarck so well perceived. There was the British awareness about the turn of the century that imperial decline was foreshadowed, and that America must be watched, cultivated, tutored perhaps, as Britain's successor; Professor Turner is very good on all this, though he should look again at his statement (p. 61) that in 1922, at the Washington Conference, the U.S. Navy "was not thought of seriously as a possible enemy fleet" in Britain. It is remarkable how many eminent British people did think so, absurd as it now seems.

And there was the partnership in the two world wars, underlined by Britain's painful interwar discovery that without the United States, power in Europe could not be balanced and the British Isles could not be secure. It is hard to see why Turner calls the Munich agreement of 1938 "the institutionalization of surrender" (p. 61). It could more properly be described as the acid test of "surrender" (in any case is that the right word?) as a policy since the mass of British people then finally learned, for the first time, the meaning of Hitlerism. It is a pity, too, that there is not something in this third chapter about the failure of the Anglo-American partnership in the 1930s in the Far East. Too often is it forgotten that an important factor in the weakness of Britain's European policy at that time was her inability to hold the line against Japanese expansion in the Far East without American help. Franklin Roosevelt could not even co-ordinate American protests to Tokyo with British protests against infringements of foreign rights in China in view of the isolationist state of American opinion.

There is, however, a good chapter on the Second World War, the high noon of Anglo-American accord, and another on American attitudes towards decolonization in the British Empire after 1945. Professor Turner does well to emphasize the ambivalence of these attitudes: the primitive American wish to demolish the Empire, so brusquely voiced by Mr. Roosevelt at Yalta in February 1945, followed by the fear that Britain would retreat from Empire so fast as to leave America alone to police the world. After that, the "unique partnership" rapidly lost its meaning. The Commonwealth, Professor Turner frankly writes, never meant anything to America, and when in 1968 Britain decided to cut her military presence east of Suez,

America could only look at Britain henceforward as merely one of her 40-odd allies, not necessarily the strongest or most reliable. The test ban treaty of 1963, writes Turner, marked the end of the road for the partnership, unless, as he suggests in the conclusion, Britain were to become, not one, but thirteen states in the American union (strange irony!).

But Turner is less than fair, and less than correct, in assessing Britain's position vis-à-vis America since 1945. He argues that Winston Churchill was inconsistent in warning America against Soviet expansion in his Fulton, Missouri, speech in March, 1946, when he himself "handed over" certain European countries to Stalin at their notorious meeting in Moscow in October, 1944 (p. 180, n. 40). He forgets Churchill's rejected pleas to Roosevelt during the war that we should agree with Stalin about his territorial claims in Europe while we were still under arms and Russia needed our help. Churchill never denied that Stalin must be compensated in Eastern Europe; the question he raised in March, 1946 was what limit should be set to Soviet expansion in the west. Professor Turner also criticizes Britain (p. 180) for wanting summit talks with Russia in 1953 and 1955 when she had called for armed resistance to Russia in 1946. But Americans have practiced plenty of summitry with both Russia and China lately and will no doubt continue to do so. The fact is that in the years after 1945 America followed where Britain led in policies towards the Communist world. Britain aroused America to the need for an effective military balance in Europe in 1945 and 1946; America at length and suspiciously acquiesced. In the 1950s Mr. Dulles identified negotiating with Communists as parleying with the devil, Britain urged peaceful, though armed, coexistence, and a day came in 1963 when President Kennedy said "me too." Britain's role in helping guide America, now admittedly finished, was never superfluous.

F. S. NORTHEGE

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German-Polish Relations, 1918-1933. By Harold von Riekhoff. (Baltimore: The Johns Hopkins Press, 1971. Pp. xi, 421. \$15.00.)

German-Polish relations between 1918 and 1933 constitute perhaps the most severe test of the controversial Versailles system, and there is indeed a great need for a balanced, in-depth analysis of the specific issues which shaped the relations of these two countries, each in its own way a creation and a victim of the postwar set-

tlement. Professor von Riekhoff's aim, and his chief contribution, is thus not so much to produce another history of the period but to provide an analysis of the problems which beset it, the positions taken on them by the two governments and the consequences which followed.

In a series of interlocking and well-documented essays he traces separately the development of the major points of contention and the efforts to overcome them, particularly in the Locarno era. Perhaps no other author has so skillfully clarified the tangled and controversial problems associated with economic relations and the treatment of minorities—problems which played a major role in the strained relations between Germany and Poland in this crucial period. Nor has anyone so effectively highlighted and documented the roles of the individual diplomats and government leaders on both sides. One might well use this volume as a case study of modern diplomacy and its limitations. Professor von Riekhoff's book is an example of the excellent work which can be done on the basis of archival and other sources now available and deserves recognition as a major scholarly contribution.

His is, however, not likely to be the last word on the subject, at least insofar as interpretation and conclusions are concerned. His inclination to build a case against German policy, a theme which grows noticeably in the second half of the book, tends to upset the balance that marks the work otherwise. Revisionism inevitably emerges as the devil of the piece, and Weimar Germany stands condemned for that "natural German tendency" to base its policy on "romantic strivings" rather than reality and its "habit of judging everything connected with Poland as inferior" which blinded even her best statesmen and prevented reconciliation (p. 383).

In the end, the "only redeeming grace" in Weimar policy vis-à-vis Poland was its commitment to peaceful means to achieve its revisionist goals, and even this virtue remains in doubt since there is no way to know whether it was the result of principle or "merely a temporary concession to the military weakness under which Germany was then operating" (p. 269). The admittedly "completely uncompromising Polish attitude" was, on the other hand, simply a fact of life which could not be changed and thus one to which Germany had to adjust or be held accountable (p. 291). Since the roles of Soviet Russia and France and the other powers are also viewed primarily in this context, the author has not really fulfilled his stated inten-

tion of projecting German-Polish relations "onto a wider screen that features the inter-war diplomacy of the European powers" (p. ix).

On balance, however, there is no doubt that this is a very important study which, even with the usual faults of monographic studies, goes to the heart of the problem of peace in Europe in the period which laid the foundations of the contemporary world and which thus holds vital lessons for today. Professor von Riekhoff notably praises the courage of Germany's Social Democrats for their active promotion of economic cooperation and political accommodation in the face of great obstacles, including German public opinion. The publication of his book thus fittingly anticipated the restoration of German-Polish relations some 27 years after World War II, a time lapse nearly twice the entire life span of the Weimar Republic.

GEORGE BRINKLEY

University of Notre Dame

Peking's U.N. Policy: Continuity and Change.

By Byron S. J. Weng. (New York: Praeger Publishers, Inc., 1971. Pp. 344. \$12.50.)

This is a welcome addition to the small but growing collection of books which analyze Chinese foreign policy through case studies rather than by textual exegesis of *Peking Review*. Professor Weng's survey of all statements and actions from the People's Republic of China (PRC) relating to the United Nations virtually down to its admission in 1971 fills an important gap in the factual literature. No less important is his analytical insight, which probes behind the chronological account for the assumptions and political dynamics that explain both continuities and change in Peking's posture toward the UN. The strength of his analysis is evidenced by the consistency between his projections of probable PRC behavior in the UN and developments since the book was finished.

The first part of the book, a general overview of Chinese foreign policy, is the weakest portion, perhaps because it attempts too much in too short a space. Weng's attribution of "rational" and "irrational" characteristics to "national interests" is unfortunate. He makes no effort to speculate on how conflicting bureaucratic interests, as for instance between those pursuing diplomatic interests and those responsible for supporting revolutions abroad, affect PRC policies. But Professor Weng's analysis of policy means is valuable in placing the UN "as actor" and "as arena" within Mao Tse-tung's concepts of "contradictions" and "struggle." This correctly anticipates both the seeming con-

fusion in China's attitudes from 1949 to 1969 and its behavior since entering the UN in 1971.

Part II, the bulk of the book, is an unusually comprehensive review of Peking's statements concerning the UN examined within the context of ongoing domestic and international developments. Professor Weng's periodization is empirically derived within a theoretical framework evaluating policy as positive-negative, offensive-defensive, active-reactive, and direct-indirect. Despite the subjectivity of his terminology, the framework is nonetheless useful for demarcating tactical as opposed to strategic changes in posture. While the content analysis is purely qualitative and should be tested by more rigorous quantitative measurements, its logical inference pattern is persuasive.

Part III recapitulates the evidence in terms of China's legal approach, the instrumental use of the UN, and Mao's "thought and psychology." Although relatively brief, these three portions are neither superficial nor tendentious. They conclude the study with a dispassionate, multi-level overview which should be of value to students of China and of international organization. Professor Weng offers alternative guides to understanding Peking's postures and policies which account for unique aspects without isolating China as idiosyncratic among other states in the international system.

It is unfortunate that the publisher did not take greater pains with proof reading, especially of dates. The shortcoming is particularly reprehensible in view of the book's inordinate price, which cannot be justified by any visible production costs. Mao's first trip to Moscow was in 1949 (not 1931—p. 31); he enunciated the "lean to one side" policy in 1949 (not 1957—p. 31); the Gulf of Tonkin incident occurred in 1964 (not 1965—p. 56). It was Khrushchev who "visited Peking in July 1958" (not Mao—p. 121); the journal is *Asian Survey* (not *Agents Survey*—p. 256); etc. Perhaps haste resulted from an effort to mesh publication with public attention to the subjects covered. If so, the errors are a minor fault compared with the major strengths of the study. It is a happy coincidence that so scholarly a work becomes available just when the PRC assumes its long-delayed responsibilities in the United Nations.

ALLEN S. WHITING

University of Michigan

Intergovernmental Military Forces and World Public Order. By Walter L. Williams, Jr. (New York: Oceana Publishers, 1971. Pp. 703. \$19.40.)

This lengthy tome is evidently the doctoral

dissertation of a Yale Law School graduate who at the time of publication was a major in the Judge Advocate General Corps of the U.S. Army specializing in international law and stationed at SHAPE. It is an extensive compendium of concepts and facts concerning multinational and supranational military forces, primarily of the peacekeeping variety. It is highly theoretical both in its approach and in its methodology.

The writer's thesis, based on the positivist legal philosophy of Justice Holmes, is that "without some sort of police force," voluntary international law simply does not work well in the world. At the time he launched into his subject (or wrote his dissertation) the author was optimistic about the prospects for a U.S.-Soviet agreement on peacekeeping. He cites with approval the proposals (and equally optimistic tone) of the 1969 UN Association panel on "Multilateral Alternatives to Unilateral Action," and he correctly diagnoses the trend toward *détente* on the part of the Soviets toward their western flanks. But alas, U.S.-Soviet differences over Security Council vs. Secretary General in even noncoercive peacekeeping still consign it to the deep-freeze. I also think in retrospect that at least my own view as a member of that 1969 panel was perhaps too optimistic. Given increased U.S. willingness to use its own forces unilaterally, the arguments for improved and more reliable peacekeeping capabilities are stronger rather than weaker. But unfortunately, the author of the Nixon doctrine has not yet recognized its principal corollary—the need for a better multilateral alternative to fill the gaps left by national dropouts such as the U.S.

Mr. Williams's book is, however, really not a policy document, or even a historical text, so much as it is what he calls "policy-oriented contextual analysis" (p. 7). This description, plus the method he follows for the remaining 700 pages, are derived from a well-known framework developed over the years at Yale Law School by Professors Harold Lasswell and Myres McDougal. It is a method which even one sympathetic to their views finds too rigid to achieve the sought-after improvement in communication about complex social and political phenomena. I shall have more to say about this presently.

The first section of the book is a historical survey of military cooperation from early times to World War II. Like most doctoral dissertations, it leaves no stone unturned, in this case going back to the Delian League. We are led through the various coordinating bodies in World War I and through the League of Na-

tions, with its not half-bad record of sanctions accompanied by military observers and enforcement personnel (as in Vilna, Leticia, and the Saar), to the cooperative forms devised in World War II.

The following chapter carries us into the more fragile if not downright spongy ground of the analytical format prescribed by the Yale school. The premise of this discussion is the interdependency of the world community—fair enough. On this premise is superimposed a theory of “world social process” which subsumes the “world process of military cooperation” (p. 90). The Yale morphologists and taxonomists are certainly entitled to call what is happening in the world a process of military cooperation, although few observers I know would put it in that particular box. But their examples perforce are somewhat unconvincing and in the end inadequate to the rigor of the model. (Examples cited include: the Inter-American Combined Quarantine Force in the 1962 Cuban missile crisis, which I always thought stood as a classic example of two-power confrontation. I suppose by a vigorous stretch of the imagination it could be called an inter-American combined quarantine, but as a good example of multilateral action it strikes me as little short of ludicrous. Another example given is the Warsaw Pact forces’ occupation of Czechoslovakia in 1968, which as an exemplar seems to me hardly supportive of the author’s categorical imperatives that link “some sort of police force” to improvements in the status of international law.)

The author then moves to the relatively familiar ground of United Nations intergovernmental military forces. I cannot recall another writer on the subject who manages to subsume all aspects of the “era of peacekeeping” under the legalistic rubric of “claims.” Perhaps if I had gone to law school I would understand why this is a good rubric for the political and diplomatic processes that produced some peace-keeping operations and inhibited some others. As a layman I find the author unhelpfully locked into an approach in which everything is an example of applying to “authoritative decision-makers for settlement of claims.” This in turn is an analogy of the “constitutive process” of Messrs. McDougal and Lasswell, according to which all interactions are ascribed to authoritative decision makers who allocate goods, services, and other values to contending claimants. I don’t want to do any of these theory builders an injustice, but I must say it makes of the dynamic political-diplomatic-military process a pretty bloodless courtroom in

which King Solomon is mated to hard-core social science with a J.D.

Part 2 lays out what the author calls “selected trends of authoritative decisions,” which, translated, means some but not all issues of competence of international organizations to organize and field peacekeeping forces. The remainder of the book is a comprehensive and useful treatment of commitments on the part of governments to provide, assist, and receive intergovernmental military forces.

Again, here, however, the framework the author imposes upon himself requires that he link all of this to what he calls “community policies.” But really now, to understand its successes and failures, is it more useful to relate UN peacekeeping to the complex games played between nations within the weak fabric of voluntary international cooperation—or to pretend somehow that what we have been witnessing are “communities policies”? I myself thought our problem was the *absence* of true community, and of the crucial common values necessary to underlie it. Describing the situation correctly tells one something important about what needs to be done to remedy it. Giving the problem a highly theological cast resting on categorical imperatives seems to me to serve only recondite purposes.

This is doubly unfortunate, since the literature on intergovernmental military forces is about as thin as its present prospects. We should welcome any new contribution to the field, but what is needed is better rather than worse linkage to the real world, where peacekeeping still looks alien and suspect to the *real* “authoritative decision-makers.” Exercises like this, hobbled by the rigid canons and cabalistic formulas of the monastery, serve to put scholars several further removes from the communication urgently needed to help link national behavior and greater-than-national action. For those to whom world public order is a desperately-needed good, the time may be at hand to protest louder than in the past against the obscurity into which that norm has been forced by some of the most influential contemporary scholastics.

LINCOLN P. BLOOMFIELD

Massachusetts Institute of Technology

Global Politics of Nuclear Energy. By Mason Willrich. (New York: Praeger Publishers, 1971. Pp. xii, 205. \$15.00.)

Mason Willrich, professor of law at the University of Virginia, has written a highly competent study of the political implications involved in harnessing nuclear energy for peaceful pur-

poses. Wisely, he first explains the technological essentials of this new energy source and does so in clear and simple language. He aptly summarizes the major problems associated with the development of nuclear power, both those that spring from its technology and those that society imposes. He discusses the implications of nuclear energy in general, as well as of certain specific technological developments—breeder reactors, Project Plowshare (peaceful uses of nuclear explosives), and controlled fusion, as examples. While Willrich treats many different aspects of nuclear energy, comments here will be limited to his discussion of political ramifications.

First and foremost Willrich asks, "Will existing political institutions and policies be adequate to exploit the opportunities and to contain the dangers implicit in the peaceful uses of nuclear energy?" He feels that only when nations are willing to give up more of their sovereignty for interdependence will nuclear energy be exploited properly on a global scale. However, he doubts that nations are about to shed their nationalism. The issues of national security, of proliferation of nuclear weapons, of possible clandestine diversion of fissionable fuel to war purposes, of the attitudes of less developed countries, of cooperative development on an international basis and of others are viewed with this in mind.

With consummate skill, Professor Willrich lays out the problems, but in my opinion, the author underestimates the amount of nuclear development that can take place worldwide without a major restructuring of the nation-state system. Since World War II cost has proved the greatest obstacle to the growth of a nuclear industry. Once the economics of nuclear energy become sufficiently attractive, nations most likely will find ways to accommodate the technology.

This is not to discount all the problems that Willrich outlines. They are very real and impose major obstacles to nuclear development. Yet the energy problems of mankind are so immense that political exigencies will not kill any technology that gives promise of helping to solve them. Political factors have conditioned and will continue to help set the pace with which society is willing to adopt such technologies. Willrich is clearly right in asserting that the problems he discusses will slow the pace of nuclear developments on a global scale, but history shows that sooner or later society accepts beneficial technologies and makes its political adjustments. The major question is, "Will nu-

clear energy prove beneficial on a worldwide basis and if so when?"

RALPH SANDERS

Industrial College of the Armed Forces

Le Programme des Nations Unies pour le Développement. By S. S. Zarkovich. (Paris: Presses Universitaires de France, 1970. Pp. 246. Frs. 26.00.)

This book, written by a Yugoslavian official of FAO and former professor of statistics at the University of Belgrade, presents a critical analysis of the working of the United Nations Development Program. The overall thesis of the book is that UNDP could be a great means for solving some of the problems of the underdeveloped world, if only a different system were set up to make it work. In the beginning of the book the author claims that he considers UNDP "... the most important enterprise ever set up to extend the field of knowledge and to permit human work to improve the material conditions of existence" (p. 23). Having said this, he starts a very critical indictment of the existing system, then presents his own suggestions for improvement and ends with some more critical remarks about the Jackson report on UNDP, which was published a year before.

The author condemns the system of sending foreign experts to underdeveloped nations. They come, do their work and go, but fail to transfer their technical and expert knowledge to the people in the receiving country. The changes and improvements brought about by the expert's activities are thus of a rather limited and short-term character. Moreover, many of the experts lack the necessary qualifications; they are nevertheless maintained in their positions, as the receiving countries do not want to criticize their work for fear of losing all further financial and technical assistance. Dr. Zarkovich is hardly less critical of two other activities of UNDP: the foreign scholarships and the training centers and seminars organized by UNDP. The people from the underdeveloped countries who should profit from these programs are often insufficiently qualified, causing the intellectual level to drop below the level originally intended. The scholarships are a waste of money. Their main functions, according to Dr. Zarkovich, is to promote foreign travel. In addition to these shortcomings, the author strongly criticizes UNDP for being excessively bureaucratic and for lacking the flexibility to adapt itself to changing conditions. "Coordination" is the catchword used to deal with all kinds of problems. As the number of

coordination activities increases, so does the number of coordinators and administrators, who have become a more important feature of UNDP than have the technical people who carry on the development activities in the field. UNDP has lost its former vitality and has become what Dr. Zarkovich calls a "tired organization" (p. 135).

While praising the Jackson study for at least acknowledging that something is wrong with UNDP, Dr. Zarkovich feels that it does not offer the right remedies; it puts too much trust in more and better kinds of inspection and surveillance, assigning even more tasks to central headquarters. While this would perhaps be a good solution if the UN were a full-fledged world government, Dr. Zarkovich claims that at the present state it would only mean a further wasting of resources, without creating the improvements the Jackson report envisages.

What Dr. Zarkovich has in mind is a much stronger emphasis on the capacity of the underdeveloped countries to help solve their own problems. They themselves should carry part of the responsibility for the success or failure of a development project. He hopes to realize this by setting up international centers for training and research, financed by UNDP and having strong ties with similar institutes in the underdeveloped countries themselves. The training and research programs of the envisaged centers would be geared to the needs and special inter-

ests of the underdeveloped world. The existing programs of UNDP would have to be disbanded.

Having finished reading Dr. Zarkovich's forcefully presented case, the reader is left with the feeling that some very useful criticism has been directed at an organization which already has generated quite a bit of dissatisfaction (cf. the Jackson report). One wonders, however, whether the proposed remedies will work; whether the "holy war against bureaucracy," which according to Dr. Zarkovich, should become the "great task of our generation" (p. 214) will be won in this way. Why do international organizations in general and development programs in particular so often get bogged down in bureaucracy? Is this bureaucracy of a different and stronger kind than the one we are used to on the domestic scene? Only when these and similar basic questions have been answered, shall we know whether and where remedies can be found. Dr. Zarkovich has not tried to answer these questions of a more general nature. His book is more practical, based on an extensive knowledge of UNDP operations. It should be of interest to students and practitioners in the field of international organizations to make them think or to get them annoyed, or possibly both.

PETER R. BAEHR

University of Amsterdam

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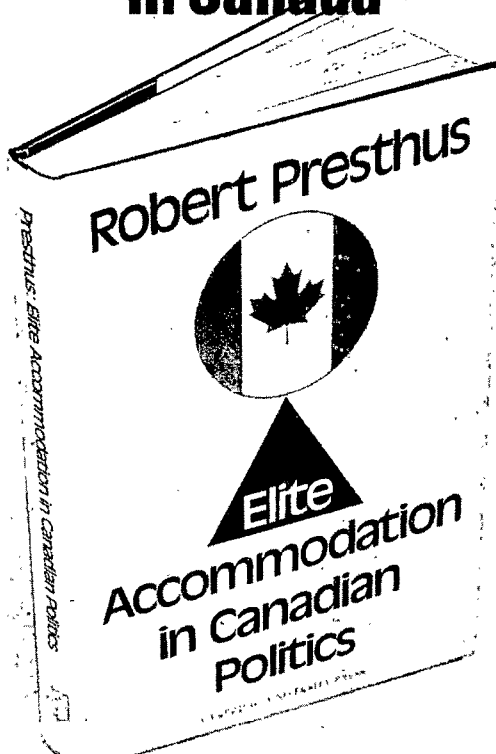
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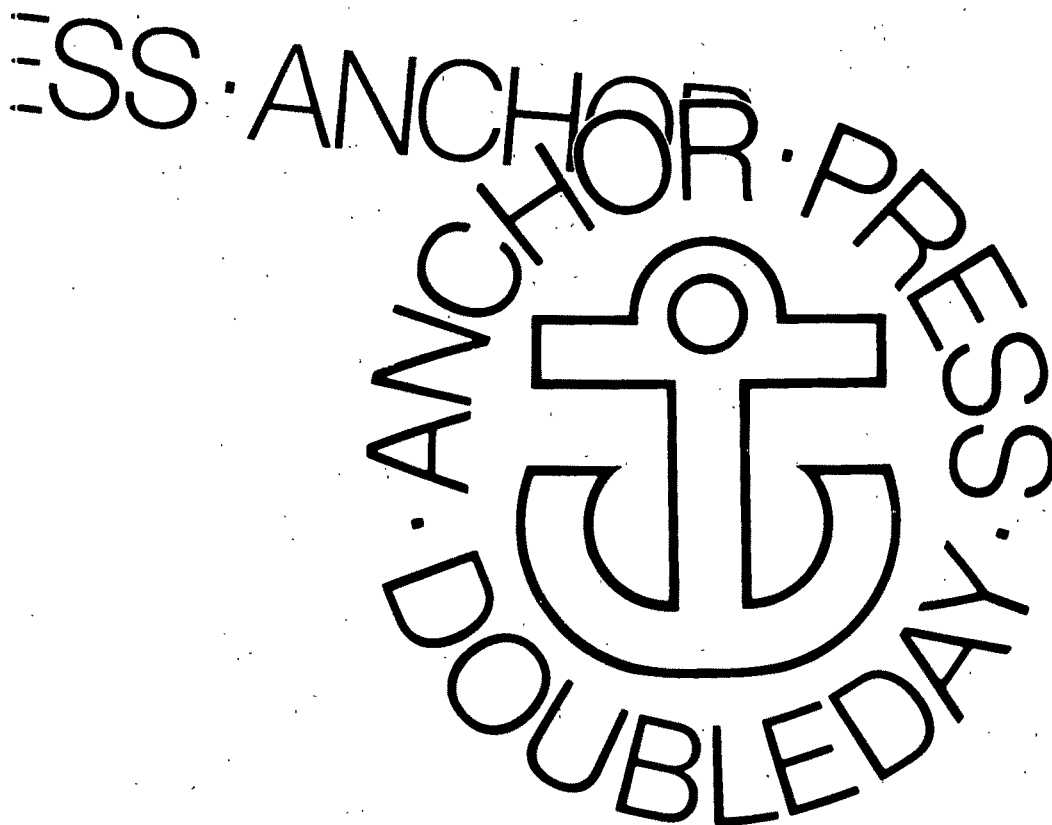
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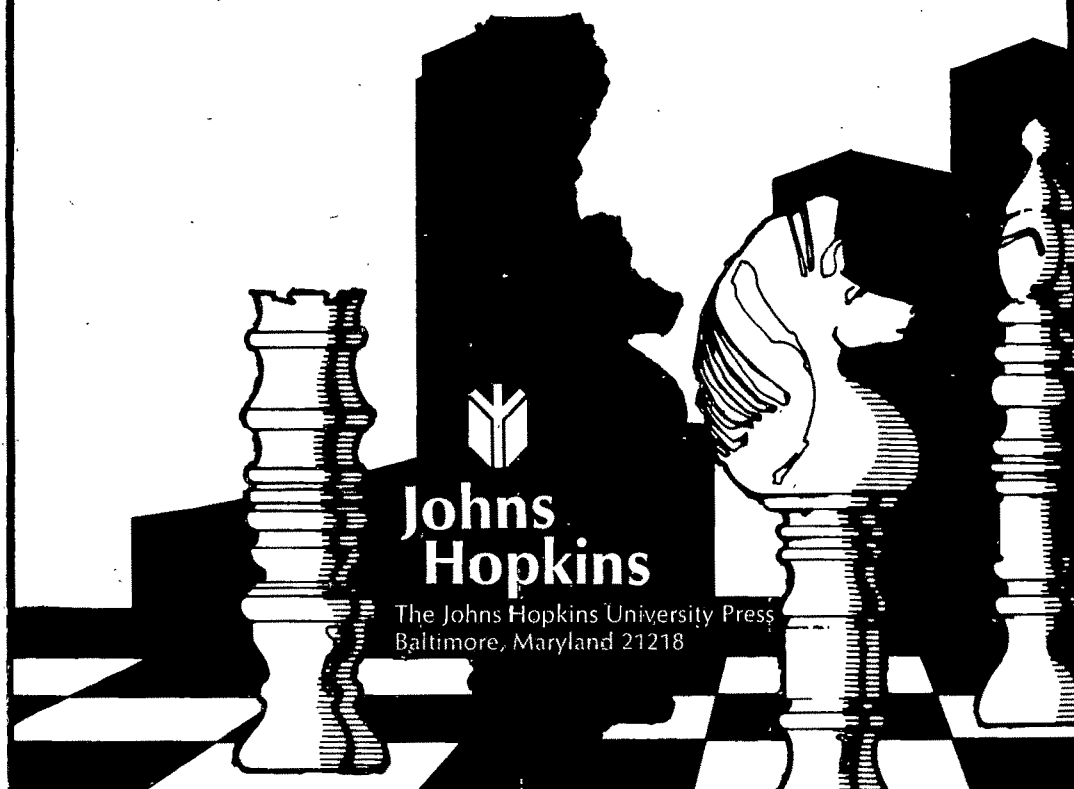
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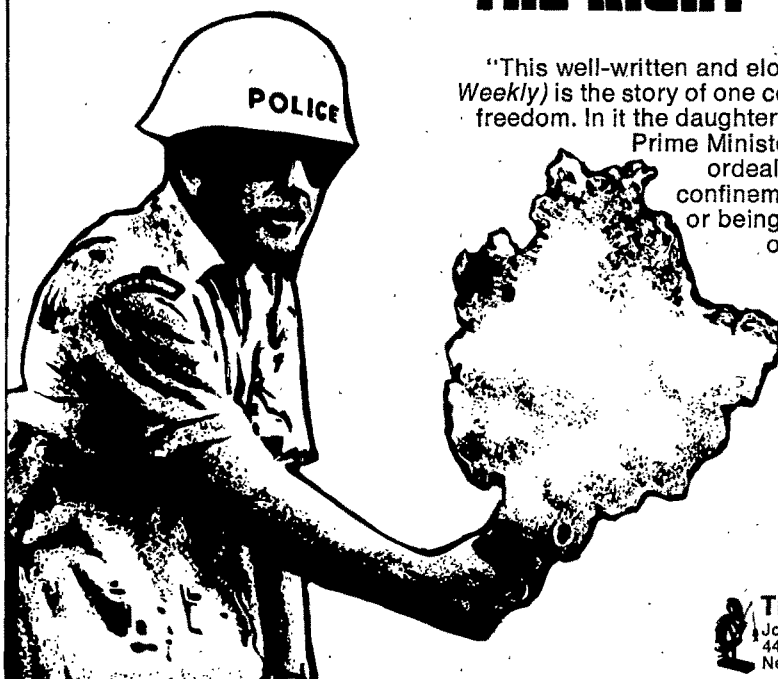
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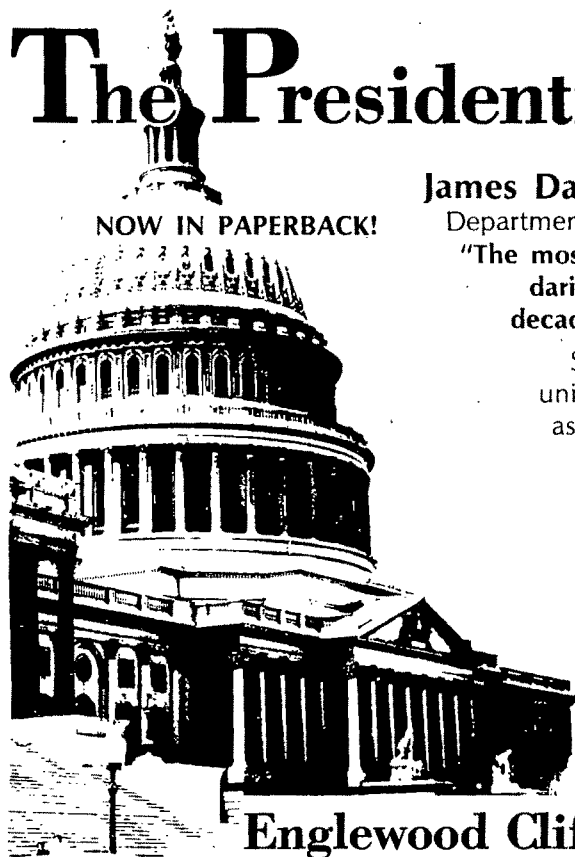
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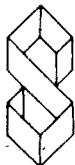
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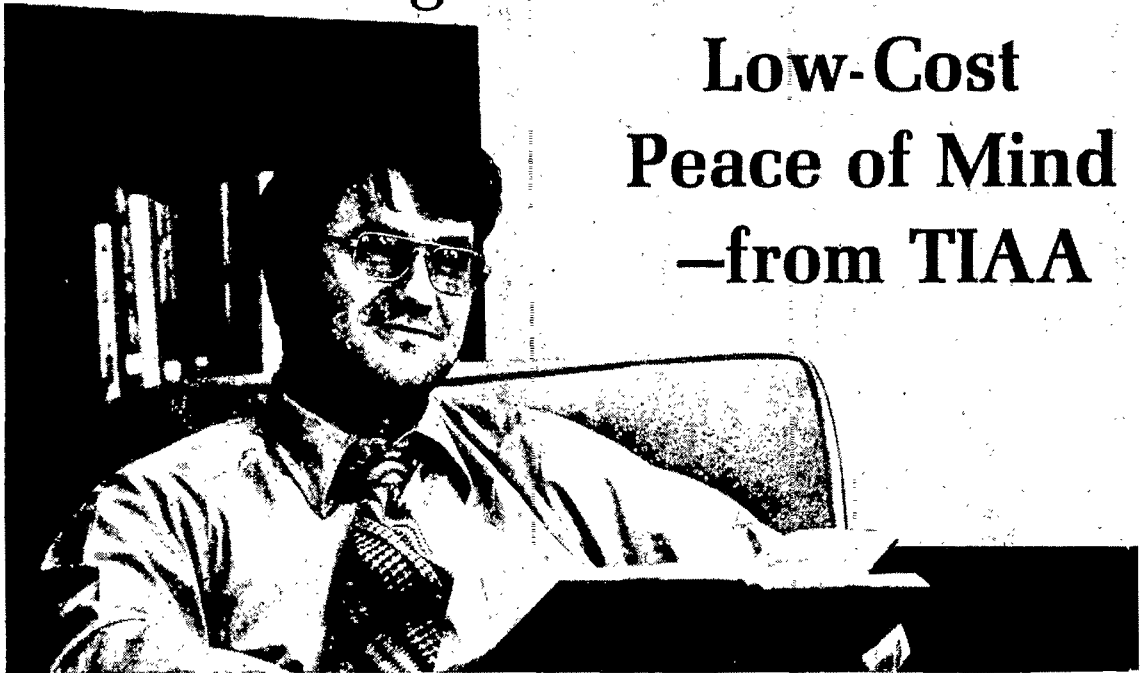
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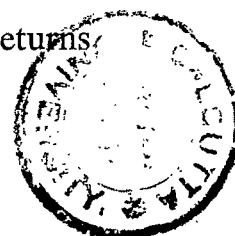
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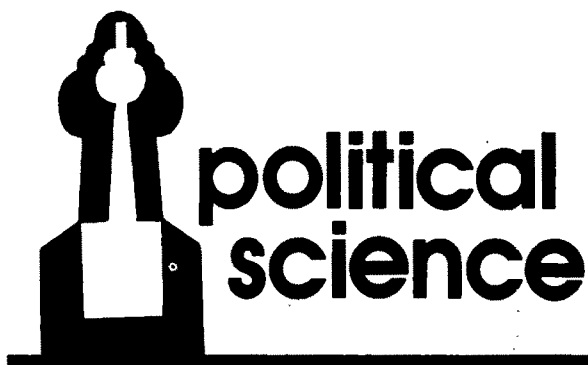
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Caste and the Decline of Political Homogeneity

A. H. SOMJEE

Simon Fraser University

The relationship between caste and democratic institutions in India has been the main focus of attention of the students of Indian studies for nearly a quarter of a century. It has presented a many-sided challenge to social analysts as a problem in structural-functionalism, social interactionism, and political dynamism. Invariably, in this relationship, the political element is thought to be growing progressively stronger and at times catalyzing other forces. It has as a result inspired a lot of lively writing on the theme of social change. At the theoretical level a variety of explanations have been offered for the understanding of the interaction between the traditional social organization based on the principle of hierarchy and the new political institutions inspired by the norms of equality. Some of the explanations have been motivated by a search for an all-explanatory theory of the Indian sociopolitical system, based on an all-pervading single factor, somewhat similar to the Marxian theory of *class*, the analogue, in the Indian contexts, being *caste*. Other explanations go in search of adaptive structures, which according to Parsons help societies out of their discordant situations. Adaptive structures in the Indian contexts are supposed to take the form of *caste associations*, which supposedly lessen the traumatic shock of political modernization by smoothly forging the operative relationship between the traditional society and the new political institutions. The single factor explanations treat caste as a cohesive unit even in politics, whereas the explanations based on adaptive structure, because of the inevitable power rivalries and cleavages within caste association, identify increasing political differentiation within the voluntary body. The theories of caste association also treat it as a vital means of political socialization.

I shall argue, however, that both these sets of explanations are simplistic and ambiguous, that they tend to treat caste in politics abstractly rather than examine the actual *involvement* of specific castes in their day-to-day politics in empirically testable situations over an extended period of time. The bulk of castes in India do not have caste associations: what they have instead are caste councils (caste panchayats) which merely address themselves to the primary *social* concerns of the caste—namely, endogamy, ritual, and pollution. A distinction, therefore, has to be made between those castes

which add voluntary structures, such as caste associations, to their existing ascriptive body and those which do not. Here again, one must empirically determine the answer to the crucial question: To what extent is the primary social cohesion of the ascriptive structure available to such newly added voluntary bodies? Finally, one needs to take a hard look at the function of political socialization attributed to caste associations wherever they exist. In my view these associations do not always perform such a function. At times they even seek to set back the political clock. Indeed, the present research suggests that *fragments* of castes, dispersed in different areas, get inducted into the political system and receive far more effective grassroots political socialization than they do through the efforts of caste association.

I shall substantiate these and other assertions with the help of material collected in fieldwork in Gujarat extending for seven years in a rural community and for six years in an urban community. It will be the argument of this paper that in order to explain the interaction between castes and democratic institutions in India we need a theory of cohesion which can identify the extent of caste solidarity when it is engaged in three different kinds of pursuits: (a) preserving itself as an endogamous unit with a distinct social identity of its own within the Hindu social organization; (b) seeking a predetermined social mobility and pressing for a higher social recognition; and (c) searching for better economic opportunities or a share in political power or both. The extent of cohesion that castes can muster in these three different pursuits is not always the same.

The paper, then, is divided into parts that discuss the following main topics: the Srinivas-Harrison model of the subsumption of the new political institutions into the caste system; the Weiner-Rudolphs model of caste association and the consequent increasing political differentiation within castes; my own model of levels of caste cohesion, leadership bifurcation, and decision-making fragmentation; political stimulus and response in a rural community; social cohesion and political difference in an urban community; and the emerging horizontal fragmentation of decision-making units.

The Srinivas-Harrison Model

Professor M. N. Srinivas, who has done a

great deal of perceptive writing on caste as a cultural anthropologist, has expressed the view that the establishment of *Pax Britannica* provided the various castes with the opportunity of overcoming the "severe limits on the horizontal extension of ties,"¹ which were imposed on them by the pre-British fragmentary political kingdoms in India. Added to this were the increased facilities for communication and transport which brought together men of the same castes who were "scattered in far-flung villages."² Long before parliamentary institutions were established in India, the networks of social organization on caste lines had proliferated. Such networks in the postindependence period were used in order to influence votes, which in turn evoked "widespread condemnation of exploitation of caste-links for election purposes"³ from the national leaders. Because the social organization of the castes on horizontal lines had *preceded* the introduction of democratic ideology, they found it easier to subsume the new political institutions.

This thesis of Srinivas was much more effectively articulated by Selig Harrison in his case-study of caste in Andhra politics. Harrison mainly concentrated on the politics of the castes in the Andhra state legislature. Pushing Srinivas' thesis to its logical conclusion, Harrison argued his case from a model of *caste-party* identity, according to which the two peasant castes of Andhra, namely, Kammas and Reddis, first united to dislodge the Brahmins from power, then fell apart as a result of political rivalry, and joined two different political parties.

The Kammas joined the Communist Party and the Reddis joined the Congress Party.⁴ In Harrison's view, therefore, the nature of political cleavage in Andhra was none other than the politically transformed social cleavage between the two peasant castes.

Harrison's work, despite its pessimism and its failure to provide a helpful model, was one of the finest contributions to Indian political sociology, unsurpassed to this date, in my view. So persuasive was his argument that a generation of students of Indian studies did not even get down to questioning his model of caste-party identity against the background of electoral data that he had provided—data that, in fact, did not fully substantiate his thesis.⁵

The Weiner-Rudolphs Model

From the point of view of the impact of the new political ideology on the traditional social organization of the castes, its induction into the political system, and its consequences, Myron Weiner and the Rudolphs presented some of the most sophisticated arguments. Their writings brought into the field of Indian studies a host of structural-functional perspectives and explanations. The Rudolphs actually concluded by means of their functionalist analysis, that the Indians had a genius for adaptation and assimilation when faced with the possibility of conflict and disruption.

Weiner rejected the traditional-modern dichotomy on the grounds that it regarded traditionalism as "temporary and reactionary" and

¹ M. N. Srinivas, *Caste in Modern India and Other Essays* (Bombay: Asia Publishing House, 1962), p. 15, 16.

² Srinivas, p. 16.

³ Srinivas, p. 2.

⁴ Selig Harrison, *India, the Most Dangerous Decades* (Princeton: Princeton University Press, 1960), pp. 211-12.

⁵ In substantiation of his argument, Harrison provided the following two tables (pp. 211-12):

Table III. Number of candidates by major castes in Andhra Delta Districts, in 1946, 1951 and 1955 State Assembly elections

Year	Communist				Congress			
	Kamma	Reddi	Brahman	Other	Kamma	Reddi	Brahman	Other
1946	9	—	1	1	4	3	4	7
1951	22	2	3	15	17	7	2	20
1955	32	4	6	24	28	7	7	25

Table IV. Number of elected legislators by major castes in Andhra Delta Districts in 1946, 1951, and 1955 State Assembly elections

Year	Communist				Congress			
	Kamma	Reddi	Brahman	Other	Kamma	Reddi	Brahman	Other
1946	—	—	—	—	4	3	4	7
1951	14	2	3	6	3	3	—	4
1955	1	2	3	3	24	7	7	25

modernity as "the wave of the future."⁶ What is true, on the other hand, is that "as economic growth has occurred in India and political awareness had increased, the number of community associations has grown." Further, "within political parties ascriptive identifications are playing a prominent role" and the major social institutions, with the exception of family and kinship units, "have associations that articulate their interests." While each of these institutions does not have its own political associations, "... increasingly, such groupings have had their political manifestations."⁷

It is indeed impossible to confirm that each caste in India, and there are more than 3,000 of them,⁸ has an association which articulates its interests. What probably the bulk of them

do is merely to seek what guarantees the survival of their cultural identity within the Hindu social organization. It would also be true to say that the bulk of them do not aspire to play a political role *as castes*. What one comes across, time and time again in fieldwork, is that political parties and electioneering candidates in search of support explore the possibility of using the social cohesion and communication channels of various castes. In that fashion, fragments of certain castes get inducted into the political process, while others do not. What portions of localized fragments of a caste get mobilized by parties and their candidates can be empirically determined. It would be, therefore, misleading to suggest that the whole caste *as a caste* gets politically mobilized and comes out with "political manifestations." In my view, empirical confirmation of the extent of political mobilization of certain castes such as the Kshatriyas, Nadars, and Jats, which did get massively involved in political activity, will dispel the en bloc view of caste politicization.

Weiner made a questionable inference that given the conditions of economic development and growth of political consciousness, castes in India came to acquire economic interests and political ambition of their own *as castes* and to give themselves voluntary associations to be able to pursue them. Nevertheless, he did raise a few pertinent questions: What in fact does happen to the primary social cohesion of a caste when it is confronted with a new set of problems? Does it always take on nonsocial and nontraditional functions as a caste? To what extent is it able to retain its primary social cohesion when it takes on new roles and functions? These and other related questions we shall examine in the next section.

Lloyd and Susanne Rudolph made a fascinating attempt at developing what they called "the sociology of caste associations." They started where Weiner had left off. While Weiner was engaged in identifying the emergence of caste association and its roles and functions, the Rudolphs in *The Modernity of Tradition* took for granted its existence in all parts of India.⁹ The question they asked was: In what way do the norms of a voluntary body such as the caste association exercise influence on the caste itself? They contended that the influence of such norms and of the institutional facilities on the caste was enormous. In their words:

Caste is losing the functions, norms and structure once associated with it and acquiring new ones.

⁹ Lloyd I. Rudolph and Susanne Hoeber Rudolph, *The Modernity of Tradition* (Chicago: The University of Chicago Press, 1967), p. 62.

These two tables indicate that the Kammas as well as the Reddis had contested the elections from both the political parties, Communist and Congress. In the 1946 election, there were more Kammas in the Congress than Reddis and none in the Communist party, although nine had contested the election on its ticket. While in the 1951 election the candidature of the Kammas on the Communist party ticket had more than doubled, their candidature for the Congress had gone up more than four times. The candidature of the Reddis on the Congress ticket had just about doubled itself. While in terms of the 1951 election results the Kammas had won a great number of seats on the Communist ticket, their strength within the Congress party remained at par with their caste rivals, the Reddis. In the 1955 election the Kammas had outnumbered the Reddis, inside the Congress, by more than three to one. Simultaneously they were wiped out as a force in the Assembly as Communists: in 1955 instead of 14 Kamma Communist MLA's there was only one. These figures did not establish Harrison's thesis of caste-party identity. On the contrary they presented us with a picture of party inroads into castes. There were obviously more Kammas to go around for both the parties and subsequently the Kammas as well as the Reddis concentrated on the Congress. See Harrison, p. 219.

⁶ See Myron Weiner, *The Politics of Scarcity: Public Pressure and Political Response in India* (Chicago: University of Chicago Press, 1962), p. 37.

⁷ Weiner, pp. 37-38, my italics.

⁸ No one exactly knows what the total number of castes in India is. There are certain genuine difficulties: the problem of determining the status of break-away fragments of castes, social mobility of certain sections of them, migrations and the emergence of sectional identity, uncontrolled assimilation of the groups of Adivasis into the Hindu social organizations, etc. It is well-nigh impossible to keep track of these continuing processes spread over the entire Indian sub-continent together with the areas of Indian migrations and arrive at a definite figure regarding the total number of castes. The above-mentioned rough total is taken from Robert Hardgrave's *The Nadars of Tamilnad* (Berkeley: University of California Press, 1969). He maintained, "There are in India more than three thousand castes, each culturally distinct endogamous community sharing traditionally a common occupation and a particular position in the localized hierarchy of caste ranking" (p. 2).

It is serving the ritual and occupational goals of traditional society less, the mobility and participation goals of modern society more. In doing so, it [the ascriptive-voluntary body] helps to substitute in the lives of ordinary Indians choice for birth, equality for hierarchy, and opportunity for fate.¹⁰

Once a voluntary body is added onto the caste, they argued, the norms of the former begin to prevail upon those of the latter. The Rudolphs, as a matter of fact, saw the "reincarnation"¹¹ of caste in caste association, and for all practical purposes they treated the association as an agent of change. By means of caste associations, they argued, the teeming millions of India, born in the rigid ascriptive structures of castes, are being homogenized, secularized, and democratized into the new ways of democratic society.¹² What makes such a transformation smoother is "corporatism" all the way: from the corporatism of caste to the modern corporatism of the caste association with little or no intervening "individualist" phase as in the case of western countries.¹³

The Rudolphs provided a sophisticated model of the political mobilization capacity of caste associations. The model helped them to identify three different types of mobilizations: vertical, horizontal, and differential, depending upon the stage of development and internal integration of particular castes.¹⁴ The vertical mobilization takes place through the effort of "traditional notables"; horizontal mobilization by means of integration of caste fragments spread over dispersed areas; and differentiated mobilization through the efforts of political parties, differently appealing to different segments of caste in terms of "ideology, sentiment and interest."¹⁵

Once an ascriptive body such as the caste "reincarnates" itself into a voluntary caste association, internal differences and cleavages within the caste come to acquire a *political* significance. Political parties further help articulate their differences. "It is in this sense," the Rudolphs argued, "that modern politics appears

to be an instrument for both the revival and the supersession of traditional society."¹⁶

The Rudolphs' model rests on the assumption that caste means the same thing to all people belonging to the Hindu social organization. While they start off with the view that different castes possess different degrees of internal cohesion, their picture of *the caste* is largely deduced from a model of the rising peasant castes or groups of castes such as the Kshatriyas, the Nadars, the Jats, etc. These castes have certain common characteristics but it would be misleading to say that *all* other castes share them. Since some castes have caste associations the Rudolphs wrongly assume that all other castes also have them. Not all the castes in India have caste associations in the sense of voluntary organizations looking after economic and political problems. The bulk of them have *caste councils*, and they are integrally connected with the maintenance of the ascriptive character of castes.

Further, the Rudolphs have attributed the functions of homogenization, secularization, and democratization to caste associations.¹⁷ Such an assertion is supported by them by means of broad generalizations about the activities of caste associations. This point being most crucial, specific empirical support in favor of their argument would have been most helpful. On the other hand, as we shall presently see, the fragments of castes attained a fair measure of political socialization and internalization of the norms of the new political institutions *despite* the self-seeking activities of some of the office-bearers of caste associations. Some of the leaders of the caste associations, such as the Kshatriya Sabha, wanted to *deseccularize* whatever electoral differentiation and secularity had been attained through day-to-day political involvement by the Kshatriyas in a rural community in Gujarat. Finally, the Kshatriya Sabha as a caste association did not aim at the homogenization of the group of castes which called themselves *Kshatriyas*, nor did its activities have that result. On the contrary its domination by the princely order resulted in a revolt led by a leadership of humbler origin—a revolt which on the eve of the general election of 1967 finally split the organization into two groups: The Kshatriya Sabha and the Kshatriya Seva Samaj. In conclusion, then, one must recognize that in the process of homogenization, secularization, and democratization evident in rural as

¹⁰ Rudolph and Rudolph, p. 103.

¹¹ Rudolph and Rudolph, p. 62.

¹² Rudolph and Rudolph, pp. 27–28.

¹³ Disputing Marx's thesis that the British rule in India would eventually lead to the atomization even of Indian villages and castes, the Rudolphs argue that "India has shown a strong propensity to *transform* rather than *supersede* traditional corporate structures, to move imperceptibly from traditional to modern corporatism without so marked an intervening individualist phase as the West is said to have experienced" (Rudolph and Rudolph, p. 23, my italics).

¹⁴ Rudolph and Rudolph, pp. 24–26.

¹⁵ Rudolph and Rudolph, pp. 25–26.

¹⁶ Rudolph and Rudolph, p. 97.

¹⁷ Rudolph and Rudolph, p. 34–36.

well as urban India, factors such as increased emphasis on education, accelerated pace of industrialization, better facilities for transport and communication, growth of regional language press, growing awareness of economic and administrative problems, intensely contested elections, and above all, political involvement in local situations, merit due recognition. These processes were *not* set in motion by caste associations, and the influence on political participation exerted on members of castes by existing associations requires specific and rigorous analysis. Broad theoretical formulations in these areas initially help us to identify problem situations: by themselves they do not make our knowledge certain. Certain knowledge requires the rigorous empirical test of hypotheses deduced from our theory.

A Model for Social Cohesion, Leadership, and Decision Making

The initial consumers of all scientific knowledge are the practitioners of the science itself: they check and test its explicit assertions and underlying assumptions. Such an exercise, however, cannot be undertaken if the theoretical formulations are couched in blanket terms. So far as the phenomenon of caste and democratic institutions is concerned, the blanket terms of "caste association,"¹⁸ "caste federation,"¹⁹ and "adaptive structure"²⁰ are used in order to explain the *entire* range of operative relationships between caste as a unit of traditional society and the superimposed modern democratic institutions and procedures. These at best explain one or a few specific aspects of their relationships.

Difficulties arise when caste *in* politics is taken to mean the participation by caste with its social cohesion and communication network intact. The problem is further complicated when voluntary dimensions of "association" and complex cooperative dimensions of "federation" are added on to a strictly ascriptive structure such as caste. Taken at their dictionary meanings "caste" and "association" are as much opposites as are "ascriptive" and "voluntary." The use of the term "caste association" can only indicate an association of some people of the same caste but not of the entire caste. In

making such a distinction what is underlined is that the extent of cohesion and communication network available to a caste for its primary social concerns, which makes it an ascriptive body, cannot be taken for granted when only some members of it band themselves into an "association" and seek to pursue nontraditional goals.

Among others, the primary social concerns of a caste are endogamy, ritual, and pollution. In the realization of those goals a caste enjoys maximum internal cohesion and the most effective use of its communication network. Such traditional concerns are basic to the caste's survival as an ascriptive structure with a distinct cultural identity of its own within the Hindu social organization; when a caste moves from these concerns toward a search for higher social recognition, economic opportunities, and a share in political power, its internal cohesion becomes progressively weaker. Search for social recognition, which is an attempt in the direction of ascriptive regeneration and a more desired position within the social hierarchy, may be blessed, sectionally or wholly, with the same extent of cohesion and a sense of togetherness as exists in a caste when it pursues its primary social concerns. But a caste's economic and political drives necessarily lead it to an internal competition for enhanced status, material gain, and therefore, to dissensions in the pursuit of specific goals and rivalry for power positions in general. It is therefore pointless to talk about the presence of the same extent of cohesion in a caste for its economic and political pursuits as exists for its social pursuits. A caste engaged in the former is not even *caste-like* in its concerns, which traditionally speaking are primarily *social*, and therefore anything other than that becomes a concern not *of* the caste but on its behalf by a group of members. As opposed to the maximum cohesion arising out of the basic social concerns of a caste, its economic and political pursuits involve appeals to new identity, economic gain, power, and status, each of which is a potential divider. Hence, each of the new appeals is invariably prefaced with a call for unity.

In order to grasp the operative relationship between castes and the newly established democratic institutions in India, therefore, we require a model which can address itself to the following: differentiation in the internal cohesion of caste, differentiation in its leadership, and horizontal or factional segmentation of its decision-making units. Let me explain each of these in some detail.

¹⁸ Rudolph and Rudolph, pp. 29-36.

¹⁹ Rajni Kothari and Rushikesh Maru, "Federating for Political Interests: The Kshatriyas of Gujarat" in *Caste in Indian Politics*, ed. Rajni Kothari (New Delhi: Orient Longman Ltd., 1970), pp. 70-101.

²⁰ Harold Gould, "Adaptive Functions of Caste in Contemporary Indian Society," *Asian Survey*, 3 (September, 1963), 427-438.

Primary, Secondary and Tertiary Levels of Caste Cohesion

It is important to note here that the quality and extent of cohesion of a caste changes from one kind of pursuit to another. A caste enjoys explicit or tacit concurrence and support of all its members when it is engaged in those functions which maintain its distinct cultural identity. It maintains this by its firm grip both on endogamy and, to a lesser extent, on matters relating to ritual and to pollution. It treats these three functions as its *raison d'être* and demands the support of the entire group towards their realization. Pursuit of these and the maintenance of norms implicit in them equally become the concern of all, although caste leaders, mostly venerated old men, became formally responsible for overall supervision through the caste councils.

When a caste strives for a higher social recognition within the Hindu social organization, it takes on itself a new nontraditional function. For such a purpose a caste has to demonstrate a higher ritual standard which becomes a responsibility of all its members.²¹ To achieve such a goal the caste elders require the assistance of a new set of leaders to help them formulate a rationale for the drive, carry on public relations work, enter into complex and often tough negotiations with the leaders of other castes and administrators, formulate strategies, and execute tactics.²² While the traditional and nontraditional leaders together launch such a drive, the outcome of their efforts affects all the members of the caste.²³

Such a venture on the part of the caste is able to induce a secondary cohesion because while all the members are affected by such a drive and even have to conform to ritual norms higher than those they were used to, the bulk of its members remain passive participants. Their participation in such a venture obviously is not of the same quality, intensity, and scope as in the case of basic social concerns of the caste—namely, endogamy, ritual, and pollution. As a

matter of fact in the caste's drive toward a higher social recognition, its internal cohesion is stretched to its optimum limits. Such a cohesion then is *secondary*, because it is not a part of a deeply ingrained mode of traditional behavior for the age-old social pursuits but is something that is *rational* put across, with a sharper role distinction between the leaders and the followers, and is directed toward a social destiny which is new and unfamiliar.

Finally, there is a tertiary level of cohesion. When a caste is engaged in a drive to secure better economic opportunities or a share in political power or an appointment of a caste member to public office, not everyone in the caste is either involved in it or directly affected by it. In gossip groups among the rank and file one often hears a sharp distinction drawn between those who would directly benefit by such a drive and the rest. The leaders, likely to benefit by such a drive, exhort their followers to get behind them. Since such drives are thus out-and-out nontraditional and lacking in religious sanction and in social organizational reference, they evidence a *tertiary* level of cohesion of the caste or factions thereof. From start to finish such drives are considered suspect for being in the interest of the few, and therefore politically divisive. Castes which get involved in such drives progressively disintegrate whatever cohesion they are able to muster at the tertiary level without losing their cohesion at the primary social level. Even in a politically divided caste, the basic social function of supervising endogamy, ritual, and pollution remains unaffected.

The following chart summarizes the relationship between caste and level of cohesion.

Bifurcation of Social and Political Leadership in Castes at the Grass-Roots Level

The operation of grass-roots democratic institutions, now nearly a quarter of a century old, has brought about a significant shift towards leadership differentiation within castes. The venerated old leaders, who sat on the caste councils as well as the statutory panchayats, have by and large, withdrawn themselves from the panchayats. Relatively younger men have taken charge of the statutory bodies. The reasons for such a differentiation are manifold: new norms and new mechanisms of decision making introduced by democratic political institutions which remain inconsistent with the traditional ways of arriving at decisions; the decline of respect for age and in some cases familial status; a questioning political culture introduced by democracy; and, above all, the

²¹ M. N. Srinivas has used the term "Sanskritization" in order to indicate a number of means used in order to gain recognition for higher social status. One of these is a conscious attempt to conform and be seen as conforming to higher ritual standards. See in this connection his *Social Change in Modern India* (Berkeley: University of California Press, 1966), p. 6.

²² See in this connection a fascinating paper by William Rowe, "The New Cauhans: A Caste Mobility Movement in North India" in *Social Mobility in the Caste System in India*, ed. James Silverberg (The Hague: Mouton, 1968). See also M. N. Srinivas, *Social Change in Modern India*, pp. 1-45.

²³ See F. G. Bailey, *Caste, Tribe and Nation* (Manchester: Manchester University Press, 1960).

Caste and Three Levels of Cohesion

Extent of Cohesion	Extent of Involvement of its Members	Nature of Cohesion
(1) Maintaining a distinct cultural identity within the Hindu social organization by means of its basic social concerns: endogamy, ritual, and pollution.	All members of caste equally affected and equally involved. Formal role distinction between the caste veterans and the rest.	Traditional goals pursued through deeply ingrained behavior. Feeling of oneness on the part of the whole caste. Primary cohesion brought about by means of ascriptive togetherness.
(2) Seeking greater social recognition; emulation of higher ritual standards.	All members of caste equally affected but not equally involved. Some become effective leaders. Role distinction between leaders and followers clearly recognized.	Nontraditional goals rationally conceived and widely shared. Deliberately brought about group support and unity. Secondary cohesion.
(3) Seeking better economic opportunities and greater share in political power.	Few affected and fewer actively involved. The rest become onlookers, suspicious or hostile. Voiced or unvoiced distinction between those who would benefit and the rest.	Material goals and plea for identification with the cause. Mystique of group importance and benefit. Uncertain group support and potential divisions. Tertiary cohesion.

needed skill in coalition building *across* castes for which the elders had neither any experience nor aptitude.

The net effect of such a leadership differentiation on the general cohesion of the caste is worth noting. Traditional respectability remained with the social leaders and they continued to enjoy castewide support in social matters. *Political* leadership, on the other hand, came to be associated with the seamy side of politics where people with money, leisure, manipulative disposition, devious methods, etc. seemed to be caught in frictions and engaged in power games and the pursuit of personal glory. Consequently, the support received by the political leaders was for the caste *per se* rather than for what they did or stood for. Since politics did not and could not become one of the primary concerns of the caste, faction building and divisiveness could easily develop. What has finally emerged out of the bifurcation of leadership is an insulated social cohesion, on the one hand, and an ever increasing political differentiation, on the other.

Fragmentation of the Decision-Making Units of the Caste

The decisions about the primary social concerns of the caste are made by the caste council and affect all the persons belonging to the caste; it is difficult, however, to find any similarly binding decision-making machinery for the nonsocial matters of the caste. Once we move away from the basic social concerns of the caste what we notice is the fragmentation in

its decision-making processes on horizontal and factional lines. Because of the attenuation of caste cohesion in nonsocial matters, and also because of the separation of its social and political leadership, the effective area of decision making often shrinks from the caste as a whole to its subunits. They are as follows:

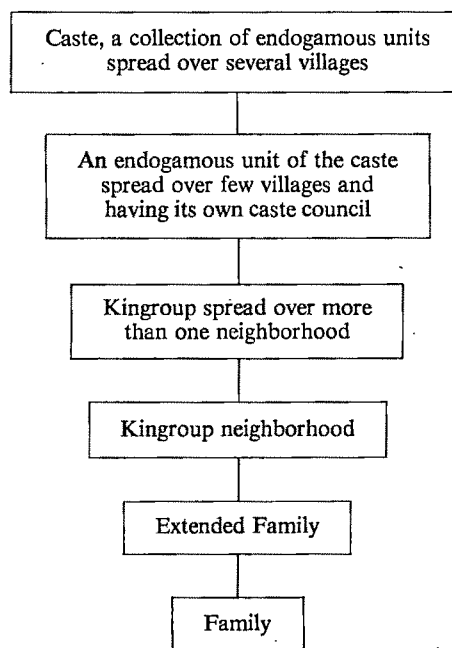


Figure 1. Range of decision-making units

Of these, the most effective unit of social interaction and decision making on a large range of issues, including the *political*, is the kingroup neighborhood and factions within it, or those that cut across such neighborhoods. Within them, where to vote is decided in the same fashion as which school to send one's children to, which doctor or lawyer to consult, where to go shopping, which movie to go to, etc.—collectively and with one's circle of acquaintances. On all such issues group decision making in the neighborhood is a way of life in which decisions on political matters are no exception. In a multikingroup or a multicaste neighborhood, the framework of decision making would correspond to the framework of social interaction.

In such socially interactive decision-making groups, influentials play an important part. Such neighborhood fragments are often linked through the liaison of the influentials by political parties and electioneering candidates. In marshaling a wider support, the caste of a contesting candidate can be an important factor but not the sole factor. Innumerable instances can be found when such neighborhoods under the influence of their own leaders did not give their electoral support to the candidate of their own caste. To be effective in a caste or multicaste neighborhood a candidate must have the influentials on his side. The flow of political influence is directed essentially through them. In that sense the framework of electoral influence is no different from the one described in *The People's Choice*, especially in the theory of the two-step flow of communication, which holds that the media influence "the opinion leaders" first, and they in turn influence "the less active sections of the population."²⁴ The present analysis substitutes the electioneering candidate or his agents for the media, wherever the media are not a significant factor.

What is thought of as the "caste vote" or caste "vote bank,"²⁵ is essentially a successful piecing together of horizontally, geographically, and factionally segmented votes scattered over different neighborhoods of the same caste.

²⁴ Paul Lazarsfeld, Bernard Berelson, and Hazel Gaudet, *The People's Choice* (New York: Columbia University Press, 1968), p. 151.

²⁵ M. N. Srinivas first used the expression "vote-bank" in connection with political influence of patron over client. See his paper on "The Social System of a Mysore Village" in *Village India: Studies in the Little Community*, ed. McKim Marriott (Chicago: The University of Chicago Press, 1955). Later F. G. Bailey used the expression in order to refer to the electoral influence of the caste leader. See his *Politics and Social Change* (Berkeley: University of California Press, 1959).

Even in such instances, neighborhood electorale diversity is as much of a possibility as homogeneity.

En bloc caste vote and political homogeneity, are rapidly disappearing in the contemporary Indian political culture. Instead, electoral diversity within castes is growing in the face of tireless effort on the part of certain "caste associations" and the adroit nomination of caste candidates to attract caste votes.

In the following section I shall illustrate, with the help of my field data, the significance of the bifurcation of social and political leadership for the political development of a rural community, especially for the growth within the community of a dynamic cross-caste political cleavage structure.²⁶ The section after that will illustrate the proliferating political differentiation, despite social cohesion, in an urban community. In both these cases the part played by the only politically active caste association, the Kshatriya Sabha, was marginal.

Political Stimulus and Response in a Rural Community

A continuing field study of a village near Baroda conducted intensively during four periods between 1959 and 1971 did not confirm the view that the castes get inducted into the democratic process by means of "caste associations." Out of the four politically significant castes—namely, Patidars (8 per cent), Kshatriyas (46 per cent), Vasavas (14 per cent), and Venkars (ex-Untouchables, 7 per cent)—only the Kshatriyas were approached by their so-called caste association, the Kshatriya Sabha, during the general elections of 1962, 1967, and 1971. On each occasion the bulk of Kshatriyas did not heed its electoral instructions and, indeed, branded the body as an organization of the upper-crust Rajputs interested in mobilizing political support on behalf of the vested interests and feudal elements. The Kshatriyas of the village were politically socialized into the ways of democratic politics like any other caste in the village: by means of actual political participation.

The village had undergone a major political change—from nonelected to elected political institutions—without the help of caste associations, political parties, or brokers. When the democratic institutions were introduced in the village, various castes, at different levels of political development and internal cohesion, responded differently to the structural changes.

²⁶ For a detailed account of this process, see A. H. Somjee "Political Dynamics of a Gujarat Village," *Asian Survey*, Vol. 12 (July, 1972), 602–608.

Subsequently they learned a great deal about the new political system by means of their operational involvement and also by emulating one another.

The two peasant castes, the Patidars (the landowning and politically dominant) and the Kshatriyas (the numerically powerful) became continually involved in a struggle for power and office in the village. Through their strategies and counterstrategies they stimulated each other to become more politically flexible and skillful in building a multicaste base of political power. The Vasavas and the Venkars also learned, through social and political emulation, how to play effectively their role as auxiliary social groups. In the last analysis the twin teachers of democracy for these four castes were the gradual internalization of the norms of the new ideology, on the one hand, and actual political involvement, on the other.²⁷

At the root of the political change in the village was the bifurcation of its social and political leadership, a necessary and sufficient condition of increasing political differentiation in otherwise socially cohesive castes. For after the major structural change in the mid-'fifties, which required the local council to be constituted on the basis of universal adult suffrage, the rough-and-tumble of democratic politics pushed into the background the old and the venerated leaders of the Patidars as well as the Kshatriyas. Until then these men had retained in their hands the dual role of social and political leadership. Since 1904, when the statutory local council was established in the village, the Patidar elders, belonging to the families with high status in the caste, had sat on the statutory body as well as on their caste council. The same was true of the Kshatriya elders whenever they got themselves nominated to the statutory council along with the Patidars.

One of the earliest repercussions among the Patidars of the introduction of the universal adult suffrage was an excessive questioning and criticism by their own younger men of their ability and role as members of the council. A similar barrage of criticism was directed towards the elder Kshatriya membership by its own younger men. For a long time the Kshatriya elders had accepted a position that was subordinate to the Patidars. That was clearly unacceptable to the politically ambitious, articulate, and restless younger Kshatriyas.

²⁷ For a detailed empirical support for the theory of stimulus-response within the framework of political emulation see A. H. Somjee, *Democracy and Political Change in Village India: A Case Study* (New Delhi: Orient Longman, 1972).

The Patidar elders faded away from the political scene without incident. The Kshatriya elders did not. The impatience of their younger men brought into play kinship differences among their two major groups. It therefore took a little longer for the Kshatriya elders to hand over power to their younger men.

The emulative framework of village politics also helped the Venkars to bifurcate their leadership along the lines of the Patidars and the Kshatriyas. Until 1971, the wave of bifurcation had not affected the Vasavas: this was because their involvement in village politics, till recently, was only marginal.

The Patidars as well as Kshatriya elders, however, relinquished merely the political leadership of their respective groups; they continued to be in command of the social leadership of their castes. Political leadership of the two castes was taken on by the younger men initially from the families which also had produced social leadership. Thus for a short period the familial status had continued to be the basis of the differentiated social and political leadership. It took another wave of change in these two castes to undermine the importance of certain families. And this happened more than a decade after the introduction of democracy, when familial status in the political realm also was challenged by still younger men in these two contending castes. The decline of familial status was marked by a corresponding rise in the importance of political skill in building intercaste, interclan, and at times even more diversified bases of political power. Henceforth the politically ambitious neither confined themselves to their own caste nor took for granted its support. Political support became for them a matter of skillful accommodation, manipulation, and contrivance. So far as local politics went, such skills had to be put to work within a highly structured situation of castes and regard for personalities. National electoral politics on the other hand, provided the politically ambitious with much greater freedom and scope for cross-caste political cleavages. A part of such cross-cutting political cleavages in national politics inevitably affected the style and character of local politics.²⁸ Differentiated political leadership thus became an agent of political diversification within the castes in local as well as national politics.

In their contest for seats in the village council the Patidars opposed Patidars with the sup-

²⁸ See Chapter VII, "Village Politics and National Politics: The General Election of 1967" in Somjee, *Democracy and Political Change in Village India*.

port from the Kshatriyas and vice versa. The support of the rest of the castes in the village for these two contending groups became increasingly differentiated. But much greater diversity of support than in the elections of the village council was evident inside the council for the election of its chairmen and other positions. The Patidars and the Kshatriyas alternately got themselves elected to the chairmanship of the council by manipulating the cleavages in each other's ranks.

The process of diversification of political support across castes was accentuated by the general elections of 1962, 1967, and 1971. Each time, because of the presentation of issues and the political pulls during campaigns, the cleavage structure of village politics changed. Instead of conforming to the existing political divisions in the village, the new cleavage structure preceding the general elections cut across such divisions.

Thus for example, in the general election of 1962, the bulk of the villagers, particularly the Patidars and Kshatriyas agreed to give their partial support to the Congress candidate for the parliamentary seat (the Maharajah of Baroda): but they refused to support its assembly candidate even though the village political actives, Patidars as well as Kshatriyas, were the members of the Congress party. Instead they gave their support to a "neighbor," an independent, who actually belonged to a neighboring village and in his capacity as the chairman of a cotton cooperative society had helped the villagers in a number of ways. On their part, the Kshatriyas had also rejected an electoral brief from the Kshatriya Sabha asking them to support a particular candidate.

The cleavage structure which emerged in the village at the time of the general election of 1962 had some interesting characteristics. The Patidars and the Kshatriyas, who by and large were at loggerheads in the local politics, jointly decided to split their votes between the Congress parliamentary candidate and the "neighbor" who stood for the assembly seat as an independent. As opposed to them, the satellite groups, such as the Vasavas and Venkars, and

the rest of the village supported both the candidates of the Congress.

During the general election of 1967, the voting pattern of the village became further diversified, this time, within the caste. The Congress had nominated the Maharajah of Baroda for the assembly seat and the Mayor of Baroda for parliament. The mayor was extremely unpopular with the villagers because he had been instrumental through state legislation in acquiring a large piece of village land which he proposed to use for industrial purposes. Among those who lost their land were Patidars, Kshatriyas, Vasavas, and Venkars. On the other side, the Swatantra party and the Jan Sangh had jointly sponsored an industrialist and an industrial executive for the parliamentary and assembly seats, respectively. Together they provided a number of jobs for the factory-worker villagers. Caught between the cross-pressure of appeal of the Congress program and the security of employment in factories, the bulk of the villagers split their votes into seven different combinations, one of which was invalid.

Out of an electoral roll of nearly twelve hundred voters, I interviewed 120, one out of every ten. Each was interviewed twice, once before and once after the election. The number of voters interviewed by caste, was as follows: Brahmins (6), Baniyas and Kacchia Patels (4), Patidars (9), Kshatriyas—Chauhans, Chavdas, Solankis, Padhiars, Rajputs, and Rathods (57), Suthars, Kumbhars, and Malis (8), Vasavas and Bharwads (scheduled tribes) (20), Venkars and Harijans—scheduled castes—(8), Non-Gujaratis (5), Muslims and Christians (3). Tables 1 and 2 show the village voting pattern overall and also by caste.

These tables clearly suggest that none of the four major castes of the village voted as caste groups. Although each gave the bulk of its votes to the Congress assembly candidate, a substantial number of votes were also spread widely over other political parties.

The Patidars were divided for several reasons: their political leadership in the village was in the hands of pro-Congress men; a Patidar had stood on Swatantra ticket for the par-

Table 1. The General Election of 1967: General Pattern of Vote in the Village

Congress	Swatantra and Jan Sahgh	Split Vote (Two non aligned Parties)	One Vote	Did Not Vote	Refused to Answer	Do Not Know	Total
35 (-7 invalid) 29%	14 12%	40 33%	4 3%	14 12%	9 8%	4 3%	120 100%

Table 2. The General Election of 1967: Pattern of Vote
in the Village among Four Major Castes

Caste	Congress		Jan Sangh		Swatantra	Janta Parishad	Did Not Vote	Refused To Answer	Do Not Know	One Vote
	MLA	MP	MLA	MP		MLA				
Patidars	6	4	2	4				1		
Kshatriyas	37	19	2	17			6	6	3	3
Venkars	6	1		1			1		1	
Vasavas	13	6	2	10		1	1	2		1

liamentary constituency; and finally, the Patidar youth wanted to try out the right-wing parties for a change. The Kshatriya, despite their caste association's (The Kshatriya Sabha's) electoral brief urging that they support Swatantra and Jan Sangh candidates, gave the bulk of their assembly votes and more than half of their parliamentary votes to the Congress. Their votes for the Swatantra candidate, who had given employment to some of the Kshatriyas in his tractor manufacturing firm, clearly indicated an element of economic pressure. Such a pressure was also felt by some Vasavas and Venkars. Only the Brahmins, normally neutral in village politics, strongly came out in support of rightist parties.

During the general election of 1971, the Congress approached the electorate with a division in its own ranks. The Ruling Congress headed by Mrs. Indira Gandhi was popularly known as the *Indicate* and its opponents, the Organization Congress was known as the *Syndicate*. The Organization Congress had sponsored the Maharajah of Baroda as its parliamentary candidate, whereas the Ruling Congress had supported a trade-union worker from the Praja Socialist Party (P.S.P.) for the same seat. By 1971 there was a considerable increase in the factory employed population of the village—nearly one-fifth of its total number. While the working class supported the P.S.P. candidate because he was a champion of their interests, the agriculturists were influenced by the charismatic appeal and the pro-underdog image of Mrs. Gandhi. It was estimated that nearly eighty per cent of the village electorate, consisting of all castes, favored the P.S.P. candidate.

Social Cohesion and Political Difference in an Urban Community

My continuing intensive fieldwork in an urban community (Anand²⁹ in Gujarat) has re-

²⁹ The study of Anand also includes the adjoining town of Vidyanagar which is the seat of a university. For the purposes of my research the twin towns were considered as one—Anand.

vealed an increasing political differentiation in castes whose primary social cohesion remained, more or less, unchanged. Anand is a rapidly industrializing middle-sized (population approximately sixty thousand) town in western India. In 1966 a stratified sample of 500 voters was drawn from 34 social groups of castes, religious minorities, and linguistic communities. During 1966–67, the voters in the sample were interviewed three times: twice before the general election of 1967, and once after. The sample of voters³⁰ was interviewed twice again in 1971: before and after the general election (parliamentary). Finally, during the general election (assembly) of 1972, the voters in the sample were interviewed twice again: before and after the election. Almost every person in the sample was thus interviewed seven times over a period of six years. This extended fieldwork in Anand was undertaken with a view to understanding the electoral dynamics of an urban community in terms of time-dimension.³¹

The resulting data confirm the view that the primarily social cohesion of a caste is not easily transferrable to its nonsocial pursuits. Political homogeneity was never attained by most castes including the Kshatriyas even though Anand is one of the more vigorous centers of the caste association of the Kshatriyas (the Kshatriya Sabha) and the home of one of its splinter groups—the Kshatriya Seva Samaj. Whenever political homogeneity was achieved by a caste, my field data indicate, it was not because of its "caste association," assuming there was one. Indeed, by and large, the cohesiveness of the caste in nonsocial matters did not go beyond primary groups such as the extended family or kinship neighborhood. This contention is sub-

³⁰ In 1971 and 1972 interviews, 27 and 28 persons, respectively, had to be substituted as some of the people in the sample had either moved away, become untraceable, or had died. Wherever possible, son, daughter, brother, sister, or neighbor was substituted for the missing or dead respondent, always from the same caste and sex.

³¹ See in this connection A. H. Somjee, *Anand: A Study in Electoral Dynamics*, forthcoming (tentative title).

stantiated in findings on the six major social and religious groups of Anand which together comprise nearly three-fourths of Anand's population. They are the Brahmins (7 per cent), Banias (5 per cent), Patidars (22 per cent), Kshatriyas (25 per cent), Christians (4 per cent), Muslims (8 per cent). The two religious minorities, Christians and Muslims, are included to permit comparison of changes in political homogeneity among them as well.

In order to grasp the political background of the respondents, an attempt was made to ascertain the tradition of their family vote, a fairly reliable index of a voter's political background. Their response indicated the following:

give him more than 17 per cent of their votes. The Kshatriyas, like the other castes of Anand, overwhelmingly supported the Congress in India's first general election held in 1952.

The general election of 1957 especially in the Kaira District, where Anand is located, took place against the background of two vital issues: a regional movement headed by the Mahagujarat Janata Parishad which demanded that a separate state of Gujarat be carved out of the composite state of Bombay; and the Land Tenancy Act, passed by the Congress government, which proposed handing over land ownership to the tenants. These were thus the

Table 3. The General Election of 1952: Caste and Vote in Anand

Caste	Extent of Electoral Participation ²	Vote for Congress	Vote for Lok Paksh
Brahmins	A little more than half	90%	10%
Banias	Nearly two-thirds	80%	20%
Patidars	A little more than half	83%	17%
Kshatriyas	More than half	98%	2%

One of the interesting features of this observation was that proportionately more Kshatriyas were politically mobilized than Patidars,

issues of regional sentiment and economic interest. The castewise performance of the parties was as follows:

Table 4. The General Election of 1957: Caste and Vote in Anand

Caste	Extent of Electoral Participation	Congress	Mahagujarat Janta Parishad Movement
Brahmins	A little more than half	82%	18%
Banias	A little more than two-thirds	100%	0%
Patidars	More than two-thirds	61%	39%
Kshatriyas	More than two-thirds	100%	0%

even though the rate of literacy and the need to protect private economic interests was higher among the Patidars. The castewise distribution of votes between the Congress and a regional opposition party (Lok Paksh), especially among the Patidars, was most striking, because of the influence exerted by a political party as opposed to a towering leader of the caste. The Lok Paksh had nominated Bhailalbhai Patel, possibly the greatest leader of the Patidars since Sardar Patel. In the constituency as a whole he secured a high percentage of the popular vote, but the Patidars of Anand did not

The Banias, who had prospered with the growth of trade and commerce in Anand felt indebted to the Congress and therefore fully switched their support to it. With the Patidars the Land Tenancy Act was an extremely unpopular measure. Some of the medium- and large-scale landholders among them either had lost a part of their land or had felt threatened. At the other extreme, the Congress land tenure policy was very popular with the Kshatriyas, who were either petty landholders, tenants, or landless laborers. Being essentially the peasant castes of this area, the Patidars as well as the Kshatriyas were deeply affected by the changes in land tenure policy despite the fact that Anand's agriculturist population had registered

² This indicates the participation of families of the respondents in previous elections.

a downward trend as a result of the growth of commerce and industry there. During the general election of 1957, therefore, the Patidar support for the Congress fell below two-thirds, whereas that of the Kshatriyas rose to one hundred per cent, the highest ever recorded.

The 1962 general election was marked in Anand by a bipartisan contest of the two regionally well-balanced parties. The political opponents of the Congress had put together a new political party with a distinct economic policy, political ideology, party organization, and leadership. This was the Swatantra party. The seat of Gujarat Swatantra was and has been Vidyanagar, which is on the outskirts of Anand. Although the party had come into existence a short time before the general election of 1962, serious differences among the regional groups within the Congress and a number of economic issues, including the Land Tenancy Act, etc., helped the new party in mobilizing political support. In Anand the distribution of party vote castewise was as follows:

The most interesting feature of the Patidar vote for the Swatantra party was in the fact that it was given to the party as such rather than to the candidate. For the Swatantra party had nominated a Kshatriya candidate, while the Congress had chosen a Patidar candidate. Yet for the Patidars of Anand, the appeal of the candidate's caste proved to be weaker than that of party ideology.

The same was true of the Kshatriya vote. Even though the Kshatriya Sabha had electorally aligned itself with the Swatantra party, and had mobilized its entire machinery and resources in the campaign, and had also backed a leading Kshatriya candidate, nevertheless the Kshatriyas of Anand overwhelmingly voted for the Congress and its Patidar candidate. The Congress policy of giving protection to the poorer section of the agriculturists and its general pro-underdog image had a special attraction for them.

The general election of 1962 in Anand, from

Table 5. The General Election of 1962: Caste and Vote in Anand

Caste	Extent of Electoral Participation	Congress	Swatantra
Brahmins	More than three-fourths	64%	36%
Banias	More than three-fourths	64%	36%
Patidars	About three-fourths	37%	63%
Kshatriyas	More than three-fourths	85%	15%

The Brahmin, and more so the Bania, support for the Congress declined considerably in 1962. The economic policies of the Swatantra party proved to be quite attractive to both these castes. Some Brahmins and a great many Banias were in the grain trade and were petty shopkeepers. To them the increasing restrictions placed on economic activity by the Congress's ideology and its policies of socialism were most irksome. The Swatantra ideology of unrestricted trade and commerce, on the other hand, was particularly attractive to them.

The caste which was most strongly drawn to the Swatantra party ideology was the Patidars. At last they had found a party which immensely suited their innovative, pragmatic, and result-oriented approach to economic life. Swatantra's economic philosophy had everything that such an industrious and economically successful caste was looking for. Moreover, Swatantra leadership was locally based (i.e., in Vidyanagar), which offered the additional possibility of influencing its organization and decision making.

the point of view of caste and democratic politics, was therefore most significant: It brought about a polarization of vote on ideological and programmatic lines that cut across social cleavages of castes. The Congress as well as the Swatantra party had political support among all the four major castes, and what is more, neither candidate succeeded in attracting all the votes of the caste he represented. To top it all, even the "caste association" such as the Kshatriya Sabha did not succeed in shepherding the Kshatriya voters in the direction of its candidate. Along with a differentiated vote in their ranks, the Kshatriyas were engaged in questioning the leadership and political direction given by their "caste association."

In the election of 1962, two trends clearly emerged in the politics of Anand. First, the Kshatriyas had firmly identified themselves with the Congress ideology. Henceforth they would support it in large numbers, regardless of the caste of its candidate, so long as it maintained a pro-underdog stance and policies to al-

leviate the suffering of the poor. Second, the Patidar voters consistently sought out a party which could act as an effective opposition and alternative to the Congress. Like the Kshatriyas, they would continue to take the ideology and program of the party more seriously than the caste of its nominee.

The general election of 1962 was also a watershed in the development of secularism in the politics of Anand. It saw the diversification of political support for both the parties not only in terms of castes but also with reference to the two religious minorities, Christians and Muslims. Both these minorities had given their en bloc support to the Congress in the general elections of 1952 and 1957. Their anxieties about the secular future of the Indian Republic had evoked, in the past, an undifferentiated support for the Congress, which is more explicitly committed to secularism than most other parties. A decade after the birth of the Indian Republic, and the two general elections, the Christians first and then the Muslims acquired sufficient confidence in India's secular future to be able to diversify their democratic choice. Not being involved in religious problems which had led to the partition of India in 1947, the Christians were the first to gain enough confidence to exercise the choice clearly. The Muslims were not far behind. The distribution of party votes in terms of minorities was as follows:

Table 8. The General Election of 1967: Religious Minorities and Vote in Anand

Religious Minorities	Congress	Swatantra
Christians	74%	26%
Muslims	79%	21%

For the election of 1967, the Congress had nominated two Kshatriya leaders for the parliamentary and assembly seats, and the Swatantra party had chosen a well-known, I.C.S.-retired Patidar leader for both the seats. There was a fierce struggle for the Kshatriya vote. While the Kshatriya Sabha had come out in favor of the Swatantra party, the local Congress leaders had helped toward the establishment of a rival Kshatriya body called the Kshatriya Seva Samaj.

The period between 1962 and 1967 in India was marked by a phenomenal rise in the cost of living, increased complaints against bureaucratic slowness and excessive restrictions, and the visible new prosperity largely confined to men in industry, commerce, bureaucracy and politics. Ironically enough, the people who had prospered most during the Congress regime, Brahmins, Banias and Patidars, were looking for an alternative government. While the Kshatriyas had experienced only a trickle of pros-

Table 6. The General Election of 1962: Religious Minorities and Vote in Anand

Religious Minorities	Extent of Electoral Participation	Congress	Swatantra
Christians	More than three-fourths	75%	25%
Muslims	Nearly three-fourths	89%	11%

The general election of 1967 witnessed continued diversification in the political base of the Congress and the Swatantra party among the castes and the religious minorities of Anand. Their share of popular vote in terms of caste and religious minorities was as follows:

Table 7. The General Election of 1967: Caste and Vote in Anand

Caste	Congress	Swatantra
Brahmins	36%	64%
Banias	21%	79%
Patidars	23%	77%
Kshatriyas	84%	16%

perity, the bulk of them were not looking for a change in party rule. The Congress had lost supporters all around, including some Kshatriyas. Its greatest losses were among the Banias, then among Brahmins, and finally among the Patidars. Fall in its popularity among the Kshatriyas was very slight, nevertheless it was there. But what was significant in their case was that a well-known Kshatriya leader, namely, Narendra Singh Mahida, who had gone over to the Swatantra³³ in the 1962 election and had subse-

³³ For the details of this see Rajni Kothari and Rushikesh Maru, "Caste and Secularism in India: Case Study of a Caste Federation," *The Journal of Asian Studies*, 25 (November, 1965), 33-50.

quently returned to the Congress fold before the 1967 election, could not, along with a Kshatriya running mate for the assembly seat, pull more votes for the Congress. This had occurred in spite of the fact that the local Congress leaders were instrumental in establishing a rival Kshatriya body, namely, the Kshatriya Seva Samaj.

Inaugurated on the eve of the general election of 1967, the Kshatriya Seva Samaj was designed by and large to bring about a cleavage among the Kshatriyas along *class* lines. As opposed to the Kshatriya Sabha, whose leadership remained with the princes and feudal elements, the Kshatriya Seva Samaj brought into prominence small businessmen, school teachers, and skilled workers in the ranks of the Kshatriyas.

The Samaj under the leadership of men of humbler origin proved an effective check in the area on whatever influence the Kshatriya Sabha could wield. The Kshatriya vote for the Congress, despite the work done by the Samaj and despite the caste of the two Congress-nominated candidates, had actually dwindled a little. What is important is not so much the continuing decline of Congress's popularity among the Kshatriyas but its failure to get impressive political results even when it employed caste-oriented means.

The emergence of a non-Congress vote among the Christians and the Muslims was also most significant. The decline in Congress popularity among the Muslims in the 1967 election was considerable. While the diversity in Christian and Muslim vote indicated a growing confidence in the secular character of political parties, there were certain additional local reasons for the disaffection with the Congress party: A group of the Christians in Anand had had a raw deal at the hands of the local Congress bosses in connection with the closing of the hospital they ran, thus prompting some of the Christians to try an alternative to the Congress. As far as the Muslims were concerned, the Voharas among them, a subgroup, engaged in commerce including the grain trade, had developed professional concern about the restrictive Congress policy in matters of trade in general. The economic interests of the Voharas had induced them to identify themselves with the others in the *profession*. Since the bulk of the grain traders favoured Swatantra ideology, the Voharas went along. In summary, the Christians as well as the Muslims had now started making use of political means in order to express their disapproval of the Congress policies rather than remaining solely concerned with the future of secular state in India.

In the general election (parliamentary) of 1971 the trend toward declining political homogeneity among castes and religious communities of Anand continued. As in all other previous general elections in Anand, the 1967 contest was almost completely a battle between two political groups—this time between the two factions of the old Congress, the Ruling Congress headed by Mrs. Gandhi, and the Organization Congress led in Gujarat by Morarji Desai.

Gujarat was the home state of Morarji Desai, the principal contender for power against Mrs. Gandhi, and Desai had his own men deeply entrenched in the entire infrastructure of the districts, and the bulk of the MLA's were loyal to him. Thus the well-known Kshatriya leader, Narendra Singh Mahida, the nominee of the Ruling Congress, who was a sitting M.P. and a minister for the state in the Union Defence Ministry, faced a formidable task. He began shakily and got no help from experienced political workers. The Organization Congress had pitted against him Pravinsingh Solanki, a sitting M.P. from another constituency and the son of a Kshatriya Sabha leader, Natvarsingh Solanki. To help his son in the campaign, the latter started using the machinery of the Kshatriya Sabha, but he was subsequently discouraged by the Organization Congress bosses because they feared that the Ruling Congress might bring into the fray the rival body, Kshatriya Seva Samaj. The bosses also made sure that the Samaj leadership, mostly employed in the district infrastructural bodies, was kept in good humor. In Anand, therefore, the two caste bodies were kept out of the electoral contest.

As for the other castes of Anand, they were initially inclined to view the election of 1971 as a contest between the two Kshatriyas. But their attitude subsequently changed when the regional newspapers on both the sides started playing up the theme of political stability, economic development, employment, prices, fair distribution of income, etc. On top of that came the campaign visit of Mrs. Gandhi, for which more than a hundred thousand persons turned out from within the radius of about forty miles. Her meeting was disrupted by the agents of the Organization Congress, a disruption that adversely affected the following of the Organization Congress in Anand. More and more criticism was voiced against its tactics and its electoral alliance with such three ideologically diverse parties as the Swatantra, the Jan Sangh, and the S.S.P. It would therefore not be an exaggeration to say that the election of 1971 in Anand took place in an atmosphere of a

near breakdown of party machinery on both sides. The Ruling Congress had utilized the charismatic appeal and the pro-underdog image of Mrs. Gandhi. The Organization Congress, on the other hand, had relied heavily on personal influence and pressure, terror tactics, etc., to get a favorable response. In terms of caste and religious communities the performance of the two sides was as follows:

Table 9. The General Election (Parliamentary) of 1971: Caste and Vote in Anand

Caste	Ruling Congress	Organization Congress
Brahmins	23%	77%
Banias	29%	71%
Patidars	10%	90%
Kshatriyas	76%	24%

Table 10. The General Election (Parliamentary) of 1971: Religious Minorities and Vote in Anand

Religious Minorities	Ruling Congress	Organization Congress
Christians	33%	67%
Muslims	63%	37%

The support for the Organization Congress among the Brahmins, Banias, Patidars, and Christians was most impressive. Its support among the Kshatriyas was greater than that received by the Swatantra party in the two previous general elections. Its support among the Muslim voters, too, was impressive, especially because one of its electoral allies was the extreme right-wing party (the Jan Sangh), and also because the election had taken place in an atmosphere of communal tension in the cities of Gujarat. The most impressive support for the Organization Congress had come from the Patidars, who had identified themselves with it enthusiastically.

The Organization Congress, as a matter of fact, was many things to many people. To some it was *the* Congress with its stalwarts like Morarji Desai seen as unjustly treated by Mrs. Gandhi. Others felt Mrs. Gandhi had only two types of supporters left: Muslim communalists and the Communists. They therefore preferred a right-wing combination on the Hindu side which they thought the Organization Congress represented. Finally, Mrs. Gandhi was seen as an enemy of the most progressive and well-administered state, namely, Gujarat, where she

had tried to topple the ministry of a protégé of Morarji Desai.

The Ruling Congress, on the other hand, had clearly introduced into Indian electoral politics a specific economic appeal which was comprehensible to the average voter. As Mrs. Gandhi herself repeated in her various campaign speeches, her policy was not to take away people's prosperity but to generate new wealth which could reach the poor. Earlier her policy of the nationalization of banks, which had proved a great blessing to the small borrower, had established her credentials in the eyes of the lower income group. To the needy and the poor, therefore, the Ruling Congress was the party of the underdog. To the middle class and to businessmen her party held out the hope of political stability, which a motley assemblage of parties like the Organization Congress and its allies, as experience in other states showed, did not.

While the ratio of popular vote between the Ruling Congress and the Organization Congress, among our six groups was 3:4, it was not so wide among the lower strata of society, which had narrowed the margin of the Organization Congress.

Finally, we examine the general election (Assembly) of 1972. That election could not have been more opportune for the Ruling Congress, whose landslide victory in the previous election not only provided political stability to India but also restored confidence in its democratic future. The architect of that victory was Mrs. Gandhi who in the eyes of the average voter rose to great heights at the time of crisis. Her rise to prominence meant that the Indian democracy no longer needed extraordinary men like Nehru to run it. It meant that even a person like Mrs. Gandhi, whose capacity for leadership or political skill had remained untested till recently could rise to the occasion when faced with a challenge. What she gave the average voter was a touch of self-assurance. Moreover, her timing and handling of the Bangladesh crisis electrified the country. On the eve of the 1972 election, therefore, there was no doubt about which party would win in the bulk of the Indian states which had gone to the polls. Most of the guesses around election time were merely about how large the victory of Mrs. Gandhi's party would be.

Unlike the elections in the past, the 1972 election in Anand saw three political parties in the fray: The Ruling Congress; the Organization Congress; and the Jan Sangh. The two Congresses had pitted against each other two young Kshatriyas, both of humble birth, almost

of equal age, both from the same village, and even distantly related. While the nominee of the Ruling Congress was the more educated, that of the Organization Congress had the advantage of having been a member of the dissolved assembly. The Jan Sangh, on the other hand, had nominated a young Patidar. The response to the three political parties in the election of 1972 was as follows:

Table 11. The General Election (Assembly) of 1972: Caste and Vote in Anand

Caste	Ruling Congress	Organization Congress	Jan Sangh
Brahmins	54%	38%	8%
Banias	45%	30%	25%
Patidars	38%	37%	25%
Kshatriyas	74%	24%	2%

Table 12. The General Election (Assembly) of 1972: Religious Minorities and Vote in Anand

Religious Minorities	Ruling Congress	Organization Congress	Jan Sangh
Christians	67%	28%	6%
Muslims	67%	30%	4%

The Ruling Congress had greatly improved its position and its support from all groups except the Kshatriyas. Among the Kshatriyas the declining trend in support for the Ruling Congress had continued, and such Kshatriya vote as it got was largely for the party rather than the candidate. The Organization Congress nominee of 1972 had also been the nominee of the undivided Congress in 1967. The same Kshatriya electorate gave 84 per cent of its votes to him in 1967, whereas in 1972 it reduced its support to 24 per cent because he had identified himself with the Organization Congress and not with the Ruling Congress. The Patidars, instead of supporting the Patidar candidate nominated by the Jan Sangh, gave three-fourths of their votes to the two Kshatriya candidates nominated by the two Congresses. All the six major groups of Anand diversified their votes. Even the two religious minorities gave some support to a traditionalist party such as the Jan Sangh. Through such electoral diversity the castes and religious minorities had asserted their right to vote wherever they pleased regardless of the attempts made by political parties to exploit their caste sentiment and the fear of losing the freedom of conscience.

Before concluding this section, it is instructive to note the extent of popular support of the Kshatriyas enjoyed by the Congress from 1952 to 1972, during the six general elections (see Figure 2). Here I am making the assumption that the Ruling Congress of 1971 and 1972 was the Congress.

From 1962 to 1972, the period when a diversification occurred in the Kshatriya vote, despite the presence or absence of a Kshatriya candidate or candidates, or electoral brief, for or against, from the Kshatriya Sabha or Kshatriya Seva Samaj, the bulk of the Kshatriyas continued to support the Congress. Over the years, however, their support for the Congress steadily declined. Neither the presence of a caste candidate nor a brief from a "caste association" could check it. The election of 1971 had presented them with a choice between the two ex-princely Kshatriyas and that of 1972 between two Kshatriyas of humble birth. Such a conflict merely hastened the incipient process of vote for the party rather than the social background of the candidate.

Fragmentation of Decision-Making Units

Political parties in search of mass support covetously look to the existence of primary social cohesion within the endogamous units of caste and hope to tap it or turn it into a "vote bank." In actual practice, however, such a complex enterprise turns out to be replete with obstacles. Since the primary cohesion of the caste exists for its *social* concerns only, political parties are forced to start at the base, with the members of the caste and caste-neighborhood influentials, and work their way horizontally so as to encompass more and more supporters.

Each caste consists of a number of endogamous units; each endogamous unit is spread over a number of villages and towns; within each village or town, fragments of an endogamous unit are dispersed over a number of caste and multicasite neighborhoods; within each such neighborhood there are a number of groups which structure the social interaction and political decision making of their members. Each of these groups, further, has its own influentials and factions. In order to mobilize mass support of a caste, a political party is required to win over as many influentials to its side as possible. Such a task is usually assigned to a caste political active. He contacts the influentials. However, the factional structure of groups and rivalry among the influentials, campaigning by political parties, perception of issues, interpretation of personal interest, attraction to certain personalities, influence of re-

PERCENTAGE
OF KSHATRIYA
VOTE
FOR THE
CONGRESS

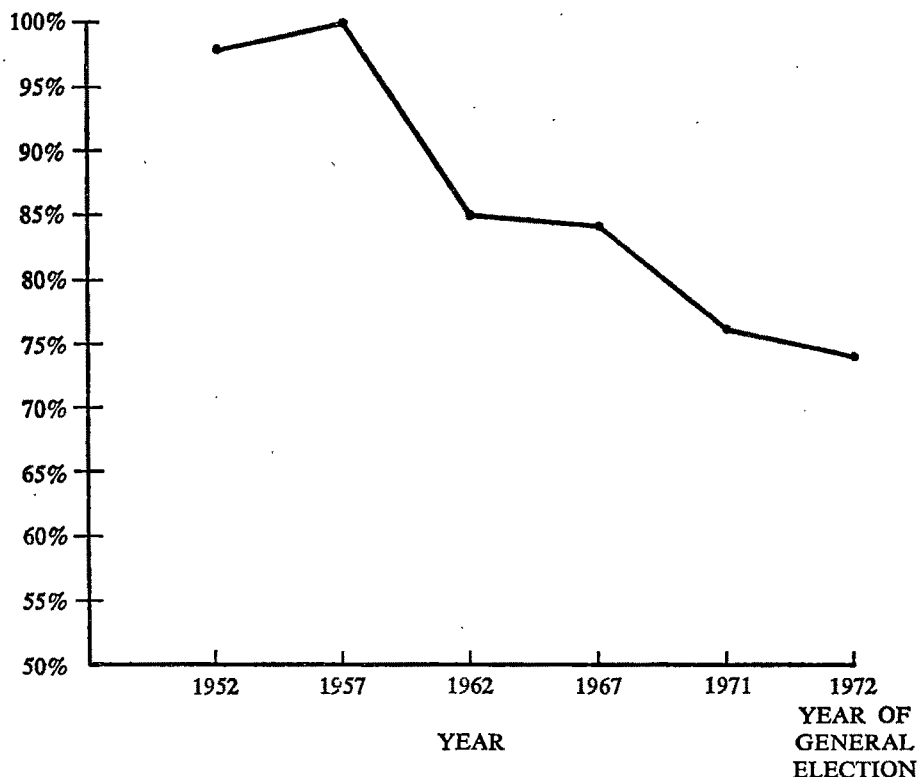


Figure 2. Percentage of Kshatriya vote for Congress in six general elections

gional newspapers, exposure to different political views in the place of one's work, finally, learning from a cumulative electoral experience itself, all these together, do not make the task of a caste political active very simple. Despite the work done by caste political actives and the appeal of the caste of a contesting candidate, and also despite the work done in certain instances by "caste associations" such as the Kshatriya Sabha, the electorate in the rural as well as the urban community, as we have seen in these pages, registered a growing political diversity.

One of the greatest imponderables in any attempt to analyze the relationship between caste and politics in India is the intractable problem of the communication network of the caste. The fact that a caste is horizontally integrated and continues to hold itself together for the pursuit of its primary social concerns, despite rapid change in all other aspects of Indian life since independence, indicates an effective network of communication integrating its mem-

bers toward common social goals. In the existing studies on caste not much is written on the nature of this network or about its mode of operation. To what extent and with what success such a network can be used for electoral purposes remain to be empirically determined.

In the final analysis any attempt to grasp the relationship between caste and politics in contemporary India takes us back to the problem identified by Professor M. N. Srinivas, that during the *Pax Britannica* individual castes achieved horizontal integration at the social level. So far as their politics are concerned, in my view, a reverse process is very much in evidence. Political cohesion of the castes, wherever present, is in the process of shrinking from the wider horizontal integrations to smaller and more diversified residential segments. In conclusion, I am now persuaded that what the political parties do at best is to build a patchwork of segmented political support on multicaste lines, rather than rely on a caste, however numerous it may be.

The Attribution of Variance in Electoral Returns: An Alternative Measurement Technique

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Beyond determining which individuals will hold public office, elections provide the citizens of modern democracies with their most public opportunity to pass judgment upon public officials and public policies. Despite their public and quantitative nature, however, the meaning of electoral results in any terms beyond "won" and "lost" is by no means clear. Exactly what was the electorate voting for or against? This paper offers one tool for resolving some of this ambiguity by proposing a method by which the changes in the partisan distribution of votes within electoral districts may be attributed to national, subnational, or local complexes of influences. Prefacing the introduction of this new technique is a brief review of the most prominent methods of approaching this problem employed in the past. In the discussion of these methods a number of weaknesses are focused upon; these, it is argued, are largely overcome through the use of the more refined model presented here. The results of an application of the new technique to a series of congressional elections are then reported. Finally, a number of possible applications of this technique, both within and beyond the realm of electoral analysis, are briefly noted.

The voter in an American election is faced with a wide range of stimuli, some national, some statewide, some local, to which he may react and on the basis of which he must decide whether and how to vote. It would be useful to define and measure for each district three variables reflecting the relative importance of each of these three levels of influence in determining the outcome of elections. In Schattschneider's terms, what is to be measured is the "degree of nationalization of electoral forces,"¹ or what might also be called the "proportion of district-level electoral variance explained by national level forces," or the "relative magnitude of the national variance component," as well as the

corresponding variables defined for the state and local levels. Since this problem has been attacked before, I begin by discussing two earlier models of aggregate electoral swings and the measurement techniques associated with them.

Previous Approaches to Electoral Nationalization

In any attempt to operationalize these variables, a fundamental ambiguity must be resolved: that is, at least two reasonable definitions may be assigned to the set of terms just introduced. "Nationalization of electoral forces," and the corresponding term at the state level, clearly imply similarity among different areas; it is necessary to specify the type of similarity intended. For example, "nationalization of electoral forces" may refer to the degree to which a shift in the distribution of the vote in one district is identical to the shift in the distribution of the vote in each other district. This is the operationalization employed by the earlier models. On the other hand, this term may refer to the degree to which different areas respond to the same electoral forces, whether or not the effects of these forces are numerically identical across space. By this second definition, "nationalization" is achieved when the issues (broadly conceived) which determine the shifts in votes among elections are identical in all districts, even if they produce numerically different results in different districts.

If what is to be measured is the degree to which districts move together in a partisan sense, the most obvious measure is the standard deviation of the difference in the partisan distribution of the vote between two elections, computed across districts. That is, if x_{jk} is the Republican proportion of the vote in district j at election k , we compute the standard deviation of the distribution $x_{jk+1} - x_{jk}$. The magnitude of this variable is partially determined by the overall volatility of the vote; thus a more interpretable measure derived from the same general model is the proportion of the variance in the local vote distributions explained by the assumption that

$$x_{jk+1} = x_{jk} + (\bar{x}_{k+1} - \bar{x}_k), \quad (1)$$

¹ See Elmer E. Schattschneider, *The Semisovereign People: A Realist's View of Democracy in America* (New York: Holt, Rinehart and Winston, 1960), chap. 5. Donald Stokes, "Parties and the Nationalization of Electoral Forces," in *The American Party Systems: Stages of Political Development*, ed. William N. Chambers and Walter D. Burnham (New York: Oxford University Press, 1967), pp. 182-202 uses the term more nearly as it is meant here.

where \bar{x}_k is the average Republican proportion of the vote, computed across districts, at election k ; restated in words, the assumption is that the Republican proportion of the vote in election $k + 1$ in district j is equal to the Republican proportion of the vote in that district in the immediately preceding election plus the difference between the *national* Republican proportions of the vote in elections $k + 1$ and k . If desired, this equation may be rewritten in equivalent form,

$$x_{jk} = n_j + (\bar{x}_k - \bar{n}), \quad (2)$$

where n_j is the "normal" vote for the Republican party in district j and \bar{n} is the "normal" national Republican vote.² Again in words, this suggests that the vote for the Republican party in any given district may be expressed as a normal vote plus a deviation from that normal vote, where the deviation is *constant for all districts at any single election*. The standard deviation measure tells the degree to which all districts follow a common trend, while the analysis of variance technique tells how reliable an estimate may be made on the assumption that they do follow a common trend. (By "normal" vote in this context is meant the metaphysically defined distribution of votes which would occur if no temporally related influences were operating. It is, of course, unmeasurable. For the purposes of all these models, which are concerned only with shifts in votes between elections, the average distribution is an acceptable estimate for the "normal" distribution. The mean distribution is the baseline from which deviations are measured in this paper.)

This model, operationalized by computing the standard deviation of the distribution of interelection differences, has been employed with some interesting results by Butler and Stokes in comparing Britain and the United States,³ but it suffers from a number of deficiencies, two of which apply only to the standard deviation technique and two of which apply to the model generally. First, the standard deviation technique is one that may be applied to only one pair of elections; if for any reason one or both

of these elections is seriously atypical, the results will be correspondingly biased. As expressed in equation (2), however, the model may easily be applied to a series of elections. Second, so long as only one pair of elections is analyzed, it is impossible to compare areas within a nation, since only a national index is computed. Further, cross-national comparisons using the standard deviation technique must be suspect since the value of the resulting index is a function of overall volatility as well as of "nationalization." Again, as expressed in equation (2) the model may be applied to a series of elections, in which case the proportion of variance explained by the equation may be computed separately for each district. Cross-national comparison is also facilitated since the index resulting from the application of this technique is standardized with regard to overall volatility. Third, and most important, the notion that electoral choice is a purely national phenomenon, implicit in this model, is a rather severe oversimplification of reality. In the words of one group of analysts, "We can think of no good reason and only one bad reason—habit—to justify the assumption that the words 'The Democratic Party' conjure up the same image in all voters."⁴ It is quite clear that support for the Democratic party in the state of Alabama does not necessarily imply support of the same policy stands, or even interest in the same policy areas, as does support for the Democratic party in the state of New York. Considering this, it is hard to imagine why the proportions of the vote received by these two parties should move in unison. It is particularly toward the removal of this deficiency that the alternative model presented below is directed. Finally, as I shall argue, the definition of "nationalization" which is implicit in both these models seems to be fundamentally deficient.

In order to take account of state and district level effects in interpreting the results of congressional elections, Donald Stokes has proposed a more elaborate "variance components model of political effects" based on the statisti-

² See Philip Converse, "The Concept of a Normal Vote," in *Elections and the Political Order* by Angus Campbell, Philip Converse, Warren Miller, and Donald Stokes (New York: John Wiley, 1966), pp. 9–39. For the purposes of the analysis reported here, the "normal Republican vote" has been defined to be the mean Republican share of the two party vote over the entire decade of the 1950s.

³ Stokes, "Parties and the Nationalization of Electoral Forces." See also David Butler and Donald Stokes, *Political Change in Britain* (New York: St. Martin's Press, 1969), pp. 66–75.

⁴ David Kovenock, Philip Beardsley, and James Prothro, "States, Party, Ideology, Issues, and Candidate Choice: A Preliminary, Theory-Relevant Analysis of the 1968 American Presidential Election," a paper prepared for the 8th World Congress of the International Political Science Association, Munich, September, 1970, p. 13. For a demonstration that "party image" and party identification can have independent effects, see Donald Matthews and James Prothro, "The Concept of Party Image and Its Importance for the Southern Electorate," in *The Electoral Process*, ed. M. Kent Jennings and Harmon Zeigler (Englewood Cliffs: Prentice-Hall, 1966), pp. 139–174.

al model of analysis of variance.⁵ Equation 2) may be rewritten by replacing the single subscript j (representing the j th district) with the pair of subscripts ij standing for the j th district within the i th state; and by replacing the second term on the right with the single expression a_k , which is the constant (through space) effect of national forces on the partisan distribution of votes specific to election k , obtaining

$$x_{ijk} = n_{ij} + a_k. \quad (3)$$

The model which Stokes has proposed starts with equation (3) and adds to it two more terms, b_{ik} , representing the deviation from the prediction of equation (3) which is specific to election k in all districts of state i (the impact of state level forces); and c_{ijk} , representing the remaining deviation, postulated to be the effect of district-level forces in district ij at election k . The final equation thus becomes:

$$x_{ijk} = n_{ij} + a_k + b_{ik} + c_{ijk}. \quad (4)$$

From this equation, Stokes derives the expression

$$\text{Var}(x_{ij}) = \text{Var}(a) + \text{Var}(b_i) + \text{Var}(c_{ij}), \quad (5)$$

plus three covariance terms which he finds to have negligible impact and therefore ignores. The degree of electoral nationalization is then quite obviously defined to be the ratio of $\text{Var}(a)$ to $\text{Var}(x_{ij})$, that is, the proportion of the total variance in the partisan distribution of votes which is attributable to national factors. Likewise, the relative importance of statewide and local factors may also be computed.

The primary defect of the Stokes model is its implicit definition of electoral nationalization, that is, its measurement of the degree to which the trends in districts are identical not only in direction but also in magnitude. (The same objection, of course, applies at the state level.) Two obvious difficulties with this definition may be observed, both arising from the fact that different districts are, in fact, different along any number of demographic and political dimensions. First, if a national (or state or local) factor is assumed to have a constant impact on some types of voters, e.g., leading to a five per cent shift among blacks towards the

Republican party, the impact of that factor on the aggregate electoral result, far from being constant across districts, should be a function of the proportions of the electorates in those districts who are of those types. In this example, the effect of the factor suggested would obviously be much greater in a district with a large proportion of blacks than it would be in a district with very few blacks. Indeed, since different groups may react in opposite ways to the same stimulus, the direction as well as the magnitude of the shift caused by a single factor may vary across districts because of varying distributions of ethnic, social, political, or economic groups.

Second, districts may vary considerably in their susceptibility to influence from whatever source, for example, because of differences in the distribution or intensity of partisanship.⁶ Ignoring differences of this kind can lead to quite incongruous results—possibly a prediction that since the country swung 10 per cent Democratic, a district which in the last election was 95 per cent Democratic will in this election be 105 per cent Democratic. Looking at Stokes's own data disaggregated and examined one district at a time, one finds that in nearly 10 per cent of the cases the proportion of the overall variance explained by national factors is *negative*. (That is, the prediction of the Republican proportion of the vote based on equation (3) is, on average, less accurate than a prediction of the normal vote for each election.)

This is a difficulty worth overcoming, and the following model is proposed as a step in that direction. As Stokes observed, mathematical models, despite the disciplined and "finished" form in which they are presented, are continually evolving. This model should be seen as a step forward (I hope) from his model, but not necessarily as the final step. Because of its complexity, it will be described in some detail, at the risk of some redundancy of definition of terms whose meaning may have already become clear.

An Alternative Technique

As in Stokes's model, it is assumed that the partisan distribution of votes in a district at any given election is the result of a normal vote for that district plus deflections from that norm caused by national, state, and district level

⁵ Donald Stokes, "A Variance Components Model of Political Effects," in *Mathematical Applications in Political Science*, ed. John M. Claunck (Dallas: Southern Methodist University Press, 1965), pp. 61-85. An elementary discussion of analysis of variance may be found in William Hays, *Statistics* (New York: Holt, Rinehart and Winston, 1963), chaps. 12 and 13. A more technical treatment of variance components analysis may be found in Shayle R. Searle, *Linear Models* (New York: John Wiley, 1971).

⁶ Butler and Stokes, pp. 135-150; Bernard Berelson, Paul Lazarsfeld, and William McPhee, *Voting* (Chicago: University of Chicago Press, 1954), especially chap. 5; D. Katz and S. Eldersveld, "The Impact of Local Party Activity Upon the Electorate," *Public Opinion Quarterly*, 25 (Spring, 1961), 1-24.

forces. By the national (or state or district) effect on the district vote is meant the impact of national (or state or district) factors on that vote. The term "factor" is broadly conceived and includes both variables that are and are not under political control. Also as in Stokes's model, it is assumed that the factors operating at each level may be aggregated to form a single index for that level. This index will be called the national (or state or district) force. In fact, the model is concerned only with these aggregate indices; their meaning may become clearer, however, if the definition of their component parts, the factors, is made more explicit.

Under this definition of "factor," such variables as unemployment, labor unrest, candidates, crime, foreign events, or the weather might all be factors which impinge on electoral outcomes. While the existence of a factor is independent of voting, its designation as a national, state, or district factor is made solely on the basis of the geographic range of the district electorates affected by it. For example, a riot is clearly a local phenomenon; the way in which the riot is controlled, however, may make it a national factor if it affects the confidence of voters throughout the country in the ability of the government. Any factor that impinges on the choice of voters in all districts is defined as a national factor; a factor which is specific to the voters of a single state is a state factor; and any factor which is specific to a single district is a district factor.⁷ It should be stressed that the range of voters influenced by a factor, and hence the level to which it is assigned, may be independent of its geographic origin or the level at which it might be controlled. It must also be emphasized that this model does not, in fact, require that individual factors be either catalogued or quantified.

Stokes assumes that the effect of each force, as just defined, is constant for all districts in which that force applies. For the reasons outlined above, this seems to be a shaky assumption at best. In its place, the present model assumes that in each district the effect of a force may be expressed as a linear function of that force, where at each level the parameters of the function are specific to the *district*, although fixed over time. The model which this describes may be expressed by the equation

$$x_{ijk} = n_{ij} + (p_{ij})(a_k) + (q_{ij})(b_{ik}) + (r_{ij})(c_{ijk}), \quad (6)$$

where p_{ij} , q_{ij} , and r_{ij} are the coefficients of the linear functions and the constant terms have been absorbed into the normal vote term, n_{ij} .

In fact, solution of equation (6), or estimation of its parameters, is impossible, since the only measurable variable is x_{ijk} . With one additional assumption, however, values of the three variables of interest—measures of the proportion of the variance in the district level distribution of the vote explained by each level of political forces—may be estimated. At the same time, it will be possible to estimate regression coefficients which are constant, linear, but unknown, functions of p_{ij} , q_{ij} , and r_{ij} . While no absolute interpretation of these coefficients is possible, they are internally comparable, and thus suitable for regression or correlation analysis.

The assumption which makes this analysis possible is that district forces are independent across districts and that state forces are independent across states. Then, since the numbers of districts and states are both large, both sides of equation (6) may be summed over the entire country with the expectation that the last two terms on the right will sum to constants over time. This being the case, it may immediately be seen that the national average distribution of votes is a linear function of the national force. Since the coefficient of determination is unaltered by linear transformations, the proportion of the district level variance explained by national forces may be estimated by squaring the correlation between the national average and the district distributions of votes over a series of elections. Further, the residuals from a regression equation predicting the district distribution of vote from the national distribution are the same as those which would have been obtained had the national force itself been used as the independent variable. (This assertion is based on the fact that the number of districts is so large that the contribution of each individual district to the national average is negligible.) Repeating this procedure on the residuals within each state in a nested two-stage regression scheme,⁸ one can compute a regression coefficient (\hat{q}_{ij}) and the proportion of variance explained for the state level. Finally the remaining variance is attributed to the district level.

⁷The definition should actually be somewhat broader, since it is my intention to include those things which are caused by national factors, even if they do not themselves affect all voters.

⁸See Searle; John Johnston, *Econometric Methods* (New York: McGraw-Hill, 1963), pp. 258-260.

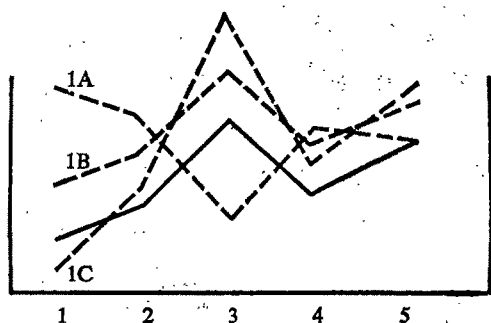


Figure 1. National Force (Solid Line) and Three Hypothetical Districts with High National Variance Components

The meaning of "nationalization" implied by this model may be made clearer graphically. In Figures 1 and 2, the solid line represents the national forces at each of five elections, while the broken lines represent the partisan distribution of votes in each of a number of districts. For all those districts in Figure 1, the national variance component is quite large because the district vote is clearly a linear function of the national force. This is true even for District 1A, for which p_{ij} , from equation (6), would be negative. The crucial fact is that for each of these districts, the vote distribution is a function almost entirely of national forces, even though the reactions to those forces are different in every district. The national variance components for each of the districts represented in Figure 2, however, are all quite low. Again, the concern is with the degree to which districts respond to the same forces, not with the degree to which their responses are identical.

The solid line in Figure 3 shows the deviation from the normal vote at each election between 1952 and 1960 for the Second Congress-

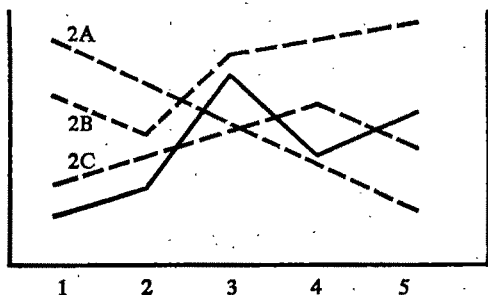


Figure 2. National Force (Solid Line) and Three Hypothetical Districts with Low National Variance Components

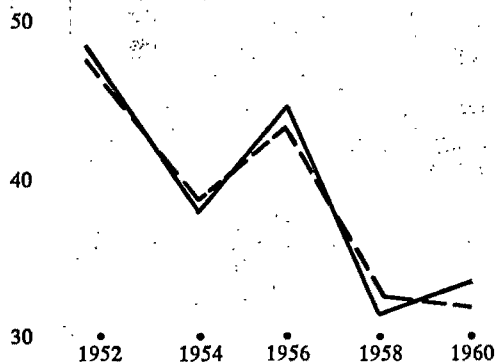


Figure 3. Actual Percentage Republican (Solid Line) and Estimated Republican Percentage (Broken Line) for the 2nd Congressional District of Illinois

sional District of Illinois, a district in which, by the estimation of this technique, 96.8 per cent of the variance in the partisan distribution of votes could be explained by national-level forces. The broken line shows the estimate of the vote for each election made on the basis of the national force. It will be noted that these lines very nearly coincide, indicating that the estimate is reasonably accurate. In Figure 4, the same type of data is displayed, this time for the Thirtieth District of California, in which only 39.5 per cent of the variance could be explained with reference to national forces. In this graph, the estimate clearly follows the actual data much less closely, illustrating that in this district factors other than those summarized in the index of national forces play a significant role in determining shifts in the partisan distribution of votes.

The principal defect of the technique just suggested applies primarily to subnational units

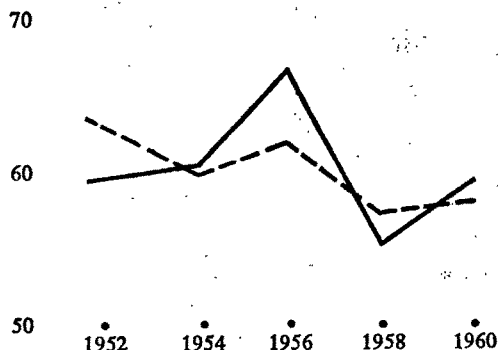


Figure 4. Actual Percentage Republican (Solid Line) and Estimated Republican Percentage (Broken Line) for the 30th Congressional District of California

(e.g., states and districts). That is, this model may be generalized and applied to any number of levels of causation, but for each level of causation considered, one degree of freedom is lost in the estimation of each lower (i.e., included) level. This means that with the same number of elections, the estimates become progressively less good as more levels of effects are considered. This defect is shared by the Stokes model and indeed is inevitable in any statistical treatment of congressional election results. It means, however, that a fairly long series of electoral data may be required for any complex analysis. On the other hand, the gain is that the definitional handicaps of the previously discussed models have been overcome, for what is now measured is the degree to which the electoral trends in different districts may be considered to be the effects of common causes, rather than the degree to which the trends themselves are the same.

It may be fairly objected that this model, like the other, deals only with the behavior of those voters who shift, either between parties or into or out of the electorate. Nonetheless, two observations may be made which suggest that this is, at worst, a marginal problem. First:

Because of the pervasive effects of party loyalties, no candidate for Congress starts from scratch in putting together an electoral majority. The Congressman is a dealer in increments and margins. He starts with a stratum of hardened party voters, and if the stratum is broad enough he may have a measurable influence on his chance of survival simply by attracting a small additional element of the electorate—or by not losing a larger one.⁹

This situation, of course, is faced by candidates for other offices as well. Although most voters vote regularly and for the same party, those who do shift may have a decisive impact on the outcome of elections. Thus, to the extent that the model is concerned with only a small segment of the electorate, it is an important segment. Second, when one considers how these voters might be influenced, it becomes clear that the attitudes of many of the regular party voters are important, and are considered indirectly by this model. On the one hand, the candidate may appeal directly to the uncommitted voters. On the other, he may appeal to them through others. From what is known of electoral behavior, it is quite clear that the appeal through others is likely to have the greater effect. Those who are most likely to shift their votes are least likely to make use of the media

or to have significant levels of political information at their disposal.¹⁰ It is unlikely that they shift on the basis of a direct appeal from the candidates. Rather, these people acquire what political information they have and those political cues which are likely to determine their votes through "opinion leaders," intermediaries who are more interested in politics, more informed, and more likely to vote regularly.¹¹ The level of activity of the opinion leaders, the fervor with which they campaign, the frequency with which they are sources of positive cues, may be influenced, even though their votes for the candidates of their parties are more or less assured. While the candidate is guaranteed the minimal support, the votes, of "a hardened stratum of party voters," the appeal he makes to them may determine how well he does among the uncommitted. This means that the attitudes towards national, state, and local influences of those who do not change their votes are also considered by the present model. While it does not allow separation of these two paths of influence, it does allow estimation of the relative importance of the three levels of electoral factors in determining the final deviation from the normal vote in any given district.¹²

An Illustrative Application to American Congressional Elections

Application of this technique requires that a fairly long series of electoral data be available for each district to be studied. Whatever the ultimate impact of the wave of reapportionment brought about by the Supreme Court on policy or democratic legitimacy, it has rendered the decade of the 1960s unsuited for this analysis. Consequently, the data analyzed in this section were taken from the five congressional elections

¹⁰ Philip Converse, "Information Flow and the Stability of Partisan Attitudes," *Public Opinion Quarterly*, 26 (Winter, 1962), 578-99.

¹¹ Paul Lazarsfeld, Bernard Berelson, and Hazel Gaudet, *The People's Choice*, 2nd ed. (New York: Columbia University Press, 1960); Berelson et al., pp. 109-17.

¹² This line of reasoning suggests, incidentally, that aggregate analysis of this type might be preferable to survey analysis even if the individual level data were available. Since much of the causation dealt with is postulated to be indirect, it is unlikely that individuals would give "accurate" responses if asked whether they give most weight to national, state, or district issues. Further, since the Congressman must deal with his constituency as a whole, in considering congressional behavior aggregate measures may be more appropriate. To be strictly correct, what we are measuring is the degree to which a constituency behaves *as if* each voter weighted each level of causation in a given way, and not the degree to which this is actually the case.

⁹ Warren Miller and Donald Stokes, "Constituency Influence in Congress," in Campbell et al., p. 368.

which took place between 1952 and 1960. The other requirement of this technique is that there be more than one district within each state in order that the state and district level effects may be separated. The Republican percentage of the two-party vote was therefore computed for each of the 280 congressional districts, in 31 states, which met the following three requirements (these are the same requirements Stokes set in applying his technique, which has the same data requirements):

- (1) The district boundaries were not changed during the decade. (This condition eliminated a few districts from two states which partially redistricted during the 1950s.)
- (2) The district was contested in each of the five elections. (This condition eliminated virtually all of the South as well as a small number of northern districts.)
- (3) At least one other district in the same state met conditions (1) and (2). (This, of course, eliminated the very small states.)

In those cases in which one of the major party candidates received the endorsement of a third party, the votes thus received were totaled with those received as the candidate of the major party, the decision having been made that third-party endorsement should be considered to be a prime example of a district-level force. If a candidate was endorsed by both of the major parties, the district was considered to have been uncontested and was thus eliminated from the analysis.

Table 1 compares the results of this analysis with those obtained by Stokes using the same data and the technique already discussed.¹³ It will be immediately observed that the values obtained by this method differ radically from those which Stokes obtained. While both sets of figures suggest that the state level is the least significant, the positions of the national and district levels are reversed. Where Stokes's model attributes nearly half of the variance in the partisan distribution of votes to the district level, the new method suggests that more than half of the variance is the result of national-level forces. In light of what is known about the political thinking of even the vast majority of opinion leaders, any model which ascribes great importance to local factors must be suspect. (It should be remembered that both models assign any variance which is not explained by national- or state-level forces to the district level. In this respect, the district-level variance component is in part a measure of the "badness

¹³ Stokes, "A Variance Components Model of Political Effects," p. 76.

Table 1. Percentage of District Level Variance Explained by National, State, and District Level Forces

	Explained Variance (%)		
	National Level	State Level	District Level
National Average (New Technique)	54.6	19.2	26.2
Stokes's Results	32.0	19.0	49.0

N=280

of fit" of the model, and it is probable that the district-level variance component which was reported for the new technique is still too large.)

In Table 2, the average percentage of the district-level variance in electoral results explained by national-, state-, and district-level forces is set forth for a number of subsets of districts. Within each level, an analysis of variance was performed for each partition of districts, and the results of these analyses are presented in the table.

When the districts are disaggregated according to dominant party and level of competitiveness,¹⁴ two trends emerge. First, districts in which the Democratic party averaged more than 50 per cent of the two-party vote tended to be considerably more national in their orientation than were districts which were dominated by the Republicans. Second, within each party's sphere of influence, the more competitive the district, the more variance was explained nationally. The latter finding is not at all unexpected, and is quite consistent with the finding that turnout is also related to the degree

¹⁴ The definition of competitiveness used here is derived from Edward Cox, "Measurement of Party Strength," *Western Political Quarterly*, 13 (December, 1960), 1022-42. The mean and standard deviation of the Republican percentage of the two party vote over the decade were computed. A district was defined to be a "marginal Republican" district if the mean Republican percentage was between 50 per cent and 50 per cent plus one standard deviation, a "moderate Republican" district if the mean was between 50 per cent plus one standard deviation and 50 per cent plus two standard deviations, and a "safe Republican district" if the mean was greater than 50 per cent plus two standard deviations. The Democratic districts were likewise classified. This measure has the virtue of taking into account the volatility of the vote as well as its mean. In a "marginal" district, we would estimate the probability of the dominant party's winning an election to be between .5 and .8, in a "moderate" district we would estimate this probability to be between .8 and .97 and in a "safe" district to be greater than .98.

Table 2. Analysis of Variance of National, State, and District Variance Components for Subsets of Districts

	National		State		District		N
	Explained Variance	F Test	Explained Variance	F Test	Explained Variance	F Test	
Party Competition							
Safe Democratic	60.5		14.9		24.6		57
Moderate Democratic	63.9		18.1		18.1		27
Marginal Democratic	66.8	$p \leq .01$	9.9	NS	23.3	NS	18
Marginal Republican	56.0	$F_o = 3.34$	19.1	$F_o = 1.95$	24.9	$F_o = 1.26$	41
Moderate Republican	51.7	$F_r = 2.29$	20.5	$F_r = 2.29$	27.8	$F_r = 2.29$	52
Safe Republican	46.2		23.7		30.0		85
Region							
New England	57.9		23.3		18.7		19
Mid-Atlantic	47.4		19.4		33.2		85
E. North Central	65.7		11.6		22.7		83
W. North Central	60.3	$p \leq .01$	21.0	$p \leq .01$	18.7	$p \leq .01$	38
South (N. Carolina only)	24.4	$F_o = 5.90$	44.7	$F_o = 3.96$	30.9	$F_o = 3.93$	7
Border States	56.8	$F_r = 2.09$	28.1	$F_r = 2.09$	15.1	$F_r = 2.09$	15
Mountain States	55.3		22.0		22.7		12
Pacific States	34.7		25.7		39.7		21
Number of Congressmen During Decade							
One	54.4		21.4		24.1		126
Two	52.1	NS	18.1	NS	29.8	NS	105
Three	61.5	$F_o = .95$	15.4	$F_o = .98$	23.2	$F_o = 1.79$	43
Four	55.0	$F_r = 2.45$	25.5	$F_r = 2.45$	19.4	$F_r = 2.45$	5
Five	32.2		3.4		64.5		1
Ballot Form							
Party Column	57.8	$p \leq .05$	16.6	NS	25.6	$p \leq .01$	111
Modified Party Col.	62.2	$F_o = 3.37$	22.6	$F_o = 1.46$	15.2	$F_o = 4.69$	32
Office Block	50.3	$F_r = 3.00$	20.6	$F_r = 3.00$	29.2	$F_r = 3.00$	137

NS = $p > .05$. F_o = Observed value of F . F_r = Value of F required for statistical significance at $p \leq .05$.

of competition within the district.¹⁵ It suggests that in competitive districts, more attention is devoted to national politics, which is only reasonable, since it is in competitive districts that most of the national campaign is focused, and it is in competitive districts that any shift in the distribution of votes may have a nationally relevant impact. The finding that Democratic districts tend to be more national in their voting than Republican districts may be explained by remembering that the most important national trend during the decade in question was the

gradual disassociation, in the minds of the voters, of President Eisenhower and the Republican party. In 1952, Eisenhower drew enough voters to Republican congressional candidates to elect the only Republican Congress of the decade. By 1956, his coat-tails were essentially nonexistent. This loss of Eisenhower's drawing power is a factor which would affect most strongly districts in which there were a large number of Democrats. (The Republicans, although perhaps reinforced in their voting intentions, did not shift and consequently did not contribute to the amount of variance to be explained or to its explanation.) The fact that one factor was of such importance suggests that even a five election series may not be long enough to give a completely unbiased picture. It should be remembered, however, that this series of elections is not a sample, but is rather

¹⁵ For evidence that turnout is related to perceived closeness of the election, see Angus Campbell, Philip Converse, Warren Miller, and Donald Stokes, *The American Voter* (New York: John Wiley, 1964), p. 54; for data suggesting that turnout is related to competitiveness in primary elections see V. O. Key, Jr., *Southern Politics* (New York: Alfred A. Knopf, 1949), p. 506.

the universe of elections which took place during the decade studied. Further, it is the same universe of elections upon which the political actors involved had to base their judgments. But if different issues had been salient during this period, the same districts might have scored differently.

Looking at the data by region, one finds that the only areas which diverge greatly from the national averages are the South and the Pacific states. With regard to the South (actually, seven North Carolina congressional districts were the only southern districts to meet the three requirements for inclusion in the analysis), it should be stressed that the observed difference is not the result of Southerners reacting differently from Northerners to the same issues; the low national variance component indicates that regardless of the direction of the reaction, factors which were salient in the North and West were not salient in the South. While it is impossible to tell with these data, it seems reasonable to suppose that at least a part of the large state variance component is actually the result of regional forces. For the Pacific states, a similar (although less pronounced) trend appears, primarily because of the influence of the state of Washington.

If the personal characteristics of the incumbent are important in determining electoral outcomes, one would expect the district variance component to be large. This expectation may be upset, however, if only one man represents the district during the time considered. In this case, what is legitimately part of the district effect may be incorrectly interpreted as part of the normal vote. For this reason, districts were disaggregated according to the number of turn-overs of representative during the decade. No significant relationship was found between the number of incumbents and any of the three variance components. Indeed, the greatest deviation from the national average—the mean for districts which elected three different congressmen during the interval—was in a direction opposite to that which would have been expected. Thus, it may reasonably be argued that turn-over has little impact on this measure. The overall volatility of the vote in a district also has little impact on the measure, the correlation between the standard deviation of the Republican percentage of the vote and each of the three variance components being less than .10.

Another possibility is that institutional arrangements, particularly the form of the ballot, might alter the results. Cummings finds, for example, that the correlation between presidential and congressional votes increases as it becomes

institutionally easier for the voter to cast a straight party ballot.¹⁶ On an individual level, it has also been found that the level of split-ticket voting is related to the form of the ballot.¹⁷ This leads to the expectation that the presidential coat-tail effect will be strongest in those states which employ the party column ballot format, and might lead to the expectation that these states would also have districts with the highest national variance components. The results are difficult to interpret because the largest national variance components are observed for those districts which have a ballot format intermediate between the party column and office block types. This format requires a separate vote for president but follows the party column format for the other offices. One possible explanation is that this intermediate form of ballot requires some thought for the voter to link the presidential nominee with the rest of his party's ticket, encouraging the use of party as an identifying cue in addition to relying on the name of the presidential nominee. Consequently, in off-year elections, when there is no presidential nominee, voters in these states may have acquired the necessary facility to allow them to link party and president. In the straight party column states, this facility may not be developed, however, because even in presidential years, the voter need rely only on party label or candidate's name (not both as in the other states). In the office block states, the complexity of the task and the paucity of cues provided may make consistent voting more difficult, thus increasing the district level variance component.

Other Suggested Applications

The computation of a national average of the proportion of electoral variance explained by national, state, and local level forces represents a substantial step in itself toward reducing the ambiguity of national electoral returns. On the basis of the analysis reported here, one can reasonably infer that swings in the congressional vote are importantly influenced by forces operating nationally. The American political system looks considerably more national, and less like a loose federation of unrelated local political systems, than Stokes's analysis would lead one to expect.

The utility of this approach is not limited to the description of a single national political system. As Stokes demonstrated with his model,

¹⁶ Milton Cummings, Jr., *Congressmen and the Electorate: Elections for the U. S. House and the President, 1920-1964* (New York: Free Press, 1966), pp. 172-97.

¹⁷ Campbell et al., *The American Voter*, pp. 143-49.

the decomposition of local electoral variance can lead to interesting cross-national comparisons. For example, when computed on the basis of the 1946 election of the Constituent Assembly and the 1953, 1958, and 1963 elections to the *Camera dei Deputati* in Italy, the average proportion of the variance in the Christian Democratic vote explained by national factors (in a sample of 721 from the more than 7,000 communes) was 37 per cent. This is considerably below the American figure reported in Table 1. Even without performing the computation, it may be asserted that the corresponding figure for Great Britain would be noticeably higher than for the United States. (The proportion of the variance attributed to national factors by the new method must be higher than the 47 per cent which Stokes reports.)

As the regional comparisons reported in Table 2 imply, since the index of nationalization is computed for each district, this technique may also be used for intranational comparisons. The dramatic differences in the degree of electoral nationalization between the South and Pacific Coast states and the rest of the country come as no surprise to anyone familiar with American electoral politics during the decade of the 1950s. Additionally, when enough parties are actively competing so that the assumption that the proportion of the vote received by one party is independent of that received by another appears to be reasonable, comparisons between parties within a single nation may be made. Table 3 illustrates these points by comparing the national variance components for the *Democrazia Cristiana* and *Partito Socialista Italiano* in five regions of Italy. For the nation as a whole, the national variance component computed for the PSI is more than one and one-half times that computed for the DC. Further, the differences among regions for the PSI are even more dramatic than are those between the parties.¹⁸

Although no analysis of the regression coefficients produced has been carried out beyond observing that the national regression coefficients (corresponding to p_{ij} in equation [6]) were negative for the North Carolina districts upon which the southern averages were based, analyses similar to those just noted could also be performed on them.

While interesting by themselves, the national, state, and district variance components may also be profitably employed as inputs to more

Table 3. National Variance Components for *Il Partito Socialista Italiano* and *La Democrazia Cristiana* by Region

	Democrazia Cristiana	Partito Socialista Italiano
National Average	.37	.68
Northwest	.39	.82
Northeast	.43	.85
Center	.41	.74
Mezzogiorno	.30	.44
Sicily & Sardegna	.36	.47

far-reaching analyses. These components are descriptive measures; no hypothesis is tested when they are computed. Once established, however, they may be used as either dependent or independent variables in analyses which do test hypotheses regarding many aspects of political behavior.

If democracy is a system of government in which the actions of policy makers are controlled by the people, the relationship between the behavior of the electorate and that of legislators assumes clear importance. In attempting to explain the behavior of American legislators, reference has frequently been made to their constituencies. A number of proposed theories, both normative and empirical, specify the apparent or preferred nature of the connection between constituents and the behavior of legislators. Two general classes of these models may be profitably reviewed here to illustrate how the aspect of constituency behavior described by this technique might figure in analysis: the "responsible parties" models of democracy, and the "constituency delegate" models.

The responsible parties model argues that political power ought to be vested in a cohesive political party, which by virtue of its total control of the government may be held totally accountable for its actions. That this model makes demands upon the legislator is understood; he is to follow explicitly the platform or leaders of his party. In the legislature, he is expected to vote with his party; in his contacts with his constituents, he is to be a representative of his party, adhering to its principles even at the cost of his seat. This model also makes demands upon the voter, although these are less often articulated. He is assumed to vote for a national party, either because he agrees with its platform or because he has confidence in its leaders. Orientations toward local candidates and issues, except as taken up by the national parties, are to be ignored, as are traditional loyalties. In the terms we have been using, the

¹⁸ Regional definitions are from Giorgio Galli and Alfonso Prandi, *Patterns of Political Participation in Italy* (New Haven: Yale University Press, 1970), p. 332.

level of "nationalization of electoral effects" is assumed to be quite high.

Opposed to this model, the constituency representative model stresses the representation of people and interests, even at the expense of clearly defined responsibility for the government as a whole. Where the responsible parties model suggests that the legislator be bound either to his national party's platform or to its leaders and argues for collective responsibility, this model favors individual responsibility and initiative on the part of independent legislators. The legislator is assumed to act independently of any supraconstituency influence, and correspondingly, the voters are assumed to be primarily oriented toward local factors.

The technique which has been proposed allows measurement of the degree to which the behavior of a constituency electorate follows the prediction of one or the other model of representation. Examination of congressmen's behavior on roll-call votes gives one indication of the model with whose predictions they are acting in accord. Table 4 reports for each party the correlation between congressmen's party unity scores on the party opposition roll calls taken during the 87th Congress (the last Congress elected during the decade from which the electoral data analyzed here were taken) and the national variance components for their districts. The expectation is that there will be a positive correlation, indicating that legislators and their constituents conform to the same model of representation. This should be the case if the problem is attacked normatively, since the legislator *should* orient himself towards politics in a way consonant with the orientation of the electorate. It should also be true if the problem is approached behaviorally, since by describing voters' decision criteria, we specify the behavior patterns required of the legislator if he is to maximize his chance of reelection; while by specifying the legislator's behavior patterns, we describe many of the stimuli affecting the voters.

The table confirms the expectation that the importance of national factors in determining local electoral results is positively correlated with party voting. For both parties this relationship is statistically significant, albeit not overwhelmingly strong. When domestic and foreign policy roll calls are considered separately, the relationship appears to be stronger for domestic issues, marginally for the Democrats but significantly for the Republicans. If domestic policy is further divided, the greatest contribution to this relationship is seen to be made by the general field of social welfare leg-

Table 4. Correlations Between Media Access Variables and National Variance Components for Il Partito Socialista Italiano and La Democrazia Cristiana

	Independent Variable		National Variance Component	
	2	3	DC	PSI
1. Radios per Capita	.32	-.69	.11	.41
2. Televisions per Capita		-.26	.10	.15
3. Per cent Illiterate			-.09	-.47

islation, for which correlations of .21 for the Democrats and .22 for the Republicans were observed.

Alternatively, analysis may be directed toward explaining the differences in the degree of electoral nationalization observed among electoral districts. Although it is not logically necessary, since intermediate structures could perform the same function, a high degree of electoral nationalization is more likely to be found in those areas where a high proportion of the voters have direct access to the national media than in those areas where information is largely filtered through local intermediaries. Looking again at Italy—where the variance in such variables as literacy (prerequisite to access to the printed media), ownership of radios, and ownership of televisions is significant—Table 5 shows that the hypothesized correlations actually exist, especially for the PSI. The greater strength of these relationships for the PSI is probably the result of its organizational weakness. While such organizations as Catholic Action and the Catholic Church itself can serve as nationalizing influences on the Christian Democratic voter in the absence of access to the national media, no similarly strong surrogate exists for the Socialist. Thus media access carries much more relative weight in determining whether or not the Socialist voter will respond to national forces.

Finally, the general approach I have suggested here is applicable beyond the realm of electoral behavior whenever variance at one level is to be attributed to a number of higher levels in a nested arrangement. For example, this approach might be applied in explaining variations over time in the budgets of bureaus that are nested within divisions, which in turn are nested within departments and finally within the nation as a whole.¹⁹ Other examples might be arms levels within alliances or trade

¹⁹ I am grateful to one of the anonymous reviewers for suggesting this possibility.

figures nested either geographically or by commodity type. Other applications will, no doubt, occur to those working in these and other areas of research.

The technique for apportioning variance among several nested levels of influence which has been presented here has been shown to

overcome some of the definitional handicaps of earlier methods. It must not be regarded as the final step in the gradual evolution of models in this area. It is to be hoped, however, that it will prove an aid to further progress in both substantive and methodological research.

Comment: On the Measurement of Electoral Dynamics

DONALD E. STOKES

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No one who seeks to capture in a formal model the realities of political or social change is likely to feel the results are free of ambiguity. I gave a good deal of thought to the point raised by Richard Katz and to several other interpretive questions that surrounded my analysis of electoral change. The point discussed by Katz is an important one. Although I do not accept his view of what ought to be done, it is a pleasure to acknowledge the work of so penetrating a critic.

There is, to begin with, no difference between us as to the nature of the national forces measured by my own model. They are indeed forces tending to produce similar shifts in the strength of the parties (or the extent of turnout) in all constituencies. I might well have used some modifying adjective such as "unidirectional" or "uniform" in describing these forces. I undertook the analysis in the first place because I wanted to learn more about the role which like swings of electoral fortune might play in the cohesion of congressional or parliamentary parties. Hence, my statistical operations were, as Katz correctly notes, intended to describe the influence of such forces.

But I was well aware, as Katz also notes, that national issues (or state or regional issues) can produce swings of different magnitude or even direction across a set of local constituencies. I said in part:

Some forces which arise at a higher level may have opposite effects in different states or local constituencies. For example, the religious aspect of the candidacy of John Kennedy in 1960 had quite different effects in Protestant and Catholic areas.¹

Indeed, I included within my model several covariance terms which were intended to detect the differential impact of national forces on state or local politics and of state forces on local constituencies. I noted that

such effects will tend to produce a positive or negative covariance between movements of the vote at higher and lower political levels. In 1960, for example, the net Republican gain in the national mean due to religion would have been *positively* associated with the movement of the vote in Protestant states or districts, *negatively* associated with

the movement of the vote in Catholic states or districts.²

The estimates of the values of these covariance terms did prove very helpful in assessing the importance of differential effects in congressional and parliamentary electoral change in the recent past. They suggested in particular that differential effects had been of much less importance than had uniform effects. Indeed, when these covariance terms were converted to their normalized form as correlation coefficients and their magnitude measured by the square of the product moment correlation coefficient, the values obtained were no larger than we would expect by chance if differential effects were completely negligible.

I should emphasize this aspect of the model and the empirical results to which it led, since it bears on a main difference between Katz's approach and my own. My reasoning was this. If differential effects of national and state forces were in fact completely absent from electoral politics and the model were applied to a finite number of elections, chance patterns of variation would tend to yield values of these squared correlations greater than zero, despite the absence of real effects. But we also know how large these chance departures from zero values would tend to be for a given number of elections. Hence, this expected departure, rather than zero, is the appropriate benchmark against which to compare the empirical values obtained from five elections. When this comparison was made for the American congressional elections of the 1950s it emerged that the squared correlations were on the average no greater than we would expect them to be if differential effects had been completely negligible. This finding encouraged me to focus on the variance components terms of the model, which were intended, as Katz notes, to measure the influence of uniform national and state forces.

The stratagem was simply a particularization to my analysis of a prudential point that needs to be kept in mind in undertaking any correlation or regression analysis. With only a few data points we will often be able to fit a regression line quite well and exhibit a high positive or negative correlation coefficient even if there

¹ Donald E. Stokes, "A Variance Components Model of Political Effects," *Mathematical Applications in Political Science* (Dallas, Texas: The Arnold Foundation, 1965), p. 74.

² Stokes, p. 75.

is actually no correlation in the population from which a limited sample is drawn. Let us for example assume, for sake of argument, that the mean rainfall across the country on election day has no actual bearing on the Republican share of the vote for President. Yet if we confined our attention to only two elections and correlated rainfall with Republican strength at these two points in time, we would obtain a perfect correlation and be able to "explain" all of the variance of the vote since a regression line can always be exactly fitted to two data points. Even if we had data from three elections it is likely that we would obtain a very high correlation (and a high proportion of explained variance) as our line-fitting capitalized on chance. Indeed, if the joint distribution of rainfall and Republican vote were bivariate normal with no underlying relationship between the two, we could expect rainfall still to explain half of the variance of Republican strength as we fitted a regression line to data from five elections. These considerations argue the importance of designing a correlation or regression analysis to distinguish real from chance effects.³

These considerations must be kept in view in judging the validity of the approach proposed by Katz. His technique for gauging the influence of national forces on fluctuations of the vote essentially (1) plots for each of five elections the proportion of the vote received by one of the parties in a given constituency against the proportion the party received in the country as a whole; (2) fits a regression line to these five data points; (3) calculates the proportion of the variance of the constituency vote that is "explained" by the national vote; and (4) averages these proportions of explained variance across a set of constituencies to gauge the impact of national forces in the whole country or some part of it.

The central difficulty with this approach, as my prior remarks suggest, is that it is so likely to capitalize on chance patterns. If, in the limiting case, the technique were applied to data from only two elections, the national and constituency proportions would be perfectly correlated in every constituency, and all of the variance of party strength in the constituencies between the two elections would be "explained" by national forces. With the addition of a third election, and even a fourth and fifth, the ability

of the technique to capitalize on chance would still be high.

It follows from this that the technique would tend to inflate the apparent importance of national forces more if applied to a small number of elections than if applied to a larger number. In other words, if we were to start with a given number of elections and randomly remove elections from the analysis, the proportion of the variance explained by national forces would tend to rise as elections were deleted (although there would be some random upward and downward movement around the upward trend) until we finally would have explained all the variance when only two elections remained.

The same remarks can be made about Katz's treatment of state or regional forces, except that in this case the prior extraction of estimates of the importance of national forces diminishes still further the fund of information with which the analyst can work. The reduction in degrees of freedom that results from extracting estimates of national before turning to state forces amounts to diminishing the number of elections from five to four. As a result, the estimates obtained for state forces are vulnerable to a similar inflation.

There is a suggestion in Katz's piece that he dismissed such doubts by reminding himself that he was dealing with a complete population of elections and not a sample. He in particular observes that "... this series of elections is not a sample but is rather the universe of elections which took place during the decade studied." He hedges this position, quite properly in my view, by noting that "were different issues salient during this period, the same districts might have scored differently," a remark that admits of the possibility of seeing actual events as being drawn from a wider class of potential events. Moreover, he implicitly takes such a sampling view, as I think he should, in applying F-tests to differences between groups of constituencies.

These are questions that very quickly take us into the philosophical mists. I can well understand why some people find it strange to admit a wider class of possible events into their examination of the pattern of actual events. Whatever Katz's view of these deeper questions, however, I do not think that regarding the elections of a given period as a complete universe dispels the difficulties I have described. Once again, the case of two elections makes the point admirably. Two successive congressional elections would most certainly constitute the complete population of congressional elections in this two-year period. But this quite correct

³ This point is widely commented on by the literature. See, for example, S. S. Wilks, *Mathematical Statistics* (New York: John Wiley and Sons, 1962), p. 593.

observation would not make it any less nonsensical to apply Katz's technique to these two elections and attribute all of the variance in constituency returns between the two elections to national forces. Equally, denying a sampling viewpoint does not help us over the difficulty that the importance attributed to national forces would tend to decline as more elections were brought into the analysis.

In view of these considerations, I do not see in Katz's empirical results any reason to question my own for America and Britain in the recent past. His higher estimates of the importance of national forces can easily be attributed to the liability of his technique to inflate these estimates when applied to data from only a few elections. Similarly, his higher estimates of the importance of state forces in explaining the variance that remains after the apparent effects of national forces have been removed can be laid at the same door. The exceedingly nonvanishing values of the covariance terms of my own model led me to realize that there was a lot of chance to be capitalized upon in five elections—as well as to believe that uniform forces were more important than differential ones in the period I considered.

Although the comparisons Katz draws between his results and mine are, for the reasons I have given, not very helpful, the comparisons

he draws between his results for different groups of constituencies seem to me more interesting. It is of course possible that the difficulties I have cited have inflated his estimates more in some groups of constituencies than in others. But if we assume that the bias is fairly uniform, there are interesting contrasts to be drawn by region, party strength, turnover of Member, and the like. I should also applaud his extending his work to comparative materials. So few serious attempts have yet been mounted to contrast electoral patterns from more than a single political system that such a venture is welcome, however much the results must be regarded as a first approximation.

Let me therefore conclude by saying that I share with Katz an interest in non-uniform electoral forces. My earlier analysis focussed primarily on uniform forces since I was interested in their role in the cohesion of legislative parties. But I also tested for the presence of non-uniform forces. The results of the test were negative for the period of my work. But the test made me aware of how important it is to distinguish real from chance effects. Katz's failure to do so almost certainly leads him to overestimate the importance of national forces. Hence, I endorse his ends but not his means and feel that his findings on recent American and British elections are easily reconciled with my own.

Rejoinder to "Comment" by Donald E. Stokes

RICHARD S. KATZ

After carefully considering Donald Stokes's comment, which was particularly useful because on every major point each of us apparently understood what the other had done, I can see two points of contention upon which I would like to comment further. The first, which is given almost exclusive attention in Stokes's comment, is his technical objection to my analysis. The second arises from the fact that our methods, being based on different models of political reality, are attempting to quantify fundamentally different things, although perhaps unfortunately calling them by the same name. With regard to the first point, the question is whether my application of the technique I proposed was methodologically permissible. With regard to the second point, the question, whose answer is not dependent on the answer to the first question, is which model is theoretically superior in the sense of being the more useful.

Stokes's technical objection to my analysis is in reality an objection to the application of the technique to a short series of data. He does not apparently object to the internal logic of the model, nor I must conclude need he object to the results of an analysis based on a longer series of data. This is an important point, to which I will return, as our techniques would not yield the same results no matter how long the series of elections. His point regarding the application of my technique to a series of five elections is important as well, however, since so long as the constitutional dictate of decennial reapportionment is in force, we can never expect to have more than five congressional elections without changes in constituency boundaries, and even where longer series are available, they are spread over so long a time as to raise serious questions of the comparability of their beginnings and ends.

There are two aspects to the question of whether five elections are adequate for analysis: the vulnerability of the measure to the nonrandom peculiarities of the five elections analyzed, and the extent to which the model is simply capitalizing on random variation in the data. Stokes's comment is addressed primarily to the latter, although his observation that my method would attribute 100 per cent of the variance to national factors with a two-election series is fundamentally gratuitous to both aspects of the question.

Certainly with limited series of data the results of any analysis are vulnerable to their idiosyncracies. As Stokes notes, I warn of this in discussing the possible impact of Eisenhower's candidacy when I observe that "were different issues salient during this period, the same districts might have scored differently." The same vulnerability applies to Stokes's method; had no national hero been a candidate for the presidency in the 1950s, Stokes might well have found a significantly lower level of electoral nationalization. In this respect I intended to suggest that the five elections might be considered as being drawn from a wider class of events. I did not intend to suggest that either my technique or Stokes's allows us to generalize to this wider universe. Thus one is led to stress the importance of basing comparison, in so far as possible, upon comparable series of data. This point is particularly well illustrated by Stokes's analysis of turnout, in which a normalized national variance component of .86 is observed precisely because presidential elections coincide with alternate congressional elections. Stokes is wise, therefore, when he only contrasts British and American figures with regard to partisan distribution (although the comparability of these, too, is unknown), leaving turnout aside from his international comparison.

The extent to which the technique which I have proposed can be said simply to capitalize on random factors depends on the relative magnitudes of the random and nonrandom elements of variation in the data. This, in turn, is largely dependent on whether we view the elections analyzed as constituting a random sample from the universe of possible elections or rather as a highly nonrandom sample representing, aside from measurement error and the ever present "other factors," the universe to which we may generalize. In his comment, Stokes appears to favor the sampling viewpoint. If we take this approach, random factors must be seen as important, and my technique would certainly be vulnerable to them. But is this a reasonable approach? I would suggest that it is not, for the same reasons that it would not be reasonable for a physician to generalize about the effects of all drugs after experimenting with a random sample of five medicines. Regardless of the number of individuals upon whom he experiments (or in our case regardless of the

number of constituencies involved) five is an inadequate sample for generalizations to be made to an infinite universe. This is as true of Stokes's work as it is of my own. Thus, we are forced to adopt a far more restricted view of the range of our generalizations. I observe that the "series of elections is not a sample but rather the universe of elections which took place during the decade studied" because to attempt to generalize beyond the elections which took place would condemn any statistical analysis of election results to invalidity. But, if we take this more restrictive view, the relative importance of the random element in the data rapidly diminishes. (When I applied F-tests to differences between groups of constituencies, the view I was taking was to regard the *constituencies* as a sample.)

Indeed, the impact of a small data base on my technique need not always be in the direction which Stokes suggests. While the proportion of variance which I would attribute to national forces will be affected by random error in short series of data (as Stokes's national variance component may also be affected) because of the relatively small size of the random factor this effect may not be so large as to lead, as Stokes suggests it must, "to an entirely artificial reduction in the estimate placed on the importance of such [national] forces as additional elections [are] brought into the analysis." In the original article, I reported the national variance components for the *Democrazia Cristiana* for five regions of Italy based on a series of four elections. In Table 1, I reproduce these figures together with the corresponding data based on a series of six elections, those on which the original analysis was based augmented by the elections of 1948 and 1968.

Table 1. National Variance Components for
La Democrazia Cristiana by Regions

	National Variance Components Based on:	
	6 Elections	4 Elections
National Average	.44	.37
Northwest	.47	.39
Northeast	.62	.43
Center	.50	.41
Mezzogiorno	.31	.30
Sicily and Sardegna	.36	.36

In no case was the estimate of the importance of national factors higher when based on the shorter series of elections. While this fact un-

derlines the vulnerability of the measure to the peculiarities of each election of a short series, it does suggest that Stokes vastly overrates the liability of my technique to artificially inflate the national variance component.

My conclusion with regard to the criticism that five elections are too few would be this: to the extent that it is valid, it applies equally to Stokes's procedure and to my own, as well as to any other analysis of electoral data. That is, neither of our procedures provide a sound basis for generalization beyond the particular elections considered unless an *a priori* assumption of their representativeness of the wider universe is made. What each does is to provide a statistical summary description in the terms of the model upon which it is based of a series of elections in a particular place, at a particular time, and in particular political circumstances.

Whatever weight one eventually decides to give to Stokes's technical criticisms, they do not strike at the theoretical basis of what I have tried to do. Yet the fact remains that with any conceivable series of data, Stokes's procedure and my own will yield different estimates of the impact of national, state, and district level forces. This is because they are measuring fundamentally different things. The "nationalization" which Stokes measures is the degree to which changes in the partisan distribution of votes are numerically identical across districts. The "nationalization" I am measuring is the degree to which these changes may be treated as if they were the results of a common cause. Stokes appears to miss the importance of this difference when he attempts to defend his empirical results against mine. There is no reason for him to do this. In comparing his results with mine I did not wish to imply that there was anything technically wrong with his figures, which are the result of quite masterful analysis. In saying that "any model which ascribes great importance to local factors must be suspect," I did not mean that his figures were the result of improper analysis but that they were based on a definition of "nationalization" which did not seem to me to be very useful.

The real question is not whose numbers are right; I would contend that within their own frameworks both are correct. The question is which model of reality is the more useful for any particular theoretical or analytical purpose. My complaint about Stokes's model is that it fails to take account of differences among districts which could lead the same forces to have effects of different magnitude or even direction in different areas. To this complaint, Stokes's observation with regard to the impact of Ken-

nedy's Catholicism is not a defense but a caveat. Kennedy's religion is a factor, in other words, which must be taken into account in interpreting his results, not one taken into account by his model. Other issues which might have different effects in different areas (race relations, restrictive trade legislation, and defense spending to name only three) come readily to mind. It was precisely because Stokes's model must mishandle this type of issue that I was led to the alternative which I have proposed.

Stokes and I have proposed different models, or theories, of the way changes in the partisan

distribution of votes occur at the district level. Each has then derived a quantitative description of the congressional elections of the 1950s in the terms of his model. The descriptions differ because the underlying notions of political reality differ; no doubt each has oversimplified and distorted reality to some extent. It is my contention, simply, that the model of reality I have proposed oversimplifies less and that the measure of nationalization produced is more useful to the analysis of political behavior and more in line with what is generally meant by "explained variance" in social research.

Electoral Choice in the American States: Incumbency Effects, Partisan Forces, and Divergent Partisan Majorities*

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During the last two decades of political research, the survey studies of voting behavior have achieved perhaps an unparalleled accuracy in the specification and explanation of a dependent political variable—the vote for president. Theories have been developed and tested at the individual level concerning a host of sociological, psychological, political, and institutional factors thought to give partisan direction to voting in presidential elections. Comparable scholarly attention to individual voting in statewide contests is nowhere apparent. By and large, it is thought that the path to statistical success in the study of state politics lies in the use of aggregate election figures which, unlike national sample surveys, may be delineated by state political boundaries.

Aggregate analyses of partisan vote divisions in various statewide contests, however, leave a good deal of confusion in their wake—indeed, skepticism that the theories of individual voting behavior tested in the context of presidential elections hold much relevance for the explanation of individual voting decisions in statewide contests. In 1960, for example, sixteen of the thirty-six states which held either gubernatorial or senatorial elections gave a majority in one of those races to a party other than the party which won the state presidential vote. Kallenbach observes that only 59.7 per cent of state elections during the period 1956 to 1964 produced pluralities for presidential and gubernatorial candidates of the same party when these elections occurred simultaneously.¹ A variety of computations performed on statewide election

results reveals these apparent disparities between state and national contests.

On the opposite side of the coin, however, the empirical studies of individual voting decisions for president surely place some constraints on one's willingness to assert that the issues associated with state political contests largely account for these excessive disparities in the aggregate pattern. We know that most voters in presidential contests simply cast a party vote, and also that state politics generally holds much less saliency for the voter than does national politics. These findings would not recommend the hypothesis that voter evaluations of burning statewide issue conflicts can account for divergent aggregate patterns. Indeed, we might even suggest that the relationship between basic partisan loyalties and voting is stronger in state political contests than in presidential contests, since the higher saliency levels usually accompanying presidential contests yield greater opportunities for particular issues to stimulate partisan defections.

This question of why the partisan results of various statewide contests are often so markedly different from the partisan results of presidential contests in those states has received little attention, in spite of its obvious implications for contemporary theories of voting behavior. The more fundamental observation to be made, however, is that these two dependent political phenomena—national presidential politics and state politics—find explication in two somewhat distinct bodies of literature. Indeed, the separate paths which students of state politics and those of national politics have taken is an interesting bit of intellectual history in itself. One body of research focusing clearly on state politics has been directed toward the aggregate analysis of election outcomes in the states—a field which has come to be known as comparative state politics.² A second body of research

* This article is adapted from the author's doctoral dissertation, *Electoral Decision-Making in the American States: Reconciling Individual Predispositions and Aggregate Patterns* (Ann Arbor: University of Michigan, 1971). Acknowledgments are due Samuel J. Eldersveld, Gudmund R. Iversen, Donald E. Stokes, and Herbert F. Weisberg for comments on the original manuscript. The data were originally collected by the Survey Research Center Political Behavior Program and were made available by the Inter-university Consortium for Political Research, Ann Arbor, Michigan. Neither the original collectors of the data nor the Consortium bear any responsibility for the analyses or interpretations presented here.

¹ Joseph E. Kallenbach, *The American Chief Executive* (New York: Harper and Row, 1966), p. 104.

² Notable examples include: V. O. Key, Jr., *American State Politics: An Introduction* (New York: Alfred A. Knopf, 1956); Herbert Jacob and Kenneth N. Vines, eds., *Politics in the American States* (Boston: Little, Brown and Company, 1965); John H. Fenton, *Midwest Politics* (New York: Holt, Rinehart, and Winston, 1966); Duane Lockard, *New England State Politics* (Princeton: Princeton University Press, 1959); and

has taken the presidential vote at the individual level as the primary dependent variable, and these analyses have been based largely upon the national election surveys.³ The conclusion that the divergence is "data-driven" seems inescapable: The advent of the national sample surveys led electoral theorists to test propositions at the level of the individual voter. Yet since national sample surveys do not ordinarily yield valid subsamples for each of the states, the principal emphasis has been on the presidential voting decision.

On the other hand, students of state politics generally have not sought statewide sample surveys of individual voters.⁴ Rather, the availability of data on states as analytic units from the United States Bureau of the Census has itself shaped the kind of theory which scholars of comparative state politics have developed. Sensitive to Robinson's ecological fallacy, investigators of state politics have developed and tested theories about "state behavior" which bear little resemblance to theories developed by students of national presidential politics.

The purpose of this project is twofold: (1) to apply some of the theoretical propositions of individual voting behavior tested at the presidential level to voting in gubernatorial and senatorial elections and (2) to propose appropriate modifications of that theory in the state electoral context which will account for the observed aggregate disparities. The thesis is that one source of conflicting partisan majorities between statewide office contests and presidential contests within the state lies in an important contextual phenomenon of state elections—the prevailing candidate incumbency pattern. The patterned practice of state electorates to yield

partisan majorities to different parties, depending on the office, does not necessarily serve as evidence that influential partisan loyalties at the presidential level break down as an influence at the subnational level. The partisan incumbency context of the election, however, provides an additional voter motivation either for partisan reinforcement or for defection from party. I shall also examine the role of candidate incumbency status in overriding the changing reactions to the two parties across several dimensions of attitude. The contention is that forces operating at the presidential level are similarly operative on the individual voter at the state level but that the specification of candidate incumbency context is useful for explaining individual partisan defections and aggregate partisan differences within and across the states.

Theoretical Overview, Data Base, and Operational Definitions

Federal forms of government breed federal patterns of politics, and these diversities in state and national partisanship have been noted by scholars of American politics. Thus, the Republican party of New York state is not best conceptualized as the political appendage of the national Republican party in that geographical area. Any state political party will come to possess over time a set of traditions, a group of grand old men, and a series of statewide issue positions unique to the state party; and its own leaders and its opposition leaders will remain sensitive to whatever marked departures from national party policy arise.

In a federal system, many of these distinctions will be lost upon mass publics. If the classical requirements of democratic government are difficult to meet for the citizen of a unitary state, the complexities increase multiplicatively in a federal arrangement. The voter is asked to know something of several candidates for different offices, several sets of issues associated with different levels of government, and several political parties. The fragmentary nature of the information which the typical American voter is able to assimilate about his politics is well documented, at least for presidential politics. The parallel of this in the case of state politics is not difficult to draw.

My theoretical view of voters in subnational elections is not unlike that of voters in the national elections. If the voter's psychological identification with his political party is as fundamental as it seems, then the ramifications for subnational voting should be comparable to those at the presidential level. Similarly, evaluations of party will be viewed as if no basic dis-

Thomas R. Dye, *Politics, Economics and the Public* (Chicago: Rand McNally and Company, 1966).

³ For example, Angus Campbell, Philip E. Converse, Warren E. Miller, and Donald E. Stokes, *The American Voter* (New York: John Wiley and Sons, 1960). Notable exceptions include: Donald E. Stokes and Warren E. Miller, "Party Government and the Saliency of Congress," *Public Opinion Quarterly*, 26 (Winter, 1962) 531-546; Angus Campbell, "Surge and Decline: A Study of Electoral Change," *Public Opinion Quarterly*, 24 (Fall, 1960) 397-418. These studies, however, are not concerned with state political contests.

⁴ Although the selection is small, some few state sample surveys have been conducted. A four-state study is analyzed in *Public Opinion and Congressional Elections*, ed. William N. McPhee and William A. Glaser (New York: The Free Press of Glencoe, 1962). Also, the Comparative State Elections Project of the Institute for Research in Social Science, University of North Carolina, conducted a thirteen-state study during the 1968 national elections.

distinctions are being made at different levels of government. There is, in fact, good evidence for such an assumption, since almost none of our respondents ask for clarification on level of government when asked about either of the two parties. Some respondents, typically less than 3 per cent, offer remarks which suggest a state political party referent. While political sophisticates are quick to discern apparent differences between state and national parties, these distinctions are not terribly salient ones among mass electorates. By and large, a reference to one of the two major parties conjures up a set of impressions about that party as an entity which is rather unlike the scholarly view of a highly fragmented American party system.

Set in the context of current social-psychological theories of voting behavior, the condition that state electorates give partisan majorities to different parties, depending on the office contest, suggests that any of several fundamentally different processes may be operative. The marked effect of the voter's psychological identification with his political party on his vote for president is well documented. Much-attenuated effects of party loyalty in contests for state office could easily underlie the patterned practice of state electorates to select candidates of different parties for state versus presidential office. With reduced effects of party identification, the vote for various statewide office contests would be free to fluctuate as a function of the electorate's reactions to statewide issues and candidates. This view of voting for statewide office contestants would be in line with Charles Adrian's contention that partisan loyalties affect voting in state elections only slightly.⁵

While it is common for state electorates to divide their partisan majorities among different partisan contestants, the percentage differences in these majorities are only moderate. It is exceedingly rare, for example, that state A's electorate gives 80 per cent of its vote to the Democratic presidential candidate but only 20 per cent to the Democratic gubernatorial candidate. Thus, with only moderate differences in percentage terms, basic party loyalties may be equally potent in statewide office and presidential office contests, but the sources producing moderate levels of partisan defection may be quite varied in the two kinds of contests. In contrast to the stability of individual party identifications, the electorate's evaluations of the Republican and Democratic parties show a

good deal more change from election to election. The voter is often led to vote against his own party's presidential candidate when he is dissatisfied with the party's domestic or foreign policies, group posture, or performance in government. While these more fluid dimensions of partisan attitude have been shown to have a substantial effect on the division of the two-party vote for president, it does not necessarily follow that they influence subnational party evaluations in the same way. Other dimensions of attitude tied to the political posture and performance of the state parties may guide momentary defections from party at the state level. Thus, while party identification may be equally significant in voting for state and presidential contests, different partisan attitude sets associated with those two kinds of contests may produce such alternate patterns of partisan defection.

A third view of voting in statewide contests would converge more closely with that associated with the studies of individual-level voting in presidential elections. Both basic partisan loyalties and the evaluations of party across several dimensions of attitude, changing from time to time, may mark the vote for senators and governors in a manner similar to their presidential vote effects. Other factors associated with particular state elections may aid in specifying why the Democratic presidential candidate receives equivalent vote proportions in two states while the Senate campaigns of his partisan colleagues exhibit widely divergent results. As noted above, extensive treatment will be given here to one such contextual factor—the statewide candidate incumbency pattern—which may contribute to differential partisan majorities in gubernatorial, senatorial, and presidential contests.

The theoretical meaning of a contextual variable such as the candidate incumbency pattern prevailing in a state is a good deal more elusive than it appears at first consideration. In the narrow sense, the concept will be measured perfectly by specifying whether the respondent voted in a state election pitting, for example, a Democratic incumbent against a Republican challenger. Yet, the linkage between that condition, which presumably favors the Democratic candidate, and the voter's decision is mediated by any of several components of individual reaction, some of which flow from that initial condition, others of which do not. Consider the following five items which may be thought to be among the more important elements of the incumbent's net advantage: (1) candidate familiarity, (2) campaigning advantages associ-

⁵ Charles R. Adrian, *Governing Our Fifty States and Their Communities* (New York: McGraw-Hill Book Company, Inc., 1963), p. 112.

ated with officeholding, (3) voter's sense of approval of the *status quo*, (4) candidate personal appeal, (5) candidate issue stands. The first three fall most clearly within the commonly conceived notion of an incumbent's advantage. First, voters are more likely to know the names of the incumbent candidates and, for any election other than a high stimulus one, that familiarity will be a decided advantage. Second, through a host of structural properties associated with officeholding, the campaign apparatus of the incumbent candidate will show a clear advantage over that of the challenger (e.g., carrying out official duties, while also campaigning, at public expense; public funding of dissemination of mailed "reports" to constituents; employing administrative assistants, fortunately skilled in both administration and politics; patronage privileges; private constituent services; and occasional free radio and television time for "reports to the people"). Third, a sizable portion of the electorate, generally uninformed about political affairs and feeling a relative sense of satisfaction with "things as they are," may simply approve of that *status quo* by voting for the incumbents.

These kinds of considerations fit well with what is normally thought to be the incumbent advantage. The remaining two may show up in a statistical treatment of the subject, although they are not entirely consistent with our conceptual notion of the incumbency effect. An incumbent may outdistance his opponent in electoral royalties by virtue of the same issue stands and personal appeal elements which favored him in an earlier election when he, himself, was a nonincumbent candidate. Since measures of personal reactions to the senatorial and gubernatorial candidates are unavailable, the assessment of the pure incumbency effect may be hampered. Several comments suggest that the distorting effect may be less than initially thought, although it undoubtedly remains to some extent. First, the candidate familiarity advantage is likely to overshadow the remaining components. For electorates relatively uninformed about state political contests, simple recognition of one candidate is an advantage likely to exceed the subtleties of various candidate issue stands. Stokes and Miller have shown that, in contests for the U. S. House of Representatives, the "incumbent candidate is by far the better known."⁶ Second, the incumbency

effect will be estimated by utilizing a data set spanning many contests and several election years. By including all respondents from a national sample voting in Democratic incumbent-Republican challenger contests, a good portion of the individual candidate effect will have subsided. For example, is there much reason to believe that the personal appeal of a Republican incumbent in one state is markedly greater than that of a Democratic challenger in another state; and is that difference likely to be systematic across contests? Especially under the various control conditions set out below, I am inclined to think that these residual effects have not much inflated the incumbency effect estimates. Yet, it should be acknowledged that the incumbency variable as a measure of prior officeholding is an imperfect measure in the broader sense in light of the fact that these alternative effects cannot be systematically removed.

Data for the analysis were taken from the national sample surveys of the University of Michigan's Survey Research Center, spanning a period of some fourteen years. The well-known question concerning the basic party identification of the respondent serves as the measure of party loyalty. Earlier work has shown, however, that while the elector's basic identification with his political party remains remarkably constant over time, he is likely to pick and choose from time to time among alternative party candidates as a result of momentary partisan evaluations. That is to say, a respondent who considers himself to be a strong Democrat may vote Republican after becoming dissatisfied with the Democratic incumbent president's handling of foreign policy matters. Measurement of these more fluid attitudes toward party was achieved through the use of several free-answer questions, tapping the respondent's evaluations of the Republican and Democratic parties. From the 1956, 1960, 1964, and 1968 National Survey Studies, indices of partisan attitude on domestic issues, foreign issues, group related attitudes, party performance in government attitudes, and presidential candidates were constructed for each respondent.⁷

The results of ascertaining the respondent's choice for U.S. senator and governor scarcely have appeared in earlier reports on individual voting decisions, although the senatorial vote question has been included in the Survey Research Center's questionnaire since 1956 and the gubernatorial vote question since 1964.

⁶ Donald E. Stokes and Warren E. Miller, "Party Government and the Saliency of Congress," in Angus Campbell, Philip E. Converse, Warren E. Miller, and Donald E. Stokes, *Elections and the Political Order* (New York: John Wiley and Sons, Inc., 1966), p. 204.

⁷ The appendix contains a technical description of the construction of these indices.

These two pieces of information, as well as the presidential vote, will be used in this paper to pinpoint the patterns of individual electoral choice which give rise to divergent partisan majorities in the states. On occasion, these national samples will be pyramided, or combined, in order to produce a more adequate sample size for various control conditions.⁸

Several decisions must be made when we attempt to isolate respondents in each of the three possible kinds of contests: (1) Democratic incumbent opposed by a Republican challenger; (2) neither candidate holding incumbency status; and (3) Republican incumbent opposed by a Democratic challenger. First, those respondents voting in contests where the winners received more than 85 per cent of the two-party vote or where both the Democratic and the Republican parties were not represented among contestants were excluded from the analysis.

Especially during the 1950s, Democratic gubernatorial and senatorial candidates in the South won election and re-election by wide margins—typically 90 to 100 per cent. The justification for excluding respondents voting in uncontested races is obvious. Less obvious is the justification for also excluding those respondents voting in contests with only token opposition for the majority party (i.e., the opposition gaining less than 15 per cent of the vote). These token opposition contests show a high concentration in the southern elections of the 1950s and early 1960s, in which the Republican party initially sought to establish a basis in the South. In order to vote against a Democratic incumbent, a southern respondent would have been required to vote not just against the Democratic party but also for a party which had only begun to surface in his state. Thus, not all southern respondents, but those respondents voting in these noncompetitive elections (85 to 100 per cent for the winner) are excluded from the analysis. The practical effect is a reduction in the number of southern respondents, the inclusion of which would have led to a probable overestimation of incumbency effects.

To be coded as an incumbent, a candidate must have held office for at least one year, either by interim appointment or election, prior to the time of the election in question. Candi-

dates were not coded as incumbents when their appointments occurred within one year of the election; since one component of the incumbency phenomenon is undoubtedly visibility, a short stint in office coupled with reduced exposure resulting from appointment without prior campaign experience would not place that candidate on an even footing with other incumbents. Special elections, when they occurred on the same date as regularly scheduled elections, were included. On occasion, however, three states held both regular and special Senate elections on the same date: Kentucky and South Carolina in 1956; and Tennessee in 1964. Rather than deal with multiple responses, those three special elections were excluded.

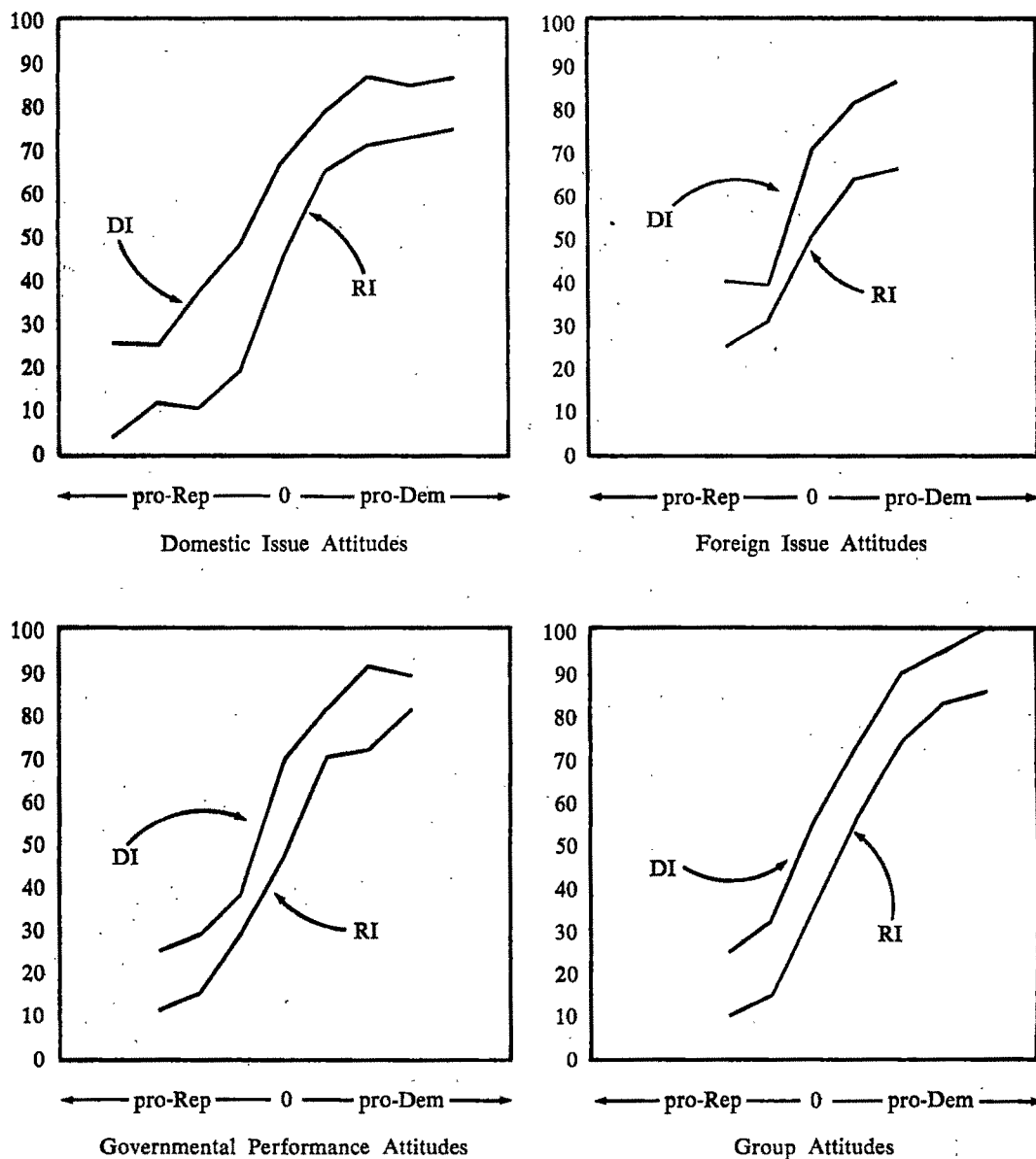
Party Loyalty, Attitudes toward Party and Subnational Voting

Do the voter's attitudes toward the parties across several dimensions, which so clearly affect presidential voting,⁹ have similar implications for senatorial combatants? This question seems well answered by the graphical display of Figure 1. In these graphs, the percentage voting Democratic is plotted for each level of partisan attitude, retaining the distinction between the two types of incumbency contests. The considerable monotonicity in these relationships is clear, with few departures over the four dimensions of partisan attitude.¹⁰ It is also clear from these graphs that the incumbency status of the candidate is quite important. Thus, in spite of the respondent's negative reaction to one of the parties, he is more likely to vote for that party's candidate if that candidate is an incumbent. These differences are very nearly constant across levels of partisan attitude.

⁹ Campbell et al., *The American Voter*; Donald E. Stokes, "Some Dynamic Elements of Contests for the Presidency," *American Political Science Review*, 60 (March, 1966) 19-28.

¹⁰ The question of whether these patterns are indeed linear is an interesting one. Although there are some perturbations on the extremes, I am satisfied with some earlier work in which I utilized a linear multiple regression model to partition the departure of the aggregate vote from 0.5 (the assumed result for evenly matched contests) into two components: one indicating the net contributions of attitudes toward the parties, the other indicating the net contribution of the incumbency advantage. I have since, however, considered an alternative hypothesis which suggests an S-shaped curve for which the effects of the attitude decrease slightly as the attitude becomes more extreme in either partisan direction. A reasonably good fit is obtained with a least squares polynomial of the third degree, and the behavior of the first and second derivatives of the obtained function rule, at various points on the attitude scale, is quite consistent with certain derived consequences of the non-linear theoretical formulation. That work is in progress.

⁸ For a discussion of the conditions under which the pyramiding of several national samples can be justified, see Andrew T. Cowart, *Electoral Decision-Making in the American States: Reconciling Individual Predispositions and Aggregate Patterns* (Ph.D. dissertation, University of Michigan, 1971), chap. 2.



DI: Democratic incumbent, Republican challenger
 RI: Republican incumbent, Democratic challenger

Percentage voting Democratic for U.S. Senator by four dimensions of
 partisan attitude and by candidate incumbency pattern
 (Pyramided sample)

Figure 1

One difficulty in the interpretation of the differences in the levels of the slopes for the two types of incumbency contests lies in the varied distribution of basic party loyalties across the states. State A's electorate may return Democratic candidates to office with substantial vote

margins, not because those candidates are incumbents, but rather because the distribution of basic party identifications is rather onesidedly Democratic in the state. Thus, the selection of respondents voting in Democratic incumbent races unduly increases the number of

Table 1. Percentage Voting Democratic for U. S. Senator by Party Identification, Domestic Issue Partisan Attitudes, and Candidate Incumbency Pattern^a
(Pyramided Sample)

Incumbency Pattern	Domestic Issue Partisan Attitudes								
	Democrats			Independents			Republicans		
	+	0	-	+	0	-	+	0	-
Dem. Incumb./Rep. Chall.	93%	91%	74%	69%	60%	46%	40%	17%	12%
Total N, all voters	(308)	(189)	(88)	(77)	(100)	(97)	(40)	(82)	(165)
No. Incumb.	91	81	53	59	27	33	*	16	9
Total N, all voters	(173)	(94)	(36)	(29)	(48)	(49)	(19)	(69)	(126)
Rep. Incumb./Dem. Chall.	83	74	56	53	39	22	26	15	3
Total N, all voters	(242)	(149)	(36)	(81)	(103)	(68)	(47)	(127)	(206)

^a "+" indicates pro-Democratic score, "0" indicates neutral score.

"-" indicates pro-Republican score on indices of partisan attitude.

* Cell size too small for specification.

Democratic party identifiers, while the selection of respondents voting in Republican incumbent races similarly increases the comparable proportion of Republicans. A major advantage of the pyramided data set is that the total number of respondents is large enough for an adequate test of the three effects—party identification, attitudes toward party, and incumbency status of the candidate.

In Tables 1 and 2 the percentage voting Democratic is given within each combination of the three independent variables. Party identification and the indices of partisan attitude have been collapsed to three levels. The two dimensions of partisan attitude chosen for this

test are attitudes toward the parties on domestic issues and attitudes toward the parties as managers of government. Similarly constructed tables for the remaining two dimensions of attitude would reveal very much the same pattern, except that the lower dispersion on those two attitudes leaves more cell entries unspecified. Within each of the three categories of party identification, the tables display the percentage of respondents voting Democratic for U.S. senator by attitudes toward the two parties on each dimension and by the incumbency pattern of those state elections. Comparisons of the percentages down the columns of the tables suggest the extent of candidate incumbency

Table 2. Percentage Voting Democratic for U. S. Senator by Party Identification, Governmental Performance Partisan Attitudes, and Candidate Incumbency Pattern^a
(Pyramided Sample)

Incumbency Pattern	Governmental Performance Partisan Attitudes								
	Democrats			Independents			Republicans		
	+	0	-	+	0	-	+	0	-
Dem. Incumb./Rep. Chall.	92%	91%	71%	79%	61%	37%	27%	24%	13%
Total N, all voters	(313)	(203)	(69)	(61)	(132)	(81)	(26)	(83)	(178)
No Incumb.	86	85	67	*	41	32	*	21	9
Total N, all voters	(162)	(98)	(43)	(20)	(49)	(57)	(10)	(62)	(142)
Rep. Incumb./Dem. Chall.	84	76	57	56	42	25	13	18	6
Total N, all voters	(230)	(130)	(67)	(48)	(120)	(84)	(32)	(107)	(241)

^a "+" indicates pro-Democratic score, "0" indicates neutral score.

"-" indicates pro-Republican score on indices of partisan attitude.

* Cell size too small for specification.

effects, while comparisons across the tables tell us something about the effects of party identification and attitudes toward party on voting for U.S. senatorial candidates.

For example, Democratic party identifiers who favorably assessed Democratic domestic policies gave Democratic incumbent senators 93 per cent of their votes, while they gave only 83 per cent of their votes to Democratic challengers running against Republican incumbents. Similarly, Republican party identifiers who were distinctly pro-Republican in their attitudes toward partisan domestic policies cast 97 per cent of their votes for Republican incumbents; however, where Democrats held incumbency status, this vote drops to 88 per cent Republican.

The entries in these tables show a slight increase in incumbency effects when party identification and evaluations of party across these two dimensions are nonsupportive of each other. As an approximate measure of incumbency effects, one might take the difference between the percentages Democratic for respondents voting in Democratic incumbent Senate races and for those voting in Republican incumbent Senate races. For Democratic party identifiers, the difference in percentages is noticeably greater when evaluations of party domestic policies conflict with party identification than when they support party identification. For Democrats who favor their own party's domestic policies, the difference in the Democratic vote percentages for Democratic incumbents and Democratic challengers is 10 per cent, compared to an 18 per cent difference for Democrats opposed to their party's domestic policies. Similarly, the largest difference in percentages among Republicans occurs when domestic policy evaluations conflict with party identification: a 9 per cent difference in the supportive context; a 14 per cent difference in the conflicting context. Thus, when the two motivations are conflicting, there is a slightly greater tendency to resolve that conflict in favor of the incumbent.

Some Comparisons with Gubernatorial Voting

An assessment of factors affecting voting in gubernatorial elections is hampered by the scarcity of data points. Only two of the presidential election studies since 1956 have ascertained each respondent's choice for governor. A pyramiding of the 1964 and 1968 studies, in which the gubernatorial vote question was included, does not yield a sample of sufficient size to achieve control conditions similar to those of

Table 1, Table 2, or Figure 1. Nonetheless, some illustrative comparisons are possible.

Incumbent governors in the states are often thought to be a breed apart. Especially in recent times, the governor as chief administrator has borne the stigma of increasing tax rates in support of expanding social services, urban needs, and so forth. In light of these unique state issues and their natural connection with the performance of incumbent governors, it is often suggested that the effects of basic party loyalties are much reduced in contests for state governorships. Tables 3 and 4 show the percentage voting Democratic in gubernatorial and senatorial elections, respectively, by party identification for the 1964, 1966, 1968, and 1970 elections. Whatever effects particular statewide issues and candidates have, they are not sufficient to reduce substantially the voter's basic ties to his party.

Nonetheless, comparisons of the two tables suggest some differences in the two kinds of contests. The estimates for off-year elections, 1966 and 1970, are most helpful here because they are free of obvious contaminating effects of presidential election years. At first glance, the ties with party appear somewhat weaker for gubernatorial voting than for senatorial voting. Note, for example, that 98 per cent of the strong Democrats actually voted Democratic for U.S. senator while only 86 per cent did so for governor. Closer inspection reveals, however, that for both 1966 and 1970, the senatorial vote is more Democratic across categories of party identification than is the gubernatorial vote. While difficulties of cell size preclude a partitioning of respondents by type of incumbency pattern, other scattered data suggest a possible explanation for the observed pattern. In 1970, eighteen Democratic incumbent senators sought re-election, but only six Democratic incumbent governors did so. The situation was the reverse for Republican incumbents: only ten Republican incumbent senators sought re-election while seventeen Republican incumbent governors did so. Thus, what we are observing here is a probable inflation of the Democratic vote for senator across categories of party identification due to the outnumbering of Republican incumbents by Democratic incumbents. Similarly, a corresponding deflation of the Democratic vote for governor occurs, probably as a result of the more numerous Republican incumbent gubernatorial candidates. It should further be noted that for strong Democrats voting in nonincumbent contests in 1970, the Democratic vote percentage for governor increases to 97 per cent ($N = 41$) and the

Table 3. Percentage Voting Democratic for Governor by Party Identification
1964, 1966, 1968, 1970

	Strong Democrats	Weak Democrats	Independents	Weak Republicans	Strong Republicans
1964	88% (155) ^a	70% (115)	59% (96)	22% (55)	5% (57)
1966	86 (107)	65 (130)	44 (111)	13 (88)	4 (71)
1968	83 (88)	73 (86)	37 (82)	25 (40)	*
1970	86 (133)	72 (120)	42 (149)	23 (99)	1 (77)

^a Entries in parentheses are total *N*'s on which percentages are calculated.

* Cell size too small for specification.

■ Democratic vote percentage for senator decreases to 96 per cent (*N* = 23).

My tentative conclusion is that, in the generic sense, ballots in state elections are no less a function of the voter's identification with his political party in one kind of contest than in the other. However, a difference in the constitutional facts of life for the two kinds of contests does tend to produce two patterns at variance. Since many state governors are limited to single terms by their state constitutions, nonincumbent contests are more common in gubernatorial than in senatorial elections. Of the three kinds of contests examined here, the most frequent kind of senatorial contest during the last two decades has been that involving a Democratic incumbent and a Republican challenger. On the other hand, nonincumbent and Republican incumbent contenders are more prevalent in gubernatorial contests. The result

is that senatorial voting tends to be somewhat more Democratic across categories of party identification than does gubernatorial voting. Scattered data do suggest, however, that for nonincumbent contests, both gubernatorial and senatorial voting exhibit about the same level of basic party voting.

Relative Accountings In Incumbent and Nonincumbent Contests

The importance of candidate incumbency pattern has been stressed as a source of partisan defection and as a means of overriding overt negative reactions to party, as well as reinforcing positive ones. Those who retain favorable images of the Democratic party with respect to various population groups are more likely to disregard that assessment by voting Republican if the Republican candidate happens to be the incumbent. Little has been said

Table 4. Percentage Voting Democratic for U. S. Senator by Party Identification
1964, 1966, 1968, 1970

	Strong Democrats	Weak Democrats	Independents	Weak Republicans	Strong Republicans
1964	90% (210) ^a	83% (174)	59% (173)	27% (125)	7% (111)
1966	92 (63)	68 (82)	43 (72)	9 (44)	7 (44)
1968	90 (156)	75 (162)	44 (172)	21 (122)	9 (89)
1970	98 (114)	85 (111)	62 (146)	23 (93)	8 (63)

^a Entries in parentheses are total *N*'s on which percentages are calculated.

about the contest in which neither candidate is an incumbent. Specifically, we have not been free to examine the monotonicity in these relationships by the kind of graphical analysis of Figure 1; since the number of senatorial contests in which neither candidate is an incumbent is generally small, resulting sample sizes under control conditions become unmanageable.

Conclusions about the two kinds of incumbent races would suggest that voting in the nonincumbent contest should be more a function of attitudes toward party than it should in the incumbent races, since incumbency status itself does seem to override overt negative reactions to party. The hypothesis is that gubernatorial voting and senatorial voting are best accounted for by attitudes toward party in the nonincumbent races and less so in the two types of incumbent contests. It is in the nonincumbent contest that the vote is more free to fluctuate as a direct function of attitudes toward party, relatively unencumbered by the blocking effect of candidate incumbency status.

This formulation was prompted by some initial comparisons with presidential voting. For example, for respondents who cast both presidential and senatorial votes in 1968, the magnitudes of the multiple correlation coefficient for the regression of the vote on the four dimensions of partisan attitude is substantially greater for presidential voting (0.689) than for senatorial voting (0.527). Similar estimates for presidential and gubernatorial voting are 0.668 and 0.473, respectively. The candidate incumbency election context probably has a good deal to do with that difference. The question that naturally arises, however, is what effect incumbency has at the presidential level. This kind of effect may be somewhat attenuated for presidential voting, in comparison to statewide voting. Since information penetration throughout the electorate is likely to be more extensive for presidential candidates than for candidates at other levels, most voters will at least have heard the names of the presidential candidates. The lower salience levels associated with state politics suggest that candidate familiarity will be less extensive in these contests.¹¹

The bar graph in Figure 2 displays the multiple correlation coefficients resulting from the

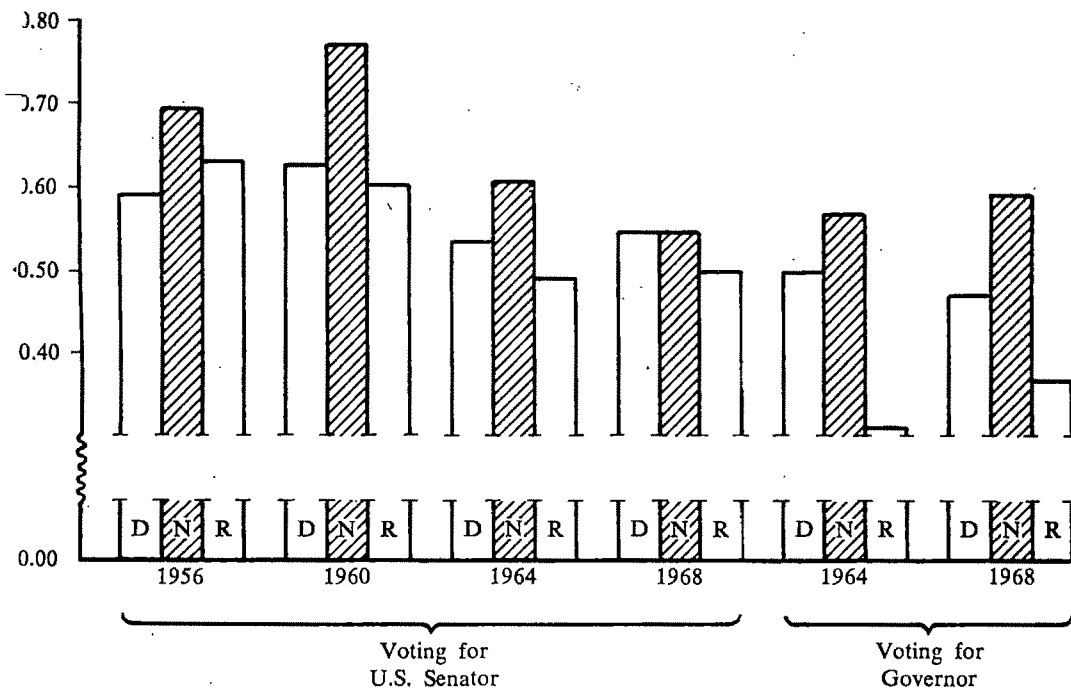
conception of the senatorial vote and the gubernatorial vote as linear functions of the four dimensions of partisan attitude. Those coefficients were computed separately for respondents voting in each of the three types of state elections: (1) Democratic incumbent opposed by a Republican challenger; (2) neither candidate holding incumbency status; and (3) Republican incumbent opposed by a Democratic challenger.¹² In each case except the Democratic incumbent Senate races of 1968, the multiple correlation coefficients computed for respondents voting in nonincumbent contests exceed those for either type of incumbent contest.¹³ Thus, voting in both gubernatorial and senatorial contests appears to be a more direct function of attitudes toward party in the relatively "pure" nonincumbent contests in which incumbency effects cannot attenuate the fundamental connection between partisan evaluations and partisan choice.

The frequency with which state electorates divide their partisan majorities between different parties for different offices is often taken as evidence that the effects of party identification are attenuated in contests for state office. The evidence presented thus far does not support such a hypothesis. After all, state electorates are but subsets of the national electorate. Given the fragmentary nature of the voter's knowledge about contests for the presidency, which typically achieve greater information penetration throughout the electorate than do contests for lower level offices, it is unlikely that the voter would discard what is for him a shorthand, judgmental device such as party identification in favor of careful assessments of statewide issues and candidates. We find no evi-

¹² The coefficients are grouped by year rather than by election type. For any particular election, the vote may not be adequately accounted for because of some unmeasured factor peculiar to that election year. Thus, rather than examining whether the magnitudes of all R's for nonincumbent contests exceed those for all incumbent contests, the question is asked whether, for each election year, the R for nonincumbent contests exceeds the R's for the incumbent contests.

¹³ Correlation coefficients computed across different sample subsets of varying size may fluctuate purely as a result of changing magnitudes of the variances of the independent variables. For excessively small N's, the correlation may be quite low as a result of a reduction in variance. It should be noted that there is no correlation between the size of the subsamples and the magnitude of these multiple correlation coefficients. In fact, the lowest N's in 1960 and 1964 for senatorial voting are found in the nonincumbent races where the multiple correlation coefficients are highest. However, the exceptionally low R's for 1968 gubernatorial voting in both incumbent races may have been affected, in part, by reduced variances for the smaller subsamples in the incumbent categories.

¹¹ See M. Kent Jennings and Harmon Zeigler, "The Salience of American State Politics," *American Political Science Review*, 64 (June, 1970) 526-535. While the findings question the usual low level of saliency attributed to state politics, the saliency advantage which national-level politics holds over state politics is clear from their presentations.



Multiple correlation coefficients resulting when voting for U.S. Senator and for Governor are treated as linear functions of four dimensions of partisan attitude, by candidate incumbency pattern (1956, 1960, 1964, 1968)
 D: Democratic incumbent
 N: No incumbent
 R: Republican incumbent

Figure 2

dence for the assertion that basic party loyalties hold less potency as a guide for gubernatorial voting decisions than for senatorial voting decisions.

An additional shorthand mechanism, however, does operate on voter decision making in state elections at a level not seen in presidential politics. The incumbency status of statewide candidates frequently serves both as a source of partisan defection and as a replacement for strong feelings toward party. In both senatorial and gubernatorial voting, we are able to account for the partisanship of the vote by attitudes toward party more fully in nonincumbent contests than in incumbent contests. Yet, even in incumbent contests, the effects are quite strong and markedly monotonic.

Voting for Presidents, Senators, and Governors: Ballot Splitting Among State Electorates

While the evidence in the preceding section lays some necessary empirical groundwork, it is only suggestive in explaining why election results for statewide contests often appear so dif-

ferent from presidential vote totals in those states. I am inclined, however, to discard the view that the elector's basic party identification does not guide the voting decision in subnational elections. Nor is it suggested that the more changing attitudes toward the Democratic or Republican parties exhibit much attenuated effects in statewide voting. Yet the test of whether a contextual phenomenon such as alternate incumbency patterns (e.g., Democratic incumbent senator and Republican incumbent governor) have the effect of propelling those vote proportions in opposite partisan directions ought to be a good deal more stringent. We begin with the view that whatever aggregate patterns prevail, their counterparts ought to be grounded somewhere in individual voter decision making. Thus, any explanation about why state electorates divide their partisan majorities between different parties should begin with the more fundamental question of why individuals divide their votes between those parties at different levels of government.

Angus Campbell's and Warren Miller's treat-

ment of the motivational factors behind straight- and split-ticket voting is perhaps the best known work on the subject.¹⁴ Their findings indicate that strength of partisan attachments, ballot form, presidential candidate and issue partisanship all affected the level of split-ticket voting in the 1956 presidential election. What is missing, however, is a comparable concern for the specification of the partisan direction of the split-ticket vote for other offices. From that analysis, it is evident that the presence of an extremely popular presidential candidate will induce defections from the opposite party in voting for president. Yet this is only one of several kinds of partisan defection which might occur. The presidential vote often will be a party vote with other factors at the state level producing the defection there. An understanding of the problem of split-ticket voting is equally dependent on the specification of the partisan direction of the split-ticket vote at the state contest level as it is at the presidential level.

What must accompany the restatement of the problem is a change in the form of the dependent variable used by Campbell and Miller. The categories of that dependent variable indicated merely the presence or absence of straight- or split-ticket voting for various national, state, or local contests. Once we ask the question of the partisan direction of the split-ticket vote at other levels of government, it becomes necessary to identify precisely the *offices* for which respondents split their ballots. For example, the Campbell and Miller category, "voted straight except for Senator or Congressman," becomes nearly unmanageable. Since the guiding interest of this project lies in the reconciliation of disparate partisan majorities for state and presidential contests, I shall confine my attention to the network of split-ticket patterns for president, U.S. senator, and governor for which the partisanship of the vote was ascertained.

The explanation of the partisan direction of a split-ticket vote necessarily conjures up a model of opposing forces. The voter's identification with his political party will work against propensities which might lead him to split his ballot at any level. But his evaluation of the presidential candidates may be reinforcing of that predisposition to cast a straight partisan ballot; or, it may increase the tendency to split that ballot when displeasure with the presidential candidate of his own party becomes severe.

In the previous section, the voter's tendency to approve of incumbent officeholders was shown to have a measured effect on partisan voting beyond normal partisan attachments. Thus, it is natural here to conceive of a separate force, which again may reinforce or may oppose normal partisan predispositions. The following hypotheses specify the kinds of patterns anticipated:

Hypothesis 1: When the incumbency status of the senatorial candidate and presidential candidate partisanship support partisan allegiance, the result is a straight-ticket vote for the senatorial and presidential candidates of the voter's own party.

Hypothesis 2: When presidential candidate partisanship supports partisan allegiance but senatorial incumbency status conflicts with both, the level of straight-ticket voting is reduced. The level of split-ticket voting for the presidential candidate of the voter's own party and the senatorial candidate of the opposing party increases.

Hypothesis 3: When senatorial incumbency status supports party identification but presidential candidate partisanship conflicts with both, the level of straight-ticket voting is reduced. The level of split-ticket voting for the senatorial candidate of the voter's own party and the presidential candidate of the opposing party increases.

Hypothesis 4: When the incumbency status of the senatorial candidate and presidential candidate partisanship both conflict with party identification, the tendency to vote a straight ticket against the voter's own party increases.

Hypothesis 5: The level of straight-ticket party voting increases as the strength of party identification increases.

In testing these hypotheses, I have used the combined sample of respondents—1956, 1960, 1964, 1968. Presidential candidate partisanship, of course, involves the assessment of the two presidential candidates.¹⁵ Thus, it is natural to think, not of the level of support or opposition for one candidate, but rather of the net distance between the two candidates based upon the respondent's evaluation of each. Presidential candidate partisanship, then, reflects a net difference between the voter's evaluation of one candidate and his evaluation of the other. The seven-category party identification code was retained since the typical problem of small cell size among leaning partisans was alleviated by use of the pyramided sample.

The dependent variable was constructed by matching straight- and split-ticket voters on

¹⁴ Angus Campbell and Warren E. Miller, "The Motivational Basis for Straight and Split Ticket Voting," *American Political Science Review*, 51 (June 1957) 293-312.

¹⁵ The procedure for constructing these indices of presidential candidate appeal is the same as that for constructing the four indices of partisan attitude, except that the search is made for free-answer content suggesting a reaction to the personal qualities of the candidates.

party identification. The relationships were first examined separately for Democratic and Republican party identifiers in order to be certain that the process was not fundamentally different for either. Then, for the dependent variable, strong Republicans and strong Democrats who, for example, voted a straight ticket for their own party are combined. The four possible combinations are listed in Table 5. The independent variables were recoded on the basis of their supportive or conflicting relationship to party identification.

It is clear from the entries in Table 5 that for those respondents who favorably assessed their own party's presidential candidate and who voted in states where their own party's senatorial candidate held incumbency status, the result is largely a straight-ticket vote. Indeed, only 5 per cent of the strong partisans cast anything other than a straight party ballot. As we move down the table from the completely supportive context to various combinations of conflict and support, straight-ticket voting decreases from the completely supportive context in most cases.

The fundamental question of partisan direction of the split-ticket vote focuses on which category of the dependent variable benefits most from the decrease in the straight-ticket vote. For those respondents who favored their party's presidential candidate but were voting in state contests in which the opposite party's senatorial candidate held incumbency status, the prevailing alternative to the straight-ticket vote is a ballot split for the party presidential

candidate and against the party senatorial candidate. Similarly, when the senatorial incumbent is the candidate of the respondent's own party but the respondent negatively evaluates his party's presidential candidate, the typical choice beyond the straight ticket is the partisan consistent vote for senator and the partisan defective vote for president. Finally, when both presidential candidate partisanship and senatorial candidate incumbency are conflicting with party identification, the alternative to the straight-ticket, party-oriented vote is a straight-ticket vote for the opposing party.

One cannot fail to note, however, the continuing effect of party identification. The insulation from these conflicting forces which partisan attachment provides is clearly demonstrated by the high level of straight-ticket party voting among strong partisans in each of the conflict situations. Also, straight-ticket party voting does decrease as strength of party identification decreases. As a further indication of the accuracy with which these conflict situations predict the split-ticket vote, the percentages recalculated in the two middle categories of Table 5 for split-ticket voters only reveal that fully 100 per cent ($N = 37$) of the strong partisans split their ballots in the predicted direction. While these results are more striking, it seems preferable to retain the straight-ticket voters, since the four-category dependent variable may represent more accurately the range of possible voter alternatives.

Hypotheses 1-4, with appropriate modifications, have been tested for gubernatorial voting

Table 5. Straight- and Split-Ticket Voting for President and U. S. Senator
by Type of Conflict/Support Condition
(Pyramided Sample)^a

Presidential Candidate Partisanship	Senatorial Incumbency Status	Strength of Party Identification	Straight R's own Party	Split: R's own Party for Pres., opp for Sen.	Split: R's own Party for Sen., opp for Pres.	Straight opposite Party	(N)
supports	supports	strong	94%	3%	1%	1%	(361)
		weak	91	5	1	4	(254)
		leaning	78	13	2	7	(105)
supports	conflicts	strong	86	10	0	4	(249)
		weak	76	14	5	5	(200)
		leaning	63	31	0	7	(108)
conflicts	supports	strong	61	0	25	14	(55)
		weak	31	4	39	26	(122)
		leaning	40	5	45	10	(23)
conflicts	conflicts	strong	67	7	10	17	(42)
		weak	42	6	18	35	(113)
		leaning	34	9	25	32	(47)

^a Italicized entries are those predicted by Hypotheses 1-5 to contain the largest concentration of respondents.

as well. Since only two elections—1964 and 1968—were available for analysis, the problem of cell size required that we combine strong, weak, and leaning partisans. The entries for senatorial voting were recomputed, eliminating strength of partisanship, for ease of comparison. The results are presented in Table 6 and Table 7. The increases along the diagonal are again clearly evident for gubernatorial voting. In the lower portions of Tables 6 and 7, the matrices were reduced by eliminating the first row and column for each of the two upper displays. Since party identification will naturally provide a good deal of insulation against the tendency to split one's ticket, the success or failure of these predictions can be identified more readily by first eliminating the partisan, completely consistent, straight-ticket voters. We then ask: Given that some kind of defection from party has occurred, how well can that defection pattern be identified? The heavy concentration along the diagonals of these reduced matrices is good evidence of the sources of that defection.

Split-Ticket Voting in Off-year State Elections

When the gubernatorial candidate of one party and the senatorial candidate of the opposite party are both successful in a given state election, we have reason to believe that those successes are tied to their status as incumbents. Just as candidate incumbency status tells us something about the partisan direction of the split-ticket vote in presidential-senatorial and presidential-gubernatorial voting, ballot splitting between different partisan gubernatorial and senatorial candidates in off-year elections should be a function of the supportive or conflicting juxtaposition of party identification and candidate incumbency status. Prior research suggests that ties with party will be stronger in

nonpresidential elections, regardless of which incumbency pattern prevails.¹⁶

Both hypotheses find support from the evidence in Table 8. The data in that table are drawn from a pyramiding of the off-year election samples, 1966 and 1970, and a pyramiding of the 1964 and 1968 presidential year samples. The concentration of respondents in the first column of each table depicts the strong ties with party in spite of the various conflicting incumbency patterns which might be brought into play. Across these different supportive and conflicting election contexts, the level of straight-ticket party voting for gubernatorial and senatorial candidates is consistently greater in the off-year elections than in the presidential election years.

Also, the concentration of the second largest percentages along the diagonals of the tables indicates that conflicting incumbency patterns do give partisan direction to gubernatorial-senatorial split-ticket voting in both off-year and presidential elections. The exception, however, is found in the final row of the table, which contains respondents for whom both gubernatorial and senatorial incumbency conflict with party identification. Apparently the nuances of the split-ticket vote for senator and governor in this completely conflicting context are a good deal more subtle than can be identified here.

The specification of the incumbency context of state elections has been useful in delineating the partisan direction of the split-ticket vote in state and national elections. If we treat incumbency theoretically as a contextual factor, then we have, in effect, perfect measurement of the concept. A more satisfying theoretical treatment, however, would view the actual effect of this contextual phenomenon on split-ticket voting as a result of several distinct components of

¹⁶ Angus Campbell, "Surge and Decline."

Table 6. Straight- and Split-Ticket Voting for President and Governor by Type of Conflict/Support Condition (Pyramided Sample: 1964, 1968)

Presidential Candidate Partisanship	Gubernatorial Incumbency Status	Straight R's own Party	Split: R's own Party for Pres., opposite for Gov.	Split: R's own Party for Gov., opposite for Pres.	Straight opposite Party	(N)
supports	supports	82%	13%	1%	5%	(136)
supports	conflicts	73	21	1	5	(124)
conflicts	supports	35	8	43	14	(37)
conflicts	conflicts	46	3	17	34	(35)
Reduced Matrix						
supports	conflicts		78%	4%	19%	(33)
conflicts	supports		12	66	22	(24)
conflicts	conflicts		6	31	63	(19)

**Table 7. Straight- and Split-Ticket Voting for President and U. S. Senator
by Type of Conflict/Support Condition
(Pyramided Sample)**

Presidential Candidate Partisanship	Senatorial Incumbency Status	Straight R's own Party	Split: R's own Party for Pres., opposite for Sen.	Split: R's own Party for Sen., opposite for Pres.	Straight opposite Party	(N)
supports	supports	91%	5%	1%	3%	(705)
supports	conflicts	78	16	2	5	(549)
conflicts	supports	40	3	36	21	(184)
conflicts	conflicts	45	7	18	30	(196)
Reduced Matrix						
supports	conflicts		70%	9%	22%	(121)
conflicts	supports		5	60	35	(110)
conflicts	conflicts		13	33	55	(107)

individual reaction—candidate familiarity, approval of the status quo, and so forth. Other measurements, presently unavailable, would have allowed for a sorting out of these various components of the incumbency advantage. In one sense, however, the results are even more striking in light of the imperfection of the measure. We are able to specify with some success the partisan direction of various split-ticket voting patterns in the states without the slightest hint of individual voter response to the qualities of those candidates.

Decomposition of the Split-Ticket Vote for President and U.S. Senator

Although substantial effects of candidate partisanship and incumbency have been observed, the conflicting context which gives rise to the split-ticket vote might not be very widespread. It is necessary to consider the relative concentration of split-ticket voters in each of the supportive and conflicting situations—i.e., for turning the problem around and computing column proportions. Without this kind of reverse

formulation, substantial effects of an independent variable might be observed when a given set of events occurred; yet the frequency of occurrence may have been small, leaving a rather high level of split-ticket voting unaccounted for. This kind of reverse decomposition will have obvious implications for the observed pattern.

After isolating only split-ticket voters, decomposing that split-ticket vote means forming nine categories by taking every possible combination of senatorial incumbency and presidential candidate partisanship. The coding of a respondent as consistent, neutral, or inconsistent on presidential candidate appeal and on senatorial incumbency status is governed by whether his vote for each of those two offices was (1) consistent with the partisan direction of the motivation, (2) unaffected since the motivation was neutral, (3) inconsistent with the partisan direction of the motivation.

In the first category found in Figure 3, both entries are "consistent." This means that the respondent split his ballot in such a way that (1)

**Table 8. Straight- and Split-Ticket Voting for U. S. Senator and Governor
by Type of Conflict/Support Condition**

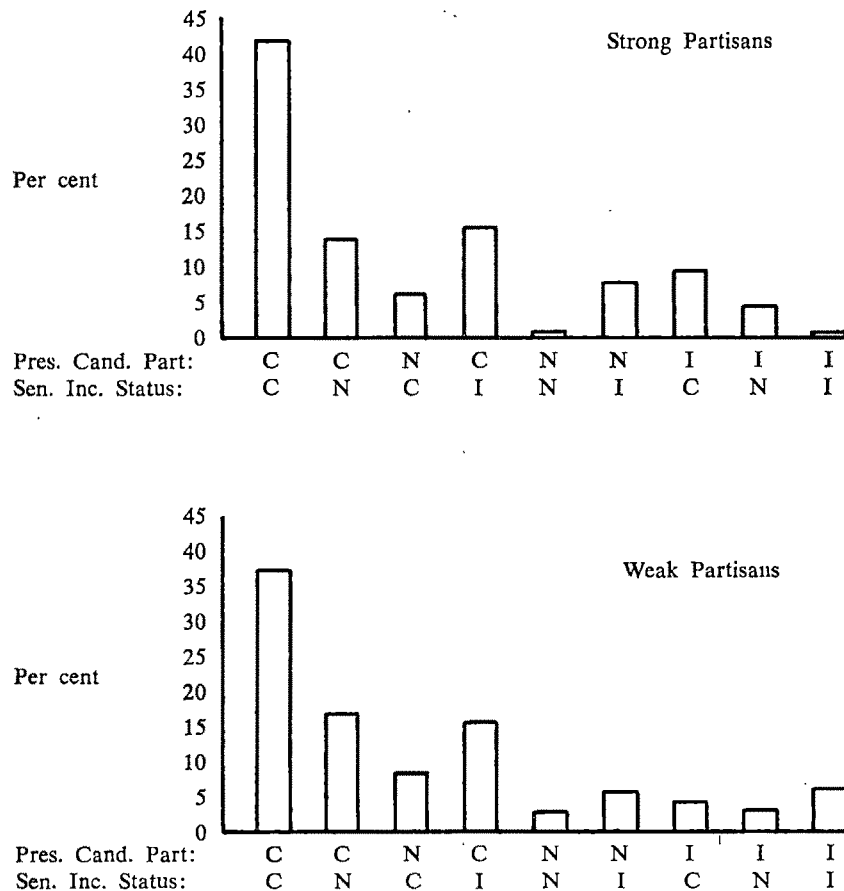
Senatorial Incumbency Status	Gubernatorial Incumbency Status	Straight R's own Party	Split: R's own Party for Sen., opposite for Gov.	Split: R's own Party for Gov., opposite for Sen.	Straight opposite Party	(N)
Nonpresidential Elections (Pyramided Sample: 1966, 1970)						
supports	supports	74%	7%	12%	5%	(82)
supports	conflicts	63	33	0	5	(40)
conflicts	supports	69	5	17	9	(42)
conflicts	conflicts	65	16	7	12	(99)
Presidential Elections (Pyramided Sample: 1964, 1968)						
supports	supports	69%	17%	11%	4%	(54)
supports	conflicts	59	32	4	6	(111)
conflicts	supports	57	6	26	12	(51)
conflicts	conflicts	87	4	4	4	(23)

in voting for president, he voted for the party whose presidential candidate he favored; and (2) in voting for senator, he voted for the other party's senatorial candidate, who happened to be the incumbent. Thus, his split-ticket vote is consistent by both criteria in the sense that he split the ballot in the partisan direction predicted. If the conflict situations identified can account for a substantial portion of the split-ticket vote, then the heaviest concentration of respondents should be found in the first category, with the remaining eight categories quite sparsely represented.

Figure 3 displays the decomposition graphically. Regardless of strength of partisan attachments, the split-ticket vote falls substantially into the first category. Thus, of the nine possible cells through which the split-ticket vote might be dispersed, the one category which simply sets senatorial incumbency status and presidential candidate appeal in conflict contains nearly half of that split-ticket voting. Actually, the second and third categories are not disconfirming of the hypothesis since, in each

of those, one of the two independent variables (presidential candidate appeal or senatorial incumbency status) favors neither party, while the other remains consistent with the predictions. Note, however, that the fourth category which contains split-ticket voters inconsistent on senatorial incumbency status, is somewhat large—a probable result of the inability to specify even to the slightest degree these respondents' reactions to the personal qualities of the candidates for senatorial office.

The implications for divergent aggregate patterns are evident. The relatively common occurrence of state electoral divisions of partisan majorities between different partisan candidates—presidential, gubernatorial, or senatorial—finds its individual level counterpart in split-ticket voting. When presidential candidate partisanship conflicts with basic party attachments, defections from party are induced in presidential voting. Similarly, the common tendency to approve of incumbent candidates brings about a higher level of partisan defection in statewide voting. The decomposition indicates that about

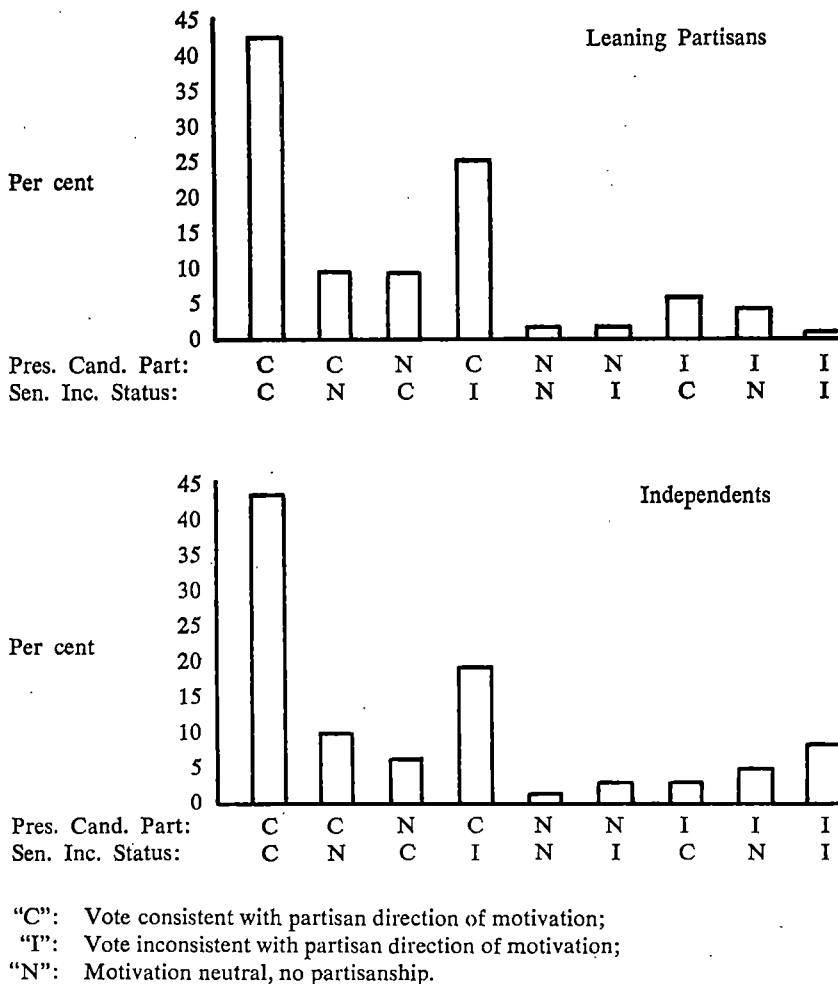


half of the ballot splitting in contests for the presidency and U.S. Senate seats follows just this kind of pattern.

Summary and Conclusions: Implications for Normative and Empirical Theories of Voting

That voters tend to approve of an incumbent officeholder, often in contrast to negative evaluation of the incumbent's party, has some implications for normative theories of voting. Philosophical treatises of the first half of the nineteenth century place an unparalleled stress upon the wisdom of the populace in the form of Jacksonian democratic theory. Yet all propositions of a given normative theory are never

internally consistent with one another on all counts. Barriers to succession for statewide office is a case which highlights that inconsistency. The Jacksonians sought to democratize the governing process thoroughly by maximizing the number of different individual citizens actually participating in that process. A certain inconsistency resulted, however, from the standpoint of the individual voter. If voters in democratic political systems are presumed to have the necessary knowledge and judgment to arrive at informed decisions, then the presence or absence of an incumbent officeholder on the ballot ought not to sway that decision in the slightest. In effect, the range of alternatives be-



Decomposition of the split-ticket vote for president and U.S. senator showing per cent of all split-ticket voters who split ballot in ways either consistent, neutral, or inconsistent with presidential candidate partisanship and senatorial incumbency pattern, by strength of party identification (Pyramided sample)

Figure 3

comes restricted, with the implication that electoral decisions will not be based upon the soundest of judgments when incumbent officeholders are allowed to seek re-election. The restrictions on gubernatorial succession, persisting in some states from the 1820s to the 1970s, imply just such a reservation about the qualifications of individual voters.

To the extent that the motivation behind restrictive succession laws was the suspicion that voters would be more inclined to vote simply on the basis of incumbency, it was not unrealistic. Neither is it a particularly happy conclusion for normative theorists. Findings of an earlier decade concerning party identification and voting brought about a revolt of the normativists who complained that the prevalence of straight party voting in a vacuum of issue awareness was not the occasion for relaxing the standards of normative democratic principles.¹⁷

Party voting, however, is not necessarily inconsistent with the image of the informed voter, prevalent in classical normative theory. Most of our respondents do give at least one response about the two parties suggesting that they associate various good and bad conditions with the presence of those parties in government. While identification with party does not mean that the voter has much of a grasp of specific party policies, it does typically reflect more than blind identification with a party name. In this sense, widespread party voting need not be abhorrent to normative theorists.

The prevalence of incumbency effects cannot be so easily reconciled with tenets of normative voting theory. One might assert that just as party identification is a useful, shorthand device for making decisions in the absence of considerable information, so too is the predisposition to vote for the incumbent officeholder as long as the voter is moderately satisfied with things as they are. Yet these findings do not suggest exactly that. Recall that even for respondents located at the most anti-Democratic point on the scales of partisan attitude, the level of Democratic voting is much higher for Democratic incumbents than for Democratic challengers. Therefore, even the most excessive displeasure with the politics and governing of one party can be overcome partially when that party's candidate happens to be the incumbent officeholder. Candidate incumbency status is

certainly a shorthand guide to political decisions, but its widespread impact upon voting in state elections runs counter to the tenets of classical democratic theory.

The implications for empirical theories of voting are best set out in the context of the seeming paradox in American federalist politics which served as the starting point of this analysis: Basic party loyalties have been shown to have pervasive effects in the arena of national presidential politics; yet election campaigns in the American states often culminate in partisan electoral decisions which are quite inconsistent with the presidential election results in those respective states. Does this mean that party loyalty gives way on a large scale to issues peculiar to each state as a guide to subnational electoral choice? The evidence presented in this paper suggests that the answer is no. The widespread occurrence of aggregate partisan disparities in state election results does not imply necessarily a fundamentally different decision-making process for the individual. The predisposition to vote for one party by virtue of the voter's identification with that party pervades decision making in contests for state governorships and U.S. Senate seats. In addition, the assessments of party, which are less stable over time, contribute measurably to the successes and failures of senatorial and gubernatorial candidates from election to election, as is the case for presidential voting.

The observation concerning disparate partisan majorities for different office contests is not without merit, however. Certainly some contextual factor operating in different partisan directions in different states is a likely source of divergent aggregate patterns between state and national contests. One such factor has been identified as the candidate incumbency pattern prevailing in each state. When the partisanship of the incumbent candidate for governor or U.S. senator runs counter to the partisanship of the individual voter, defection from party is more likely to occur.

I have attempted to specify the partisan direction which the split-ticket presidential-senatorial or presidential-gubernatorial vote would take by examining the conflicting or supportive relationship of two motivations—presidential candidate partisanship and the tendency to approve of incumbent officeholders—to party identification. The spot-welding effect of a completely consistent relationship among these three phenomena is clear in the high level of straight-ticket, party voting. As either is brought into conflict with party identification, however, split-ticket voting increases in the cat-

¹⁷ The arguments against the modification of normative theory on the basis of empirical findings are made by several writers in *Apollitical Politics: A Critique of Behavioralism*, ed. Charles A. McCoy and John Playford (New York: Thomas Y. Crowell Company, 1967), especially pp. 185-219.

egory anticipated. Also, nearly half of the total number of split-ticket voters are found to be concentrated in the one conflicting category in which the partisan directions of the split-ticket vote are consistent with the equivalent partisan directions of presidential candidate appeal and senatorial incumbency status.

While examinations of aggregate election results might have suggested that contemporary theories of voting behavior in presidential elections reveal little about decision making for voters in state elections, the testing of some of those theoretical propositions at the individual level does not bear out that view of state elections. However, the modification of those propositions by the introduction of one important concept—candidate incumbency status—is clearly in order for state electoral contests. The treatment of that phenomenon as a systematic force operating on voters in state elections both improves the applicability of those theoretical propositions and resolves to some extent the apparent disparities between election results in the states and prior theoretical expectations about individual voter behavior.

Appendix

In the survey studies of voting behavior over the past two decades, the Survey Research Center interview schedules have sought repeatedly the respondents' evaluations of the Republican and Democratic parties through free-answer questions:

I'd like to ask you what you think are the good and bad points about the two parties. Is there anything in particular that you like about the Democratic (Republican) party?

Is there anything in particular that you don't like about the Democratic (Republican) party?

Now I'd like to ask you about the good and bad points of the two candidates for President. Is there anything in particular about (Democratic presidential candidate) (Republican presidential candidate) that might make you want to vote for him?

Is there anything in particular about (Democratic presidential candidate) (Republican presidential candidate) that might make you want to vote against him?

That much of the richness in these responses has remained through the coding process is evidenced by nearly one thousand possible code categories to which a single response might be assigned. As many as five separate responses are coded. The utility of these indices of parti-

san attitude has been evident in reports on several occasions.

The first step in the construction of the indices is to extract responses on the basis of content and positive or negative evaluation: (1) response reflecting the respondent's favorable attitude toward the Democratic party as it relates to various population groups; (2) response suggesting a positive reaction to Democratic domestic policies; (3) responses suggesting a positive reaction to Democratic foreign policies; and (4) response indicating a positive assessment of the Democratic party's performance as managers of government. For each respondent, the total number of positive responses to the Democratic party is obtained within each of the four subject matter areas. Comparable summed quantities are formed for negative reactions to the Democratic party, positive reactions to the Republican party, and negative reactions to the Republican party. The positive evaluations of the Democratic party and the negative evaluations of the Republican party are then summed, yielding a scale on which high values indicate a strongly pro-Democratic, anti-Republican attitude. A similar computation is performed on the pro-Republican, anti-Democratic responses, yielding a scale on which high values suggest a strong, pro-Republican, anti-Democratic attitude. The final step is simply the subtraction of the latter from the former, the result being a scale for which high values indicate a net favorable Democratic attitude for each of the four content areas. The computations are performed separately for each of the four dimensions of partisan attitude:

w_{ij} = pro-Democratic response for j^{th} response of individual i

x_{ij} = pro-Republican response for j^{th} response of individual i

y_{ij} = anti-Democratic response for j^{th} response of individual i

z_{ij} = anti-Republican response for j^{th} response of individual i

v_i = index of partisan attitude for individual i

$$v_i = (\sum_{j=1}^4 w_{ij} + \sum_{j=1}^4 z_{ij}) - (\sum_{j=1}^4 x_{ij} + \sum_{j=1}^4 y_{ij}),$$

where initial 0,1 scoring indicates presence or absence of response.

These indices have been constructed according to the criteria used by Stokes.¹⁸ A specification of which open-ended responses are coded into each partisan attitude would require too much space for presentation here.

¹⁸ Donald E. Stokes, "Some Dynamic Elements of Contests for the Presidency."

The Impact of Party on Voting Behavior in a Nonpartisan Legislature*

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In partisan legislatures, party has been found to be the most important variable influencing the behavior of legislators.¹ This paper will examine a nonpartisan legislature to attempt to determine the relative impact of party and constituency as reference groups in cuing legislative behavior where the influence of party is institutionally minimized.

Earlier studies, all done in partisan legislatures, have produced a variety of hypotheses concerning the impact of party, constituency, and other reference groups on the voting behavior of the legislator. Here, we will be concerned with those hypotheses that relate to party and constituency.

Hypothesis 1: Party is the most important variable influencing legislative voting behavior. This finding has been consistent whether the analyst has used party majority votes, cluster bloc techniques, scaling techniques, or factor analysis to examine this behavior.² For legislators, party serves as the most important reference group in organizing their behavior, and less explicitly, as the chief device in producing

relatively structured voting situations in the entire legislature much of the time.

1a: The importance of party is due to the independent effect of party leadership and party organization within the legislative body.³ Thus, those who have found this relationship indicate that party is a manifest cue for legislative behavior. Party leaders have rewards and punishments to offer, and, especially in state legislatures, this provides a powerful incentive to go along.

1b: Party serves in a more indirect way to cue voting behavior. Legislative votes are party votes not so much because of party leaders and organizational discipline, but because party is a catch-all for sets of beliefs and attitudes, and these attitudes differentiate Democrats and Republicans. Or, even more indirectly, party influences voting behavior on a psychological level, so that self-identification as a Democrat or Republican produces distinct voting patterns, once a "Democrat" or "Republican" "right" vote is cued by some party leader or cohort.⁴

1c: Party is an important variable in explaining voting behavior because it reflects differing constituency interests. LeBlanc found in an investigation of 26 state senates that senators were more loyal to their parties in those states where constituency differences between the parties were greatest. Where parties did not have differing constituency bases, party voting was

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¹ See, in addition to the works cited below, the bibliography found in Duncan McRae, *Issues and Parties in Legislative Voting* (New York: Harper & Row, 1970), pp. 299-309.

² Julius Turner, *Party and Constituency*, revised edition by Edward V. Schneier, Jr. (Baltimore: Johns Hopkins Press, 1970); W. Wayne Shannon, *Party, Constituency and Congressional Voting* (Baton Rouge: Louisiana State Press, 1968); Lewis Froman, *Congressmen and Their Constituencies* (Chicago: Rand McNally, 1963); John G. Grumm, "A Factor Analysis of Legislative Behaviour," *Midwest Journal of Political Science*, 7 (November, 1963), 336-56; David B. Truman, *The Congressional Party* (New York: John Wiley & Sons, 1959).

³ Duane Lockard, *New England State Politics* (Princeton: Princeton University Press, 1959); Frank Sorauf, *Party and Representation* (New York: Atherton, 1963); V. O. Key, *Parties, Politics and Pressure Groups* (New York: Thomas Y. Crowell, 1964), chap. 24.

⁴ For a review of ideological and policy differences between party leaders and followers, see Herbert McClosky, "Issue Conflict and Consensus among Party Leaders and Followers," *American Political Science Review*, 54 (June, 1960), 406-27; Angus Campbell, Philip E. Converse, Warren E. Miller, and Donald Stokes, *The American Voter* (New York: John Wiley & Sons, 1960) is a standard source on the importance of party identification on voting behavior. For a review of the influences on legislative voting see Malcolm Jewell and Samuel Patterson, *The Legislative Process in the United States* (New York: Random House, 1966), especially pp. 430-431.

less frequent.⁵ Shannon, at one point, argues that the differing constituency bases of the two congressional parties would seem to account for many of their differences,⁶ and Flinn notes that constituency is the basis of party cohesion.⁷

Hypothesis 2: Constituency is related to legislative behavior, independent of its relationship to party. Froman found that among members of each party those from districts that were socioeconomically "liberal" voted more "liberally" than those from "conservative" districts.⁸ McRae found that Republicans from low-income districts and Democrats from high-income districts tend to vote less often with their party majority in the legislature than do legislators from "typical" party districts.⁹

Hypothesis 3: Party voting, even within legislatures where it is common, varies from issue area to issue area, as does constituency-related voting. Some issues, such as elections and reapportionment, find high degrees of party voting in most legislatures, other issues such as civil rights find little party voting in most. Issues such as local government and apportionment are highly related to constituency characteristics in many legislatures, other issues such as business regulation are not.¹⁰

⁵ Hugh LeBlanc, "Voting in State Senates: Party and Constituency Influences," *Midwest Journal of Political Science*, 13 (February, 1969), 33-57; for other works dealing with influences on voting in legislatures see Samuel Patterson, "Dimensions of Voting Behavior in a One Party State Legislature," *Public Opinion Quarterly*, 26 (Summer, 1962), 185-200; Duncan McRae, *Dimensions of Congressional Voting* (Berkeley: University of California Publications in Sociology and Social Institutions, 1958).

⁶ Shannon, *Party, Constituency and Congressional Voting*, p. 131.

⁷ Thomas A. Flinn, "Party Responsibility in the States: Some Causal Factors," *American Political Science Review*, 58 (March, 1964), 60-71.

⁸ Froman, *Congressmen and Their Constituencies*, pp. 92-93. Clarence Stone, however, shows that the voting behavior of representatives of different parties representing the same constituency is very different. "Inter-Party Constituency Differences and Congressional Voting Behavior: A Partial Dissent," *American Political Science Review*, 57 (September, 1963), 665-66.

⁹ Duncan McRae, Jr., "The Relation Between Roll Call Votes and Constituencies in a Massachusetts House of Representatives," *American Political Science Review*, 46 (December, 1952), 1046-55.

¹⁰ See Jewell and Patterson, *Legislative Process in U.S.*, "Voting in State Senates," p. 41. See also Wayne Francis, *Legislative Issues in the 50 States: A Comparative Analysis* (Chicago: Rand McNally, 1967); Charles W. Wiggins, "Party Politics in the Iowa Legislature," *Midwest Journal of Political Science*, 11 (February, 1967), 86-97. Warren Miller and Donald Stokes found great differences among broad issue areas in the correspondence between constituency opinion and voting of congressmen. See, "Constituency Influence in Congress," *American Political Science Review*, 57 (March, 1963), 45-56.

In the nonpartisan setting, it was assumed that party would not be as important a factor in influencing voting behavior as in the partisan setting. The first question to be examined is the extent to which party influences voting behavior. If party is not influential, the second question, in light of the hypotheses, is whether any other factors, especially constituency, will assume the function that party performs in a partisan legislature. Will constituency groups serve as references for legislative voting behavior? Will any other factor or set of factors replace the influence of party in structuring legislative behavior? These questions relate then, both to individual behavior and the structure of voting as a whole.

Using a nonpartisan legislature to test some assumptions about voting in a partisan legislature is not as perverse as it may seem. We may view such an analysis of a nonpartisan legislature as a sort of control for investigations of partisan legislative behavior. It offers a unique way to test the above hypotheses, since the removal of the influence of party should have different effects on voting behavior in the legislature depending upon what is most important, i.e., whether it is the leadership and organization of party (hypothesis 1a), and psychological or ideological functions of party (hypothesis 1b), party as a reflection of constituency (hypothesis 1c), or constituency itself (hypothesis 2). If it is the first of these variables, then minimizing party influence should have an extreme effect on voting. One would expect voting that is highly disorganized in terms of stable coalitions and random in terms of issue positions, with legislators looking for cues in all sorts of different places.¹¹ If hypothesis 1b, 1c, or 2 is correct, then we might expect the nonpartisan voting patterns to be both structured and explainable, either by party or constituency factors.

On a secondary level, we will determine whether different issues are characterized by different patterns of voting behavior, as suggested by hypothesis 3, or whether voting in a nonpartisan legislature will be so different on the group level that all issue areas in a nonpartisan setting are characterized by similar voting patterns.

The Setting

The nonpartisan body that will be examined is the Nebraska unicameral, one of two nonpartisan state legislatures. While in the Minnesota nonpartisan legislature there appears to be

¹¹ Patterson, "Voting Behavior in a One Party State Legislature."

some association between Liberal and Conservative caucuses and the state party structure,¹² in the Nebraska legislature few traces of party organization are evident. This has been the conclusion of past investigators of the legislature, and despite increasing pressure from the party organizations to revert to a partisan system, still seems to be true.¹³ The perception of the lack of partisan conflict is also held by the legislators themselves. When asked to characterize types of conflict in the legislature, only two said that party conflict was important. This number, about 4 per cent of the membership, can be contrasted with the responses obtained by Wahlke et al. in New Jersey (85 per cent responded that party conflict was important), Ohio (59 per cent), California (26 per cent) and even Tennessee (17 per cent).¹⁴ Thus, even though all the legislators will admit to a party preference¹⁵ and many have held party offices, this partisanship is not perceived to carry to the floor of the legislature.

Methodology

The data analysis in this paper utilizes Guttman-scaling and regression techniques. Every

¹²Theodore Mitau, *Politics in Minnesota* (Minneapolis: University of Minnesota Press, 1960).

¹³One observer writes that "neither seniority, partisanship, nor gubernatorial influence are of major significance in the power struggle in Nebraska's unicameral structure" (Richard Marvel, "Decision Making in the Nebraska Unicameral Legislature," [Ph.D. thesis, Department of Political Science, University of Nebraska, 1966] pp. 76-77). Interviews with high state party officials and associates of the governor confirm the impotence of these forces in dealing with what one former governor called "an army of all generals." Power within the chamber is diffuse; as the unicameral legislature is now organized, the Speaker is elected by the full membership on a secret ballot. Some disagreement exists as to the degree of power held by the Speaker; in the past it tended to be an honorific post, but is now becoming a more important position. See Robert Sittig, "Unicameralism in Nebraska, 1936-1966," *State Government* (Winter, 1967), 38-41, and Marvel, Chapter 4. Membership on the Committee on Committees is another potential source of power; the Chairman of this committee is elected by the full house, but the other members are chosen by caucuses of members from different regions of the state. Committee chairmanships are not awarded on the basis of seniority necessarily, and the normal term of office for chairmen is two terms. For an overview of the Nebraska political system see Bernard Kolasa, "The Nebraska Political System: A Study in Apartisan Politics" (Ph.D. thesis, Department of Political Science, University of Nebraska, 1968).

¹⁴John Wahlke, Heinz Eulau, William Buchanan, and Leroy Ferguson, *The Legislative System* (New York: John Wiley and Sons, 1962), p. 425.

¹⁵Interviews with legislators indicate that all are quite willing to identify themselves with one of the two major parties. These interviews are from an unpublished survey conducted by Eric H. Carlson.

roll call in each of five sessions of the Nebraska legislature (1927, 1937, 1947, 1959, 1969) with at least 10 per cent dissent was examined.¹⁶ The number of usable roll calls in each session varied from 131 in 1937 to 282 in 1969 (see Table 1). The 1927 session was a partisan session; it was examined in order to give some indication of whether the findings for the nonpartisan legislature were unique, or whether they reflected a long-standing political style in Nebraska. Longitudinal study of the 35 years since the establishment of the nonpartisan was undertaken so as to detect the development, if such exists, of some factor around which behavior in the legislature is organized.

The method used to convert the several hundred roll calls to a usable number of dimensions was Guttman scaling.¹⁷ Some researchers divide bills a priori into sets that are seemingly scalable because of similar manifest content—tax bills, civil rights bills, local government bills, or whatever.¹⁸ Guttman scaling, however, can provide a means to combine data in a way not dependent on the researcher's arbitrary classification of the issues. The use of the Q-matrix gives the investigator a method to compare all roll calls with all other roll calls in order to ascertain whether these roll calls will scale.¹⁹ Thus, it is the *legislator's* issue dimensions, not the *researcher's* predetermined ones, that are measured. Taxation bills may scale with education, civil liberties with business reg-

¹⁶The selection of years was not as random as it may seem. Originally the design called for the use of ten-year intervals, beginning with 1927 and continuing until 1967. Altering the selection to the years listed, however, had the advantage of allowing an examination of two nonpartisan sessions with Republicans serving as governor, two with Democrats. While this distribution was later found to have no apparent effect, at the time the data were collected, it seemed reasonable to assume that legislative behavior might be different during the tenure of governors from the majority party of the state.

¹⁷For an explanation of Guttman scaling and its uses in legislative analysis see McRae, *Issues and Parties*; George Belknap, "A Method for Analyzing Legislative Behavior," *Midwest Journal of Political Science*, 2 (1958), 377-412; Charles D. Farris, "A Scale Analysis of Ideological Factors in Congressional Voting," *Journal of Politics*, 20 (May, 1958), 308-38; Aage Clausen and Richard Cheney, "A Comparative Analysis of Senate-House Voting on Economic and Welfare Policy, 1953-1964," *American Political Science Review*, 64 (March, 1970), 138-52.

¹⁸See Patterson, "Voting Behavior in a One Party State Legislature," and Farris, "Scale Analysis," for example.

¹⁹Clausen and Cheney, "Senate-House, Voting on Economic and Welfare Policy"; Duncan McRae, "A Method of Identifying Issues and Factions from Legislative Votes," *American Political Science Review*, 59 (December, 1965), 909-26.

Table 1. Some Attributes of Voting in each Session*

	1927 Partisan	1937	1947	1959	1969
		----- Nonpartisan -----			
Total <i>N</i> of bills with more than 10% dissent	217	131	145	172	282
<i>N</i> of bills scaling	100	64	63	61	75
Percentage of bills scaling	47%	48%	43%	35%	27%
<i>N</i> of scales	16	9	9	9	12
Average <i>N</i> of bills per scale	6.5†	7.6	7.9	7.6	7.3
Range in <i>N</i> of bills per scale	4-11	5-14	4-21	5-17	6-11

* There were 43 senators in 1937, 1947, and 1959, 49 in 1969, and 33 in the 1927 Senate.

† Some bills appear in more than one scale, thus, this figure does not necessarily equal $\frac{N \text{ of bills}}{N \text{ of scales}}$.

ulation, and so on in combinations that would not be predicted a priori by the investigator.

An obvious problem of this analysis is that conceivably, given the number of roll calls being analyzed, some bills will scale together simply by chance.²⁰ In this analysis, 59 empirical scales with a minimum of four bills each were found in the five sessions. Each of the scales had a minimum CR of .92. All but four of these scales had a reasonably consistent content, not necessarily completely homogeneous, but related in some way. These four scales of four and five roll calls each were removed from the analysis on the assumption that they were chance collections of bills.²¹ The remaining 55 scales each had a CR of .94 or better, and no roll call in each had more than 15 per cent error. For the purposes of further analysis, one additional scale in the 1959 session was deleted because it essentially duplicated another scale. The remaining 54 scales, then, form the basis of the following discussion. The issue areas of

the scales in each year are described in the Appendix.²²

These scales delineate the issue areas in each session. To determine patterns of voting behavior within each issue area a further procedure was used. Each legislator received a scale score for each scale. Each of these sets of scores (54 in all) served as the dependent variable in a stepwise regression equation, with various constituency, personal, and party characteristics as independent variables. These characteristics are: party identification of the senator, his age, and his education; the proportion of his district that is urban; a measure of the industrialization of his district; and the partisan preference of his district as expressed in its vote for president in the election prior to the legislative session analyzed (1924, 1936, 1944, 1956, 1968).²³

²² Since the scales were built from the universe of roll calls and not from predetermined "issue" areas, the content of the scales is not always as precise as one might expect or wish. Thus it was found that education and local government issues are often mixed together in one scale, regulation and taxation issues seem to scale together frequently, as do taxation and appropriation bills in some cases. In the following discussion the scales will be labeled by their primary substantive content.

²⁰ McRae recognizes this problem and comments that "empiricism cannot be unbounded . . . otherwise it might lead to such absurdities as the selection of an arbitrary group of legislators simply to eliminate 'error' votes or the assemblage of a meaningless collection of roll calls for which the legislators in question had scalable votes" (McRae, "Identifying Issues and Factions," pp. 911-12). Roll calls with lopsided distributions of votes are particularly likely to scale randomly. For example, a vote with only 10 per cent dissent, even if all the dissents are "errors," would still empirically scale with any other set of votes if 10 per cent error is the only criterion for inclusion in the scale.

²¹ Some arbitrariness probably entered into the decision to exclude the four scales. They had three characteristics which led to the decision: slightly lower CR's than the other scales, lopsided vote distribution in most roll calls comprising the scales, and roll calls unrelated to each other. These scales were removed from the analysis because it was assumed that they were not measuring purposive behavior, only statistical artifact.

²³ Other district and personal characteristics were used in preliminary analyses. These included such variables as the size of the legislator's hometown and his occupation, the income level and racial composition of senatorial districts, and the gubernatorial vote in the senatorial districts. The six variables chosen for this analysis were chosen because, on the whole, they explained the most variance in scale scores and minimized the problem of multicollinearity. Surprisingly, the gubernatorial preference of senatorial districts was even less related to the senator's party identification than was presidential vote. The statistics used in the analysis are taken from *The Fifteenth Census of the United States*, vol. 3, pt. 2, *Population* (Washington: U.S. Census Department, 1933); *County and City Data Book* (Washington: U.S. Census Department, 1956, 1962, 1967); *Nebraska Blue Book* (Lincoln, Nebraska: Nebraska Legislative Council, 1928, 1938,

Stepwise regression finds the variable with the strongest zero-order correlation with the dependent variable, then holds this variable constant while finding the variable with the highest first-order correlation, etc. Thus, it gives us a powerful and parsimonious means to examine the relative impact of a number of independent variables on a dependent variable.²⁴ In the past, researchers have been somewhat unwilling to use this kind of multivariate technique to analyze roll-call behavior. But in the case where the aim is description of a population, rather than inference from a sample to a population, the stringent limitations on the use of these techniques need not be met.²⁵ Zavoina and McKelvey have discovered some problems in using ordinal Guttman scales as a dependent variable in a regression model, but suggest that this procedure is conservative from a hypothesis-testing standpoint.²⁶ Jackson has successfully used multiple regression techniques to predict senatorial voting in the U.S. Senate.²⁷ Regression analysis, then, appears to be the most useful and practical tool to deal with the particular problems in examining multiple causation in legislative voting.

The methodology of the paper thus provides for two steps: first to construct from roll calls in each session Guttman scales that comprise the totality of all scalable bills meaningful in terms of issue content; and second, to attempt to explain voting on these scales in terms of the independent variables of constituency, party, and two personal characteristics.

Structure in Roll-Call Voting

One might hypothetically construct two polar kinds of roll-call behavior on the part of legislatures as wholes. On the one hand, all votes taken in the legislature could fall along a single dimension, and hence would form one scale. This sort of situation occurs where par-

ties are tightly disciplined and the party leadership enforces this discipline on every vote. Voting in this dimension would be entirely explainable by party alone. Conceivably, however, one might find this comprehensive unidimensionality explainable by some other factor than party—if all votes were urban-rural votes, for example. This situation we might term a completely structured pattern of voting.²⁸

At the opposite end of this scheme would be the situation in which *no* set of four or more votes scaled. This would indicate that no group of four or more votes formed a single dimension; almost every vote would have unique coalitions of legislators on each side. This situation presumes almost no party regularity at all, nor any regularity that might be defined by constituency, or interest group influence, or any other sort of regional, or personal, or informal pressures. This type of voting might be termed "random," or "unstructured."

Probably no state legislature or house of Congress would fall at either extreme consistently, although Carlson found that the New Jersey Senate approached the former pattern in at least one session.²⁹ In the Nebraska Senate, as Table 1 indicates, the proportion of scalable bills decreased over the time period examined, from almost one-half in 1927 and 1937 to slightly more than one-fourth in 1969. Thus, in 1969, almost three-quarters of non-unanimous votes displayed no consistent relationship in voting patterns to even three other votes. The smallness of this proportion of scalable bills indicates a good deal of apparent randomness in voting behavior on the part of the legislators, since consistency of voting, whether cued by party, constituency influence, interest-group pressure, committee structure or whatever, would be indicated in clusters of bills. This seeming randomness has been commented upon by Rall, who notes that the nonpartisan legislature has "... a tendency to fragment into small blocs, some of which contain only a single member."³⁰

This pattern may be compared with that observed in other legislative bodies. Farris asserted that for the U.S. House, "... Representatives maintain enough consistency in voting behavior ... so that practically all their roll call votes can be classified as part of one scale

1948, 1958, 1966, 1968); *Census of the the Population, 1950*, 2 (Washington: U.S. Government Printing Office, 1952), pt. 27, *Nebraska*; Richard Scammon, *America at the Polls* (Pittsburgh: University of Pittsburgh Press, 1965).

²⁴ See for example, William L. Hays, *Statistics* (New York: Holt, Rinehart, and Winston, 1963), especially chap. 15.

²⁵ Hays, pp. 509-10.

²⁶ W. J. Zavoina and R. McKelvey, "A Statistical Model for the Analysis of Legislative Voting Behavior," paper presented at the 65th Annual Meeting of the American Political Science Association in New York, September 2-6, 1969 as quoted in John E. Jackson, "Statistical Models of Senate Roll Call Voting," *American Political Science Review*, 65 (June, 1971), 455.

²⁷ Jackson, "Statistical Models of Senate Voting," pp. 451-70.

²⁸ See also Eric H. Carlson, "Five State Legislatures: A Comparative Analysis of Roll Call Voting Patterns," paper presented at the Rocky Mountain Social Science Association Meeting, Colorado State University, May, 1971.

²⁹ Carlson.

³⁰ Frank Rall, "Nebraska: Sons of the Pioneer," in *States in Crisis*, ed. James Reichley (Chapel Hill: University of North Carolina Press, 1964), p. 254.

or another."³¹ Jackson's analysis of the 1961 and 1962 sessions of the U.S. Senate seem to indicate that more than 65 per cent of the bills scaled.³² Carlson, in his analysis of nine houses of five legislatures (New Jersey, Michigan, Ohio, Tennessee, and California) found that the percentage of bills scaling ranged from 100 per cent in the highly partisan New Jersey Senate to 29 per cent in the California House; only three houses had less than 40 per cent scalable bills (both California houses and the Tennessee House). Patterson found in the one party Oklahoma House that about one-half the votes there were scalable (62 of 127) and that another 17 per cent formed what he called "quasi-scales."³³

Comparing results from the Nebraska Senate to results from these other analyses, one notes that voting in the most recent sessions of the Nebraska unicameral (and these sessions are more directly comparable in terms of time) is more unstructured, in terms of scalability of roll calls, than it is in any other legislative body. This is true even in comparison with the one-party Oklahoma legislature. California provides a close rival to Nebraska's looseness of structure. The California legislature in the late 'fifties (when the analysis was done) represented a very weak partisan legislative system. The 1927 and 1937 sessions of the Nebraska Senate, on the other hand, conform more closely in degree of structure to some other legislative systems. The 1927 system was partisan, and while the 1937 system was nonpartisan, about 80 per cent of its members had served in the partisan legislature of 1935 or earlier.³⁴ One might hypothesize, then, that some behavior patterns from the partisan era lingered on among these members.³⁵ At any rate, while it is somewhat dangerous to generalize from five data points, there does seem to be a trend to-

ward decreasing structure in legislative behavior since the establishment of the nonpartisan system in Nebraska. This trend provides some evidence for the hypothesis that it is the organization and discipline of party leadership, over a series of votes that provide voting cues for legislators' overall voting patterns. It appears that, in Nebraska at least, no other cue or reference group was as salient for legislators. Thus, in the most recent session, without party organization and leadership, most votes are unrelated to other votes.

Explaining Voting Behavior

The preceding section has described the lack of structure in voting behavior in the Nebraska unicameral legislature. This section will deal with the scalar analysis of that part of the voting behavior that was structured. It has been observed that, in general, neither party nor constituency nor any other system of reference groups has been able to serve as a cue consistently enough to present a structured voting situation. In those voting situations that *are* structured, however, what are the relative influences of party and constituency?

Consider several assumptions reflecting the applicability of the original hypotheses to the nonpartisan legislature:

(1a.) We will assume that if neither party nor constituency is related to voting (r^2 and R^2 will be used as measures of this relatedness), then this fact constitutes evidence in favor of hypothesis 1a: the importance of party is due to the independent effect of party leadership and party organization within the legislative body. That is, in the nonpartisan legislature, neither party identification nor constituency influence have been removed, but party leadership and organization have been. If the hypothesis is true, then, we would expect party to explain a small amount of the variance observed, i.e., have a small r^2 .

(1b.) If party is strongly related to voting and unrelated to constituency, then this would provide evidence for the strength of hypothesis 1b: Party is an important cue because of psychological or ideological identification, not because of leadership and discipline, or because of its reflection of constituency characteristics.

(1c.) A finding that party identification is strongly related to voting and to constituency would provide some support for hypothesis 1c: Party is an important variable in explaining voting behavior because it reflects differing constituency interests. If this condition holds, however, it would not logically rule out either hypothesis 1b or 2.

(2.) If constituency is strongly related to

³¹ Farris, "Scale Analysis," p. 314.

³² Jackson, "Statistical Models of Senate Voting," p. 458.

³³ Carlson, "Five State Legislatures," Table 3; Patterson, "Voting Behavior in a One Party State Legislature."

³⁴ This lack of turnover may be explained by noting that the new nonpartisan legislature had 43 members, while the two houses of the 1935 partisan legislature had over 100 members.

³⁵ It might be noted here that another evidence of the lack of structure in the Nebraska legislature is the small size of those scales that do exist. Those bills that scale formed many small dimensions rather than one all-encompassing one as cited in the New Jersey legislature. This particular attribute of the scales is not directly comparable with most other Guttman analyses because in many cases no attempt is made to aggregate votes from different issue areas, or in some cases different bills. However, Patterson ("Voting Behavior in a One Party Legislature"), without attempting to aggregate issue areas, found his scales averaged nine votes per scale.

voting, but not to party, then evidence is provided for hypothesis 2, that it is largely constituency differences, not party, that produces party divisions in legislative voting.

In sum, we will examine the relationship found among party identification of the legislator, his voting behavior, and the characteristics of his constituency in order to attempt to confirm or disconfirm the accuracy of the hypotheses stated above.

We may eliminate assumption 2 immediately. In none of the sessions examined, neither partisan nor nonpartisan, did the party identification of the legislator relate strongly to the urban character of his district or to its degree of industrialization. In only one session did the party of the legislator correlate highly with the partisan vote in his district (Table 2). Thus, in the Nebraska legislature, party is not a reflection of constituency characteristics, at least those we have measured.

Table 2. Relationship of Legislator's Party Identification to Some Characteristics of his District*

Year	Urban Character of District	Degree of Industrialization of District	Presidential Vote of District
1927	.14	.16	.15
1937	.17	.21	.10
1947	.15	.14	.47
1959	.13	.23	.07
1969	.11	.21	.26

* Measure used here is simple r .

Table 3 illustrates the relationship, expressed in r^2 , between party identification and each scale for each year, as well as an average overall scale. Probably the most striking fact about the table is the lack of relationship between party and voting. In only 6 of the 38 nonpartisan scales is r^2 even equal to .09, and in only 9 more is it even a minuscule .04. While the preceding section illustrated fairly well that neither party nor any other group served as a potent organizing force in the legislature, these relationships indicate that in general party has almost no influence on the voting behavior of the legislators, even in these structured votes.

In the partisan Senate, the r^2 between party and scale scores is more than .09 in about twice as many scales proportionally (five of sixteen), and its value is between .04 and .09 in about the same proportion. Thus we can see a decline in the influence of party on voting from the partisan to the nonpartisan Senates, although even in the former the relationship was hardly overwhelming. A possible cause for this lack of

influence may have been the Progressive movement: George Norris was a Nebraskan, and his influence, even at this time, may have been important. Certainly, it may logically be postulated that if Nebraska had had a strong party system in the '20s and '30s, it would have been impossible to establish the nonpartisan legislature. Thus it is not surprising to find a less than strong relationship between party and voting, even in 1927.³⁶

Obviously, then, party is not strongly related to voting in even these selected roll calls, with a few exceptions. This seems to confirm hypothesis 1a, that party leadership and organization are the crucial factors producing party voting. Although the difference in this legislature between the partisan years and nonpartisan is not large, if we acknowledge that the 1927 legislature was an example of a weak party system, then the differences seem more striking.

Turning to the variance explained by the urban or industrialized character of the constituency or its voting preferences, Table 4 indicates that one or more of the constituency variables are more important explanatory variables than party in about half of the partisan scales, but in at least three-quarters of the scales in each of the nonpartisan sessions.

While these zero-order correlations between constituency and voting are, overall, higher than the correlations between party identification and voting, most of them are not extremely high. Thus, not only party is largely unrelated to most of the scales in the nonpartisan legislature, but the constituency variables are also largely unrelated. These findings do not support hypothesis 2 in any large way. We can detect a slight shift in emphasis from party to constituency as the more important set of variables between 1927 and 1937 and since then. But the impact of these constituency variables in no way substitutes for the impact of a party in a strong partisan system, nor on the whole do these variables even explain as much variance in the nonpartisan system as in the weak Nebraskan partisan system.³⁷

³⁶ An example of the weakness of the 1927 party system and the possible impact of the Progressive movement is the election scale. This scale was comprised of bills relating to the establishment of a primary election system; this policy, of course, was one advocated strongly by the Progressives and other reformers of the era. The voting, as Table 3 illustrated, was little related to party, even though election issues are one of the kinds of issues most often found to be related to party.

³⁷ Farris found 27 scales in the House. He found correlations with party very high; only three scales had simple 4's of less than .30.

Table 3. Relationship between Party of Legislator and Scale Score on each Scale

1927	r ²	1937	r ²	1947	r ²	1959	r ²	1969	r ²
Taxation	.62	Soc. Welf.	.38	Salary II	.09	Elections	.41	Salaries	.24
Tax. & Reg.	.31	State Inst.	.23	Approp'n	.06	Regulation	.15	Bus. Regln.	.14
Judiciary	.20	Bus. Reg.	.05	Education	.04	Tax. I	.08	Civil Lib.	.07
Local Gov.	.18	Rev. & Tax.	.05	Loc. Govt.	.03	Tax. II	.01	He'th & Wel.	.04
Local Fin.	.10	Priv. Bills	.04	Taxes	.03	Salary	.01	Tax. & Reg II	.04
Elections	.08	Loc. Govt. II	.01	Lic. & Reg.	.02	Local Govt.	.01	Tax. & Reg I	.02
Regulat'n	.08	Regulation	.01	Salary I	.01	Welfare	.00	Bus. Regln.	.02
Salaries	.06	Transport'n	.00	Local Govt. II	.01	Education	.00	Civil Lib.	.01
Tax Lit.	.05	Local Govt. I	.00	Regulat'n II	.00			Lic. & Reg.	.00
Welfare	.04							Lab. & Emp.	.00
Tax. & Reg.	.03							Appropriat'n	.00
Loc. Gov. II	.03							Tax & Apprt'n.	.00
Hghwys & Rds.	.02								
Appropriat'n	.02								
State Inst.	.00								
Agriculture	.00								
Mean r ²	.11		.08		.03		.08		.05

Table 5 is a summary table illustrating the variance explained in each session by the six independent variables. More variance was explained in the single partisan session than in any of the nonpartisan ones, except 1959. If these were correlations between voting choice and personal characteristics on a heterogeneous group of citizens, the correlations would seem moderately high and modestly impressive. Taken as a rough measure of a predictability of voting in a highly institutionalized organizational setting, they seem far less impressive. They seem, in fact, to indicate that voting behavior, even in these sets of roll calls that have displayed some structure, cannot be predicted by party and constituency taken together, along with two salient personal characteristics sometimes thought to be important in voting.³⁸

Unfortunately, these findings are not directly comparable with most other roll-call studies. Most of the analyses measuring the impact of party on voting have used indices of party cohesion, rather than correlational techniques.³⁹

³⁸ Breckenridge, for example, suggests that age groupings might sometimes be a factor in voting alignments. See Adam C. Breckenridge, *One House for Two* (Washington: Public Affairs Press, 1957); the discussions about social class composition of legislators indicate implicitly that social class background affects behavior in the legislature; education is not a perfect measure of social class by any means, but does correlate with it. See Donald Matthews, *The Social Background of Political Decision Makers* (Garden City, N.Y.: Doubleday, 1954).

³⁹ See for example, Turner, *Party and Constituency*; Truman, *Congressional Party*; see also David R. Derge, "Metropolitan and Outstate Alignments in Illinois and

Table 4. Scales in which one of the Constituency Variables Is More Important than the Party Identification Variable

1927	U	I	V*	1937	U	I	V	1947	U	I	V
Local Govt. I	.28	.32		Revenue & Tax	.25	.19		Salary II	.17	.14	.18
Regulation	.13			Private Bills	.12	.12		Appropriat'n.	.23	.32	.10
Salaries	.07			Local Govt. II	.07	.20		Local Govt.		.04	
Tax Lit.	.14	.08		Regulation	.05			Taxes			.12
Welfare	.10		.14	Transportation	.24	.16		Lic. & Reg.	.18	.10	.04
Loc. Govt. II	.11	.11		Local Govt. I	.04	.04	.01	Salary I	.21	.37	.09
Appropriations		.05						Loc. Govt. II	.19	.19	.08
State Institut.	.33	.12						Regulation	.28	.10	.21
Agriculture	.08	.02	.14								
1959	U	I	V	1969	U	I	V				
Taxation I	.18	.07		Health & Wel.			.09				
Taxation II	.17	.14	.04	Tax. & Reg. II		.09					
Salaries	.34	.18		Tax. & Reg. I		.46	.06				
Local Govt.	.37	.20	.10	Bus. Regulation		.09					
Welfare	.41	.22	.02	Civil Lib. II			.03				
Education	.21	.21	.02	Lic. & Regul'n	.02	.30					
				Labor & Empt.	.10	.39	.05				
				Appropriat'n	.01		.01				
				Tax & Approtrn.		.01					

* U = Urbanization, I = Industrialization, V = Vote in Presidential Election.

Table 5. Summary Measures of the Impact of Independent Variables in Explaining Voting Behavior

	Average R ²	Average R	Low R ²	High R ²
1927	.36	.59	.13 Highways	.79 Taxation
1937	.30	.54	.20 Regulation	.50 Social Welfare
1947	.30	.54	.16 Taxes	.52 Spending
1959	.43	.65	.20 Regulation	.61 Welfare
1969	.29	.52	.04 Taxation & Appropriation	.55 Taxation & Regulation

McRae divided his legislators by party before constructing scales;⁴⁰ LeBlanc dealt only with roll calls on which there was party opposition;⁴¹ and Shannon and Froman used techniques that did not really allow them to assess the cumulative impact of both party and constituency in explaining scale score.⁴² However, the variance in scale scores accounted for by party alone in Shannon's analysis seems to be substantial.⁴³ Clausen and Chaney find that in the U.S. House the average variance explained by party alone in their economic and welfare scales is .43 and .31 respectively, and in the Senate .48 and .68. Constituency relationships were not so uniformly high, particularly in the Senate, but indices of blue-collar employment and urbanization correlated moderately with

the welfare scales and minimally with the economic scales.⁴⁴

Since what we are interested in is the relative impact of these independent variables, an attempt was made to summarize the relative impact of these variables over all scales for each session (Table 6). To arrive at a summary score measuring the importance of each variable in the scales of each session, a simple method was used: Each time a variable entered into an equation first (i.e., was the most powerful explanatory variable), it was given a score of six points; if it entered second, it was given a score of five points, etc. The scores were summed for each year and the variables ranked. In addition, Table 6 shows the number of times each variable entered into an equation first in the partisan and nonpartisan scales.

While there is a good deal of variation in the relative importance of these variables from year to year, it seems that the urbanness and degree of industrialization are the most important factors in explaining the overall voting behavior in the nonpartisan scales. They seem more important than party identification and

Missouri Legislative Delegations," *American Political Science Review*, 52 (December, 1958), 1051-65 for an illustration of the use of indices of cohesion on another kind of group.

⁴⁰ McRae, *Congressional Voting*.

⁴¹ LeBlanc, "Voting in State Senates."

⁴² Froman, *Congressmen and Their Constituencies*; Shannon, *Party, Constituency and Congressional Voting*.

⁴³ Shannon, chap. 4.

⁴⁴ Clausen and Chaney, p. 146.

Table 6. Importance of Each Independent Variable in Explaining Voting in the Scales of Each Session

Characteristic	1927	1937	1947	1959	1969	First*	
						Partisan	Nonpartisan
Percentage urban in district	2.5†	2	5.5	1	4	3	11
Percentage industrialized in district*	6	5	1	5	2.5	1	9
Vote of district in last pres. elect.	5	6	4	6	1	3	3
Party identification of senator	1	1	2	4	5.5	4	8
Education of senator	4	3	5.5	2.5	5.5	4	2
Age of senator	2.5	4	3	2.5	2.5	1	5

* Unfortunately, the measures of industrialization vary. In 1959 and 1969 the figure used is based upon percentage of white collar employment in the district; in 1927 and 1937, on the percentage employed in manufacturing; in 1947, on the median years of education in the district.

† The figures represent the rank order of the independent variables in each session as computed by the simple method explained in the text. The figures in the last column represent the number of times that variable was the most powerful independent variable in the regression equation. In other words, in 11 of the 38 equations, the variable of urbanization explained more of the variance than any other variable.

much more important than the personal characteristics of the legislator or his district's presidential preferences. In the partisan legislature, party seems most important, with no clear indication of next rankings. That is, age of the senator and degree of urbanization in the district are second in total points, but the education of the senator and the vote of the district in the last election enter first into more equations than does age.

It seems significant that there is little consistency from session to session in the most important explanatory variable. This finding indicates that no outstanding alignment has been consistent over a period of time. Table 5 seems to indicate that voting patterns shift radically from session to session, with no strong underlying force providing structure for the legislators.

Table 6 treats all scales equally—those in which the R^2 for the six independent variables and scale score is .70, and those in which it is only .05. Since it seems useful to separate for closer analysis the “most important” independent variables in only those scales that are highly explainable in terms of the six given variables, Table 7 sets forth those scales in which the r^2 between the “most important”

variable and the scale scores is at least .20. These were considered the most important scales to examine, because, given the number of scales and the number of independent variables, small correlations are apt to be products of chance; and an independent variable that explains only 5 or 6 per cent of the variance in a scale score, even though it is the “most powerful” variable under consideration, cannot be thought to be shedding much light on voting behavior.

From Table 7 it seems clear that only urbanization and industrialization have much predictive power in explaining voting in the nonpartisan legislature. Party seems important only upon occasion, and one welfare scale in 1969 was most related to the senator's own background. On some occasions, therefore, constituency variables, and even occasionally party variables, do explain a goodly proportion of the voting behavior.⁴⁵

⁴⁵ The 1959 election scale, the scale most highly related to party during the nonpartisan sessions, deals with elections in a very specific, *post hoc* way. Five of the six items in the scale deal with whether the legislature should give sanction to a recount of the 1958 gubernatorial votes. The governor of Nebraska, like all

Table 7. Scales in which Voting Is Relatively Predictable

Year	Scale	Most Important Variable	% Explained by MIV	Beta MIV	R^2 All Six
1927	Taxation and Regulation	Party Identification	32%	.53	.50
	State Institutions	Urbanization	33	.77	.49
	Judiciary	Party Identification	20	.42	.27
	Taxation	Party Identification	60	.71	.79
	Local Government	Industrialization	32	.35	.56
1937	Local Government II	Industrialization	20	.65	.41
	State Institutions	Party Identification	23	.55	.39
	Revenue and Taxation	Urbanization	25	.34	.41
	Social Welfare	Party Identification	38	.65	.50
1947	Licensing and Regulation	Urbanization	28	.46	.36
	Appropriations	Industrialization	33	.47	.52
	Salaries	Industrialization	37	.55	.50
1959	Local Government	Urbanization	21	.29	.45
	Salaries	Urbanization	35	.74	.50
	Education	Urbanization	37	.69	.47
	Public Welfare	Urbanization	41	.76	.61
	Elections	Party Identification	41	.66	.56
1969	Salaries	Party Identification	20	.41	.44
	Health and Welfare	Senator's Education	23	.40	.39
	Licensing and Regulation	Industrialization	30	.77	.38
	Labor & Employment	Industrialization	39	.73	.43
	Regulation and Taxation	Industrialization	46	.83	.55

If we examine the remaining 21 scales in the nonpartisan sessions, and the 11 in the partisan sessions, the distribution of the most important explanatory variables is rather random.⁴⁶ It would not be unfair to speculate, given these random distributions and the small correlations, that the relatedness of the variables is almost a chance artifact of the probability that given enough variables and enough equations, something is bound to correlate with something else.

This perhaps excessively cynical view seems to suggest that in the case of this nonpartisan legislature, the structured voting is either related to differences in the urban-rural nature of the legislator's constituency (urbanization and industrialization measures), or it is not related to any of the variables under consideration.⁴⁷ In the partisan legislature, party identification seems more important, with these constituency variables related to some scales.

One might object to this whole analysis of the impact of constituency on voting in legislatures by noting that the kinds and number of independent variables are limited. We can agree that the kinds of measures concerning the nature of the constituency are crude, yet they do reflect some of the most salient factors about the constituency. While the congressman or legislator is not likely to perceive the constituency in exactly the terms used here, he is equally unlikely to ignore the large groupings of blue-collar workers, low-educated people, farmers, or whatever; these measures do give some indication of the importance of these groups in each district.⁴⁸ A conceivably important factor not considered in this analysis is the legislators' attitudes and sympathies toward interest groups. These attitudes may partially reflect gross constituency characteristics (a per-

his gubernatorial counterparts, is elected on a partisan ballot. Thus, there was a grand opportunity in the legislature for Democrats and Republicans to take sides on an issue of immediate and great partisan concern. While party identification did show more correlation with this set of issues than any other, still only 41 per cent of the variance was accounted for by party.

⁴⁶ Distribution of Most Important Variable in Other Scales:

	Age	Urbanization	Party Identification	Industrialization	Partisan Vote	Education	Total
Partisan	1	2	1	0	3	4	11
Nonpartisan	5	5	4	3	3	1	21

⁴⁷ Francis's more impressionistic conclusion was that in Nebraska the major source of conflict was the urban-rural difference (Francis, *Legislative Issues in 50 States*).

son representing a heavily blue-collar district is not likely to be unsympathetic with labor groups), but may contain dimensions unrelated to these characteristics. Further, the data presented here did not take into account personal attitudes of the legislators toward various economic, political, and social issues. Individual attitudes, of course, may or may not reflect constituency opinion.⁴⁹ However, data on attitudes toward interest groups and toward some economic, social, and political issues were collected for about half of the legislators in the 1969 session. Adding these variables into the regression equations did not significantly improve predictability in voting for even this small group. This is only a small and perhaps misleading bit of evidence, however, since the measures of social and economic liberalism used were rather crude ones.

In terms of individual voting behavior (as contrasted with overall voting structure), if we return to our original hypotheses and assumptions concerning their validity, we find that the data seem to rule out 1b and 1c, because clearly party is not strongly related to voting in any consistent way. The verdict on hypothesis 2 is less clearcut, because in some scales constituency does explain a large amount of the variance, while in others it explains very little. If we remember, however, that a large proportion of the roll calls did not scale at all, then hypothesis 2 does not appear to be strongly supported. In sum, when the nonpartisan legislature is used as a control, the observed lack of both structure and explainability in voting behavior must lead one to conclude that it is the organization and discipline of party leadership that produce strong party voting in legislative bodies.⁵⁰

⁴⁸ For a good discussion of this point see Froman, *Congressmen and Their Constituencies*.

⁴⁹ See Miller and Stokes, "Constituency influence in Congress."

⁵⁰ The causal factors relating to a strong party leadership within the legislature are another question. And, in fact, party leadership may be forthcoming from outside the legislative body. Van der Slik et al. found that voting in one kind of nonpartisan setting, without

party leadership organized formally within the legislative body, could be strongly influenced by partisan preference (Jack Van der Slik, David Kenney, and

Issues and Voting in a Nonpartisan Legislature

One of the consistent findings of those who have examined partisan legislatures, even highly partisan bodies, is that some issues are more highly partisan than others. Less consistent have been the findings about exactly which issues are more partisan. Regional conflict was found in apportionment and local government issues, as well as in issues of constitutional revision, civil rights, and sometimes in social welfare.⁵¹ Jewell found party conflict high in the areas of labor, elections, constitutional amendments, legislative organization, and natural resources; intermediate party conflict was found on appropriations, welfare, state offices, and schools.⁵² Wiggins in Iowa discovered high party conflict on labor, legislative organization, elections, constitutional amendments, and national issues, intermediate conflict on appropriations, welfare, state offices, and schools.⁵³ In the Oklahoma one-party legislature, Patterson found taxation a product of conflict with urban-rural coalitions, elections related to political competitiveness, education to socio-economic constituency variables, and public morals to rural-urban differences.⁵⁴ LeBlanc, in his analyses of state senates, observed that business regulation and health and welfare issues tended to be less partisan than revenue, appropriations, labor, legislative organization, and elections.⁵⁵

We will hypothesize that in a nonpartisan legislature, only those issues that are directly related to the fortunes of the parties and those tied most directly to national issues will be characterized by a high degree of partisan conflict: particularly election issues and certain kinds of social welfare legislation that are the

subject of national controversy. We will further hypothesize that issues relating to local government, agriculture, spending, and taxation will be related to rural-urban differences in constituency.

Table 8 indicates that our hypotheses are only partially confirmed. The social welfare issues in the 1937 scale were related quite directly to Roosevelt's New Deal proposals, and the election scale of 1959 was related quite directly to the fortunes of the two parties (see footnote 45); therefore, the finding that these issues are related to party is expected. The state institutions scale of 1937 is less related to any national issue, although since it dealt with court and prison reform largely, it may be capturing a liberal-conservative or Roosevelt-anti-Roosevelt alignment. The salary scale of 1969 may be viewed as a gubernatorial support series of votes.⁵⁶

Most of the spending and taxation scales that showed any relationships (and this includes the salary scales, and the social welfare scale of 1959 which contained several bills relating to increasing unemployment compensation) were related to constituency. More than half of these bills, however, were not strongly correlated with any variable, nor were most of the local government scales and agriculture related to any variable under consideration.

In sum, the hypotheses about party and constituency conflict are moderately substantiated. Different patterns of conflict occur with different kinds of bills. On the whole, however, party conflict was found only rarely (and two of the four scales on which there was party conflict were found in 1937).

Conclusions and Implications

The above findings indicate that there is relatively little structure in voting in the Nebraska legislature. This suggests not only that party is the most important reference group in structuring legislative behavior but also that in the absence of party, no other kind of reference group serves a similar purpose.⁵⁷ Even though

Samuel J. Pernacciaro, "Roll Call Voting Patterns in the Illinois Constitutional Convention: A Regression Analysis," paper presented at the Midwest Political Science Association Meeting, Chicago, Illinois, April, 1972). This nonpartisan constitutional convention was, of course, operating within the very highly partisan political setting in Illinois; many of the members, although nominally nonpartisan, were responsible to the Democratic organization of Cook County; others had strong party ties to downstate Republican and Democratic organizations. See also Caleb and Janet Clark, "The Impact of Electoral and Party Systems upon Political Conflict in State Constitutional Conventions," paper presented at the 1972 Southwestern Social Science Convention, San Antonio, Texas, March, 1972.

⁵¹ Francis, p. 21.

⁵² Malcolm Jewell, "Party Voting in American State Legislatures," *American Political Science Review*, 49 (September, 1955), 779-80.

⁵³ Wiggins, "Party Politics in the Iowa Legislature," p. 87.

⁵⁴ Patterson, "Voting Behavior in a One Party Legislature," pp. 199-200.

⁵⁵ LeBlanc, "Voting in State Senates," p. 41.

⁵⁶ The Governor, labeled a "progressive" Republican, had in his tenure from 1966 to 1970, embarked upon an effort to modernize both the fiscal structure and the organization of state government of Nebraska. Increasing the salaries of high state officials to make them more competitive with business and other states was one among many examples of what his opponents viewed as wasteful spending. Thus, many of these measures were opposed by the Democrats and were capitalized upon in the successful Democratic gubernatorial campaign in 1970.

⁵⁷ This finding is supported by the relative lack of structure found by Carlson, "Five State Legislatures," in the weakly partisan California legislature of the late

Table 8. Scales: Party and Constituency Conflict

Issue	Number of Scales in Nonpartisan	Scales highly Related to Party, $r^2 \geq .20$	Scales highly Related to Constituency, $r^2 \geq .20$
Social Welfare	4	1 (1937)	1 (1959)
Elections	1	1 (1959)	0
State Institutions	1	1 (1937)	0
Salaries	4	1 (1969)	2 (1959, 1947)
Revenue and Taxation	7	0	3 (1937, 1947, 1969)
Regulation	9	0	2 (1947, 1969)
Local Government	5	0	1 (1959)
Education	2	0	1 (1959)
Highways and trans.	1	0	1 (1937)
Labor and employment	1	0	1 (1969)
Civil Liberties	2	0	0
Agriculture	1	0	0
Private Bills	1	0	0

strong constituency differences exist, and in Nebraska the urban-rural nature of constituency is superimposed on regional differences, constituency is not a strong factor around which voting can be consistently organized.

If we examine those votes that do display structure (that is, those that scale), we find that the majority are not explainable to any large extent by party, constituency, or personal variables. Voting is simply not predictable from the kinds of measures utilized here. Obviously other variables than party identification, rural nature of the constituency, or age enter into a voting decision. Such influences as one's attitude toward an issue, susceptibility to interest group pressure, the legislator's personal desire to do a favor for a colleague—these sorts of influences are not being measured directly by the data at hand, yet they might be real influences on behavior.⁵⁸ Still, these influences are probably not entirely divorced from the constituency and party characteristics we have examined.

What implications does this lack of apparent predictability and order in voting have? One that is immediately apparent concerns the responsibility of elected officials to their constituents. While citizens do not generally think of parties in terms of ideology or policy, most Americans know even less about the policies and platforms of their legislators.⁵⁹ Thus, re-

moving this most salient cue of party identification for most voters would seem to be taking away one means of a rational voting choice, just as removing party organization seems to take away the most salient cues for the legislator.

One of the original motives behind political nonpartisanship was, in Norris's words, to give the people more power in their government by eliminating corruption caused by "crass" political deals and putting government on a business-like basis. It is difficult to see how constructing a setting in which there is no consistency in behavior on the part of representatives and little knowledge on the part of electors can give the people more power. While there are undoubtedly virtues in a nonpartisan system unrelated to the question of responsibility, to that question at least, a nonpartisan legislature seem to be a less than satisfactory answer.

Opinion Quarterly, 26 (Winter, 1962), 531-46. It seems probable that Nebraska voters are neither more nor less informed about politics than their counterparts elsewhere. A survey taken in Lincoln, Nebraska, during the election campaign of 1970 found 56 per cent of the population unable to name their congressman, 32 per cent unable to name their congressman's party (and one can assume that some of the remaining 68 per cent were guessing). Of course, for the segment of the population that is much more aware and informed, even about policy issues, the nonpartisanship of the legislature would be less of a hindrance to some sort of rational voting behavior than for the public at large. For information on this public, see G. R. Boynton, Samuel C. Patterson, and Ronald D. Hedlund, "The Missing Links in Legislative Politics: Attentive Constituents," *Journal of Politics*, 31 (August, 1969), 700-21; and Robert S. Erikson, "The Electoral Impact of Congressional Roll-Call Voting," in *American Political Science Review*, 65 (December, 1971), 1018-32.

1950s and the Tennessee legislature, where again, strong parties did not exist at that time in the legislature.

⁵⁸ Again, for a discussion on the use of constituency data as an indicator of real constituency preferences, see Fröman.

⁵⁹ See Donald Stokes and Warren E. Miller, "Party Government and the Saliency of Congress," *Public*

Appendix

The following table categorizes scales according to their predominant issue area by year:

Issue Areas of the Scales	1927	1937	1947	1959	1969	Total
Revenue, Taxation, or Appropriations	5*	1	2	2	2	12
Regulation	1	1	2	1	5	10
Local Government	3	2	2	1	0	8
Social Welfare	1	1	0	2	1	5
Salaries	1	0	2	1	1	5
Education	0	0	1	1	0	2
Civil Liberties	0	0	0	0	2	2
State Institutions	1	1	0	0	0	2
Highways and Transportation	1	1	0	0	0	2
Agriculture	1	1	0	0	0	2
Elections	1	0	0	1	0	2
Judiciary	1	0	0	0	0	1
Private Bills	0	1	0	0	0	1
Labor and Employment	0	0	0	0	1	1

* The numbers indicate the number of scales in this area.

Foreign Aid and United Nations Votes: A Comparative Study*

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Introduction

Reaction in the United States to the decision of the United Nations General Assembly on the China issue has raised anew the question of whether foreign aid has or has not been, indeed, should or should not be, used as a political instrument by the United States in seeking compliance by other states with its foreign policy positions. According to *The New York Times*, Senator Hugh Scott was "expressing a common resentment" when he observed that "a good many nations we have helped generously with foreign aid over many years have shown a classic lack of appreciation." Similarly, the *Times* reports that Senator J. W. Fulbright "suggested that rather than cutting back on United States contributions to the United Nations, Congress might reduce foreign aid to countries that had voted against the United States position on China."¹

The purpose of this essay is to apply empirical evidence to the "has or has not" portion of the debate. Specifically, its purpose is to examine in a comparative foreign policy framework the relationship between political outcomes in the General Assembly, as represented by roll-call voting agreements across an entire set of roll calls, and foreign aid allocations, one of a myriad of extraorganizational links between UN members. In principle, the inquiry will be concerned with the extent to which foreign aid is an instrument of influence in the General Assembly. In propositional form, we will be con-

cerned with the hypothesis that *the larger the agreement in General Assembly voting exhibited by a developing state with a foreign aid donor, the greater will be the amount of aid it receives from that donor*. At the same time, however, we will also be concerned with the reverse formulation of the proposition, i.e., whether the allocation of donors' aid dollars can be used to describe and explain the voting preferences of developing nations. We will examine the covariation between pairwise voting patterns in the 1963 (Eighteenth) and 1966 (Twenty-first) General Assemblies and the allocation of foreign economic assistance in two three-year periods, 1962-1964 and 1965-1967. Initially attention will be focused on the United States. The results for the U.S. will then be compared with those for other aid donors to ascertain their similarity or dissimilarity. The donors that will be compared with the U.S. are the Soviet "bloc"² and the twelve UN members (other than the U.S.) of the Development Assistance Committee (DAC) of the Organization for Economic Cooperation and Development (OECD).³

² Lack of aid data on a dyadic basis has required the aggregation of the Soviet Union and other Communist nations into a single actor, referred to somewhat unhappily as a "bloc." For purposes of the 1962-1964 aid data, the Soviet "bloc" comprises the Soviet Union, Bulgaria, Mainland China, Czechoslovakia, East Germany, Hungary, Poland, and Rumania. With the exception of Mainland China, the same states comprise the Soviet "bloc" for purposes of the 1965-1967 aid data.

³ Austria, Belgium, Canada, Denmark, France, Italy, Japan, the Netherlands, Norway, Sweden, and the United Kingdom. Australia, though not a DAC member until 1966, is included in the 1965-1967 analyses.

Although the DAC donors and the Soviet "bloc" nations together contribute the vast bulk of bilateral economic aid flows in the international system, several other states do maintain modestly sized bilateral programs, many of them in the area of technical assistance. Finland and New Zealand are among the developed nations maintaining such programs. Spain, Kuwait, Yugoslavia, the United Arab Republic, Nationalist China, Israel, India, Brazil, Mexico, and Venezuela are among the developing nations that reportedly maintain some such program, often in conjunction with a multilateral effort. See Organization for Economic Cooperation and Development, *The Flow of Financial Resources to Less-Developed Countries 1961-1965* (Paris: O.E.C.D., 1967), pp. 59-61; Organization for Economic Cooperation and Development, *Resources for the Developing World* (Paris:

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¹ *The New York Times*, October 27, 1971, p. 16.

The Aid and Voting Nexus

Foreign aid has often been used in studies concerned with general patterns of state behavior in and toward the United Nations.⁴ Little empirical or comparative work, however, has attempted to isolate what role foreign aid may play in the political processes of the organization, or how these processes and their outcomes may influence other types of foreign policy behavior, such as aid allocations.⁵ Yet the implication that aid influences voting behavior, or vice versa, is frequently suggested in the literature on both the United Nations and foreign aid, particularly as it relates to the United States. A brief, suggestive discussion of some of this literature will provide a background for the subsequent empirical analysis.

The suggestion that foreign aid is used as a bargaining tool in securing UN votes is spelled out clearly by Robert O. Keohane in his study, "Political Influence in the General Assembly."⁶ "Certain states in the Assembly are very susceptible to bilateral pressure," he writes, "no matter how subtle its application may be. The more dependent a state is on a greater power for trade, aid, or protection, the more responsive it is likely to be to pressure."⁷ Similarly, he

O.E.C.D., 1970), pp. 289-91; and Organization for Economic Cooperation and Development, *Development Assistance: Efforts and Policies of the Members of the Development Assistance Committee, 1969 Review* (Paris: O.E.C.D., 1969), pp. 60-63. Unfortunately, the data necessary for a systematic analysis of the aid programs of these donor countries comparable to that undertaken for the donors included in the study are not available. In fact, even in the case of the Soviet bloc, as will be noted below, the data are not comparable to those available for the DAC donors, thus leading to the presentation of two separate sets of results. However, the interest usually attached to Soviet aid programs seems to warrant its (their) inclusion.

⁴ See, e.g., Hayward R. Alker, Jr. and Bruce M. Russett, *World Politics in the General Assembly* (New Haven, Conn.: Yale University Press, 1965), pp. 226-27; Hayward R. Alker, Jr., "Dimensions of Conflict in the General Assembly," in *Approaches to Measurement in International Relations*, ed. John E. Mueller (New York: Appleton-Century-Crofts, 1969), pp. 163-65; and John F. Clark, Michael K. O'Leary, and Eugene R. Wittkopf, "National Attributes Associated with Dimensions of Support for the United Nations," *International Organization*, 25 (Winter, 1971), 1-25.

⁵ Two examples of studies that use UN votes to examine aid allocations are Masakatsu Kato, "A Model of U.S. Foreign Aid Allocation: An Application of a Rational Decision-Making Scheme," in Mueller, *Approaches to Measurement in International Relations*, pp. 204-10; and Wynfred Joshua and Stephen P. Gibert, *Arms for the Third World* (Baltimore: The Johns Hopkins Press, 1969), pp. 140-43.

⁶ Robert O. Keohane, "Political Influence in the General Assembly," *International Conciliation*, No. 557 (March, 1966).

⁷ Keohane, p. 19.

notes, "Threats of retaliation of one sort or another—reducing foreign aid, for example—usually need not be made explicit. Often it is sufficient that the smaller state is aware that 'Big Brother' is watching."⁸ Keohane also points out, however, that foreign aid is unlikely to be used as a bargaining tool by all foreign aid donors.

Despite the United Nations' commitment to the use of rational and deliberative procedures, states attempt to coerce one another to support their positions. Yet even the most "realistic" observer of the General Assembly will quickly realize that attempts to achieve results by using extra-parliamentary threats—for instance, the threat to reduce foreign aid—are almost entirely limited to the two great powers. Nowhere is bipolarity more marked.⁹

The use of aid by the United States to influence voting behavior is suggested by Edward S. Mason, who writes that "diplomats are frequently worried by voting records in the United Nations and suggest that an increase in aid to their particular country would improve this record."¹⁰ Lloyd D. Black has been more direct.

⁸ Keohane, p. 19.

⁹ Keohane, pp. 17-18. See also David A. Kay, "Instruments of Influence in the United Nations Political Process," in *The United Nations Political System*, ed. David A. Kay (New York: John Wiley & Sons, 1967), p. 105.

Keohane also suggests that "most decisions of the General Assembly . . . are not important enough to United States interests for it to threaten convincingly to alter the level of aid or support that it is giving, or to exercise other forms of pressure . . . for the sake of gaining a few more votes in the United Nations" (Keohane, p. 20). And he argues that the changing composition of the United Nations has probably made bilateral pressure less important than it was, say, in the 1950s (pp. 19-20).

Still, this caveat does not obviate the likelihood that foreign aid is a significant factor conditioning states' voting behavior, particularly where aid represents part of a general dependency relationship between a developing nation and an aid donor. In discussing the "appeal for good relations" as a form of bilateral pressure, for example, Keohane observes that the threat implicit in this appeal ". . . may become a painfully acute form of pressure if it is exercised by greater powers and directed toward states that depend on them for aid or support" (p. 18).

Jack C. Plano and Robert E. Riggs also observe that most UN votes are not sufficiently important to warrant a threat to alter aid levels. See *Forging World Order* (New York: Macmillan, 1967), p. 154. In an earlier work, however, Riggs discusses the difficulty of separating specific uses of aid to secure compliance in the UN from the general relationship likely to hold between these two variables; see Robert E. Riggs, *Politics in the United Nations: A Study of United States Influence in the General Assembly* (Urbana: University of Illinois Press, 1958), pp. 38-39. The important point, of course, is that whether aid is or is not related to votes remains an empirical question.

¹⁰ Edward S. Mason, *Foreign Aid and Foreign Policy* (New York: Harper and Row, 1964), p. 33.

He observes that "the State Department . . . places high value on the employment of foreign aid to . . . swing critical votes in international bodies. . . . While UN votes may not, and should not, be the primary determinant in aid decisions, it is reasonable to assert that they cannot be ignored—nor are they."¹¹

On the other hand, Francis O. Wilcox, former Assistant Secretary of State for International Organization Affairs, has argued that as a matter of *policy*, the United States eschews the use of foreign aid or similar devices to influence the behavior of UN members.¹² Interestingly, however, the section of Wilcox's article in which he makes this argument is entitled "U.N. Votes and Foreign Aid," carrying a subtle implication, quite apart from the argument itself, that there is something more than a random relationship between these two variables. Similarly, Black's normative view that UN votes should not be the "primary determinant in aid decisions" is echoed by other writers, again implying that foreign aid and UN votes are in fact nonrandomly associated. Mason, for example, has expressed the opinion that foreign aid is "frequently not a very useful instrument" for purposes of "influencing behavior in the United Nations."¹³ Similarly, Charles Wolf, Jr. has asserted that "winning votes in the UN" is an "unreasonable" objective of foreign aid,¹⁴ and Jacob J. Kaplan has expressed the view that ". . . the United States should not seek to starve a poor country into support of its foreign policy. An incentive system cannot operate in the case of every vote in the United Nations or every meeting of developing nations."¹⁵

But at least one writer has argued that policy makers should give more, not less, attention to how foreign aid might be used to influence political outcomes in the United Nations. In the concluding section of his study of *Foreign Aid in a Foreign Policy Framework*, Andrew F. Westwood argues that the "alignment of underdeveloped countries in the Cold War" has been of concern to the United States for two reasons: first, because of the felt need for general ideological support in the competitive struggle

with the Soviet Union; and second, because of ". . . the need for cooperation, and the give and take which produces it, on a wide variety of issues and problems, notably in the United Nations."¹⁶

Westwood concludes that "the efforts to press aid into service [for purposes of general ideological support] probably can be summed up as unnecessary and ill-suited indeed to this realm of ideas and emotions."¹⁷ On the other hand, he argues that the need to apply foreign aid to promote "cooperation" on a "wide variety of issues and problems" has not waned but in fact has become more important.

This [need] has not faded but grown as more underdeveloped countries have entered the United Nations and the necessary majorities there increasingly have depended on their votes. The idea that no *quid pro quo* should be asked in return for aid has governed here as well [as in the area of general ideological support], although practice has diverged from it on occasion. But here the idea has a different character—not that ideological support is too gross a return for aid, but any return, even support on an issue in which the underdeveloped country may have little interest, is inappropriate. It seems likely that one of the major questions for the future is whether aid should not be included within the broad process of give and take on concrete issues which produces cooperation among mature, independent, and equal nations.¹⁸

Data and Methodology

Scope of the analysis. The UN votes analyzed are all committee and plenary roll calls taken in the Eighteenth and Twenty-first General Assemblies, a total of 78 in 1963 and of 163 in 1966. Attention is focused on the relationship between aid and voting preferences of developing states identifiable on the basis of all of these votes; no attempt will be made to identify "critical" votes or specific issues of particular significance to either aid donors or recipients.¹⁹ It is

¹¹ Andrew F. Westwood, *Foreign Aid in a Foreign Policy Framework* (Washington, D.C.: The Brookings Institution, 1966), p. 105.

¹² Westwood, p. 105.

¹³ Westwood, p. 105.

¹⁴ Cf. Alker, "Dimensions of Conflict in General Assembly," pp. 163–64. Much additional work has been undertaken to determine whether a "critical vote" or "issue-area" approach might be a preferable way to address the question examined in this essay. Generally speaking, the results of these analyses suggest that neither of these alternatives is preferable to the generalized approach that has been chosen. For example, a contingency table analysis of developing nations' voting behavior on the China issue and their U.S. aid receipts during 1961–1967 indicated that aid allocations and voting behavior are quite probably independent of one another. See Eugene R. Wittkopf and James Green, "Empirical Perspectives on the

¹¹ Lloyd D. Black, *The Strategy of Foreign Aid* (Princeton, N.J.: D. Van Nostrand, 1968), p. 19.

¹² Francis O. Wilcox, "The Nonaligned States and the United Nations," in *Neutrality and Nonalignment*, ed. Laurence W. Martin (New York: Praeger, 1962), pp. 147–48.

¹³ Mason, p. 42.

¹⁴ Charles Wolf, Jr., "Economic Aid Reconsidered," in *The United States and the Developing Economies*, ed. Gustav Ranis (New York: Norton, 1964), p. 52.

¹⁵ Jacob J. Kaplan, *The Challenge of Foreign Aid* (New York: Praeger, 1967), p. 248. See also Kaplan, p. 73.

important to emphasize, therefore, that the purpose here is to ascertain whether a relationship between donors' aid allocations and recipients' voting behavior exists as a general phenomenon.

The aid recipients used in the statistical analyses were selected from a population of developing nations defined as independent nations that received foreign economic assistance from one or more of the sixteen members of the OECD's Development Assistance Committee²⁰ or from the Soviet bloc during one or more of the six years spanned by the study. So defined, the possible number of observations used in the study varies from 85 in 1962 to 96 in 1967.²¹ A

China Vote and U.S. Foreign Aid, 1961-1967" (paper, University of Florida, January, 1972). Similarly, work has been done using only those votes on which the United States and the Soviet Union did not agree. The results obtained differ only marginally from those displayed later, which employ all Assembly roll calls. Finally, an effort was made to replicate Bruce Russett's factor analysis of UN votes in 1963 in which he found five "super issues" in the General Assembly; see Bruce M. Russett, *International Regions and the International System* (Chicago: Rand McNally, 1967), pp. 63-67. VRA scores (discussed later) were calculated for those votes that loaded at .5 or greater on each of the three most important dimensions in the 1963 Assembly (cold war, 20 votes; intervention in Africa, 17 votes; supranationalism, 18 votes). The VRA's were then correlated with the 1962-1964 U.S. aid RA's (also discussed later). The results are given below (Table A). Although some interesting differences across the various dimensions are apparent, the important point is that the conclusions that can be drawn about the association between aid and votes (or lack of association) do not differ dramatically from those that can be drawn on the basis of looking at all roll-call votes. (See Table 3.)

Table A. U. S. Aid Allocations and U. N. Voting Agreements: Correlations Between 1962-1964 Relative Allocation Indices and 1963 Relative Agreement Indices Calculated by Issue-Area^a

	Cold War	Intervention in Africa	Supranationalism
1962	.26* (67)	.15 (67)	-.04 (67)
1963	.21* (68)	.18 (68)	-.009 (68)
1964	.32* (63)	.45* (63)	-.07 (63)

^a Correlations statistically significant at the .05 level are indicated by an asterisk.

²⁰ West Germany, Portugal, and Switzerland are members of DAC in addition to those nations cited earlier. Aid flows from Germany and Switzerland have been included in the aid matrices analyzed using the transaction-flow model. Portugal is excluded since it extended aid only to its overseas territories.

²¹ These numbers as well as the nations listed in

list of these recipient nations is given in Table 1.

Although by 1967 there were, according to the criteria set forth above, as many as 96 foreign aid recipients, no one donor actually extended aid to all of these potential recipients in any one of the six years analyzed. In fact, the number of recipients for each donor varies greatly; in 1964, for example, the United States extended some form of economic assistance to 77 developing nations, while in the same year France aided only 26 independent nations. Throughout the analysis, attention for each donor country is confined to that subset of developing states to which the donor actually distributed foreign aid.

Three additional criteria are used to delimit the actual number of aid recipients included in the subsequent analyses. First, for reasons discussed below, attention is restricted to those aid recipients that received some *positive* amount of aid from each donor analyzed. Second, only those UN members that missed 40 per cent or fewer of the roll calls in each General Assembly are included in the roll-call analyses. Although this is an arbitrary figure, some minimum number of recorded votes seems necessary since much of the subsequent analysis is based on the behavior of all or major subsets of UN members. Finally, attention is restricted to those aid donors that distributed foreign aid to eight or more developing nations beyond those excluded according to the aforementioned criteria.²²

Confining attention to the actual recipients of aid for each donor is especially important. In effect, it amends the proposition being examined to read *given that a nation receives aid from a particular donor country*, the larger its voting agreement with that donor, the greater will be the amount of aid it receives from that donor. Because of this restriction, it might be argued that the empirical results presented later are in fact not addressed to a critical factor, namely, that the level of agreements between a foreign aid donor and its aid recipients may be

Table 1, include such non-UN members as South Korea, Muscat and Oman, South Vietnam, and Western Samoa. Although these nations are, by definition, excluded from the correlation analyses, they are included here since their aid receipts from the DAC donors have been included in the aid matrices analyzed using the transaction-flow model.

²² Again, there is nothing magic in the number eight; it is simply the minimum number of developing nations included for which data on Soviet bloc assistance are available. But with an N less than eight, the technique of fitting a line to data points is either inappropriate, or the number of observations is so small as to severely restrict the generalizability of the empirical findings.

Table 1. Names and Abbreviations of the Ninety-Six Independent Foreign Aid Recipient Nations Included in the Study^a

Abbreviation	Nation	Abbreviation	Nation
1 AFGH	Afghanistan	49 LIBE	Liberia
2 ALGE	Algeria	50 LIBY	Libya
3 ARGE	Argentina	51 MADA	Madagascar
4 BOLI	Bolivia	52 MLWI	Malawi
5 BOTS	Botswana	53 MALA	Malaysia
6 BRAZ	Brazil	54 MALD	Maldives Islands
7 BURM	Burma	55 MALI	Mali
8 BURU	Burundi	56 MALT	Malta
9 CAMB	Cambodia	57 MAUR	Mauritania
10 CAME	Cameroon	58 MEXI	Mexico
11 CENT	Central African Republic	59 MORO	Morocco
12 CEYL	Ceylon	60 MUSC	Muscat and Oman
13 CHAD	Chad	61 NEPA	Nepal
14 CHIL	Chile	62 NICA	Nicaragua
15 COLO	Colombia	63 NIGE	Niger
16 CONB	Congo (Brazzaville)	64 NGRA	Nigeria
17 CONK	Congo (Kinshasa) ^b	65 PAKI	Pakistan
18 COST	Costa Rica	66 PANA	Panama
19 CUBA	Cuba	67 PARA	Paraguay
20 CYPR	Cyprus	68 PERU	Peru
21 DAHO	Dahomey	69 PHIL	Philippines
22 DOMI	Dominican Republic	70 RWAN	Rwanda
23 ECUA	Ecuador	71 SAUD	Saudi Arabia
24 EL S	El Salvador	72 SENE	Senegal
25 ETHI	Ethiopia	73 SIER	Sierra Leone
26 GABO	Gabon	74 SING	Singapore
27 GAMB	Gambia	75 SOMA	Somalia
28 GHAN	Ghana	76 SOAF	South Arabia Federation ^c
29 GREE	Greece	77 SPAI	Spain
30 GUAT	Guatemala	78 SUDA	Sudan
31 GUIN	Guinea	79 SYRI	Syria
32 GUYA	Guyana	80 TAIW	Taiwan (China)
33 HAIT	Haiti	81 TANZ	Tanzania
34 HOND	Honduras	82 THAI	Thailand
35 INDI	India	83 TOGO	Togo
36 INDO	Indonesia	84 TRIN	Trinidad and Tobago
37 IRAN	Iran	85 TUNI	Tunisia
38 IRAQ	Iraq	86 TURK	Turkey
39 ISRA	Israel	87 UGAN	Uganda
40 IVOR	Ivory Coast	88 UAR	United Arab Republic
41 JAMA	Jamaica	89 UPPE	Upper Volta
42 JORD	Jordan	90 URUG	Uruguay
43 KENY	Kenya	91 VENE	Venezuela
44 KORS	Korea (South)	92 VITS	Vietnam (South)
45 KUWA	Kuwait	93 WSAM	Western Samoa
46 LAOS	Laos	94 YEME	Yemen
47 LEBA	Lebanon	95 YUGO	Yugoslavia
48 LESO	Lesotho	96 ZAMB	Zambia

^a All nations shown were independent by the end of 1967. Those not independent throughout the 1962-1967 time period are listed below, followed by the year in which they achieved independence.

ALGE—1962	GUYA—1966	MLWI—1964	SING—1965	WSAM—1962
BOTS—1966	JAMA—1962	MALD—1965	SOAF—1967	ZAMB—1964
BURU—1962	KENY—1963	MALT—1964	TRIN—1962	
GAMB—1965	LESO—1966	RWAN—1962	UGAN—1962	

Source for determination of the year of independence is Bruce M. Russett, J. David Singer, and Melvin Small, "National Political Units in the Twentieth Century: A Standardized List," *American Political Science Review*, 62 (September, 1968), 935-950.

^b Zaire.

^c Including Aden.

significantly different from (presumably greater than) the level of agreements between a donor and its nonrecipients. Put differently, "between-group" differences may be more significant than "within-group" differences. Accordingly, some empirical evidence to be presented later specifically addresses the question of between-group differences. It will be shown that, on the basis of this analysis, there is little evidence to support the proposition that restricting the analysis to those developing nations that actually receive aid from each donor country analyzed restricts the generalizability of the empirical findings.

To examine the dual notion that aid allocations may be either a cause or a consequence of voting behavior—that is, that aid may be either an inducement to future behavior or a reward (or punishment) for past behavior—the 1963 voting data were correlated with both 1962 and 1964 foreign aid data, and the 1966 voting data were correlated with both 1965 and 1967 aid data. Further, because the General Assembly meets in the Fall of each calendar year, while foreign aid is presumably distributed throughout the year (probably most of it prior to that point in the annual Assembly meeting at which most roll-call votes are taken), correlations between the 1963 aid and 1963 voting data and between the 1966 aid and 1966 voting data have also been included.

Based on the assumption of a one-year lag between the variables, if foreign aid is more consistently used as an *inducement* to future behavior within the Assembly than as a reward for past behavior, then we would expect that the aid and voting correlations would be higher when the aid data refer to the year either prior to or during the Assembly year than when the aid data refer to the year following the Assembly. Conversely, if aid is used as a *reward* (or punishment) for past behavior, then we would expect the correlations lagging behind the aid data by one year to be the greater.²³

Despite the conceptual distinction between the use of aid as an inducement or as a reward, it would be surprising if the analyses were to unearth this distinction. The considerable stability in the distributional patterns of donors' foreign aid can easily be demonstrated.²⁴ Simi-

larly, other writers have documented that over time the basic issues and alignments in the General Assembly have remained largely unchanged.²⁵ Consequently, significantly different patterns in the relationship between aid and voting during any two- or three-year period are most unlikely to occur—and in fact they do not occur. Therefore, the net result of this analysis (i.e., using the concept of a variable lag structure in exploring the relationship between foreign aid and UN voting) will be to document further the stability of these two basic measures of relations among nations.

Foreign aid data and indices. Foreign aid data for the DAC donors were drawn from three publications of the OECD on the geographical distribution of financial flows to developing nations.²⁶ All data for the DAC donors refer to net *disbursements* of foreign economic assistance;²⁷ neither commitments nor military aid data are included. Data on 1962–1964 Soviet bloc aid were drawn from two United Nations publications,²⁸ while those for 1965–1967 were drawn from three publications of the U.S. De-

²³ See, e.g., Russett, *International Regions*, pp. 59–93.

²⁶ *Geographical Distribution of Financial Flows to Less Developed Countries 1960–1964* (Paris: O.E.C.D., 1966); *Geographical Distribution of Financial Flows to Less Developed Countries 1965* (Paris: O.E.C.D., 1967); and *Geographical Distribution of Financial Flows to Less Developed Countries 1966–1967* (Paris: O.E.C.D., 1969).

²⁷ The OECD uses the term "net total official financial flows" to describe these data. The flows are defined as the sum of grants, including grants for technical cooperation; intergovernmental reparation and indemnification payments, a type of disbursement remitted only by Italy and Japan throughout the 1962–1967 time period, and by West Germany until 1965; net official loans; loans repayable in recipients' currencies (net); and transfers of resources through sales for recipients' currencies (principally food aid distributed under the aegis of U.S. Public Law 480). O.E.C.D., *Geographical Distribution 1960–1964*, p. XII.

It will be noted that the aid data do not distinguish between various budgetary categories and administrative agencies in donor countries. In effect, therefore, it is assumed that the aid data comprise the foreign policy output of an aggregate decisional unit in each donor country. This is admittedly a very powerful assumption (as Graham T. Allison has clearly demonstrated in his *Essence of Decision* [Boston: Little, Brown, 1971]) that should perhaps be subjected to additional research. But use of the OECD-DAC data does allow one to move beyond what Baldwin has identified as the tendency to define foreign aid in terms of American institutions. See David A. Baldwin, "Analytical Notes on Foreign Aid and Politics," *Background*, 10 (Winter, 1966), 75–76.

²⁸ *International Flow of Long-term Capital and Official Donations, 1961–1965* (New York: U.N., 1966), pp. 22–23; and *The External Financing of Economic Development* (New York: U.N., 1969), pp. 44–45.

²³ It is because of the absence of an a priori rationale for assuming which temporal sequence is more likely to hold that correlation rather than regression analysis has been chosen as the analytical technique.

²⁴ See Eugene R. Wittkopf, "The Distribution of Foreign Aid in Comparative Perspective: An Empirical Study of the Flow of Foreign Economic Assistance, 1961–1967" (Ph.D. dissertation, Syracuse University, July, 1971), pp. 38–41.

partment of State.²⁹ Data on Soviet bloc aid refer to *commitments* of economic assistance (grants and credits), not to disbursements. It should be borne in mind, therefore, that the DAC and Soviet bloc data differ in kind; moreover, the fact that the Soviet bloc data are commitments rather than disbursements may help explain the relatively small number of cases displayed later for the Soviet bloc. Because of the definitional distinction between DAC and Soviet bloc data, two separate sets of analyses of the relationship between aid and votes will be included, one excluding Soviet bloc aid, the other including the data in the analyses.

Given the definition of foreign aid employed for the DAC donors, it is possible that recipients may make net repayments of aid to donors in excess of their gross aid receipts. In effect, these developing nations are "recipients" of "negative total aid." Since the total aid definition across the entire range of recipients is a net definition, it would seem most reasonable to retain all recipient states for a particular donor in the subsequent analyses, even when their total aid receipts fall below zero. Negative aid flows, however, can markedly affect the relative allocation indices used as one of the empirical measures of aid allocations. Hence, all negative aid values were excluded from the aid matrices prior to computing the relative allocation indices using the transaction-flow model. To retain comparability across the various analyses, negative aid flows have also been excluded from the correlation analyses based on the absolute vol-

ume of aid allocated by aid donors among developing nations.

The first foreign aid variable used in the correlation analyses is the absolute amount of foreign aid allocated by an aid donor among its aid recipients. The second measure used is based on a transaction-flow analysis of the aid data. The transaction-flow model used is a version of the original model devised by I. Richard Savage and Karl W. Deutsch,³⁰ as revised by Leo A. Goodman³¹ and programmed by Steven J. Brams.³² This model's principal merit in studying aid allocations is that it provides a common denominator for data on the aid programs of donors otherwise vastly disparate in terms both of their magnitude and geographical scope of activity. Further, it allows one to tie all donors together into a single "system" so that the aid activities of donors relative to one another can easily be described.

The transaction-flow measure used is the *RA* index, referred to as a "relative allocation" index.³³ The measure is defined as

$$RA_{ij} = \frac{A_{ij} - E_{ij}}{E_{ij}},$$

where

RA_{ij} = the relative allocation of foreign aid from the i^{th} donor to the j^{th} recipient,

E_{ij} = the expected allocation of foreign aid from i to j , and

A_{ij} = the actual allocation of foreign aid from i to j .³⁴

³⁰ I. Richard Savage and Karl W. Deutsch, "A Statistical Model of the Gross Analysis of Transaction Flows," *Econometrica*, 28 (July, 1960), 551-72.

³¹ Leo A. Goodman, "Statistical Methods for the Preliminary Analysis of Transaction Flows," *Econometrica*, 31 (January-April, 1963), 197-208; and Leo A. Goodman, "A Short Computer for the Analysis of Transaction Flows," *Behavioral Science*, 9 (April, 1964), 176-86.

³² Steven J. Brams, "A Generalized Computer Program for the Analysis of Transaction Flows," *Behavioral Science*, 10 (October, 1965), 487-88.

³³ The more commonly accepted term for transaction-flow *RA*'s is "relative acceptance" indices. "Relative allocation" has been chosen for the aid *RA*'s, however, since this seems a more accurate reflection of the nature of foreign aid monies treated as transaction flows given the absence of a two-way flow of funds.

³⁴ The reason for excluding negative aid flows from the analysis should now be apparent. If both A_{ij} and E_{ij} are negative, and the former is greater than the latter, RA_{ij} can be a positive number, indicating aid flows greater than an indifferent amount, despite the fact that it is based on a negative A_{ij} . Intuitively, this is, of course, an unreasonable result. More importantly, the *RA* for a second donor, k , assuming k also allocates aid to recipient j , can be markedly inflated due to the negative flow from i to j , thereby causing RA_{kj} to have an extreme effect on the correlation coefficient used to assess the association between donor k 's aid allocations and developing nations' voting agreements with k .

²⁹ Director of Intelligence and Research, *Communist Governments and Developing Nations: Aid and Trade in 1965* (Research Memorandum RSB-50, June 17, 1966), p. 2; Director of Intelligence and Research, *Communist Governments and Developing Nations: Aid and Trade in 1967* (Research Memorandum RSE-120, August 14, 1968), pp. 2-3; and Director of Intelligence and Research, *Communist Governments and Developing Nations: Aid and Trade in 1968* (Research Memorandum RSE-65, September 5, 1969), pp. 2-3. UN data have been used for the first three years since State Department estimates of Soviet bloc data are not available on an annual basis for all of the years 1962-1964. State Department estimates have been used for the later three-year period rather than UN estimates on the assumption that the former are more accurate than the latter. UN data apparently were collected from unofficial sources such as press releases as well as official Soviet and East European publications. The State Department, on the other hand, presumably received its information directly from recipient nations. It is assumed, therefore, that the margin of error is less in the State Department than in the UN data. See also Vassil Vassilev, *Policy in the Soviet Bloc on Aid to Developing Countries* (Paris: Development Centre of the Organization for Economic Cooperation and Development, 1969), pp. 61-66.

The expected value, E_{ij} , is calculated on the basis of the allocation of each donor's total aid volume as distributed proportionally among all of the recipients included in the "system" analyzed. The model assumes that donor i will send to recipient j approximately the same percentage of its total foreign aid as the percentage of the total aid which j receives from all donor countries combined. If, for example, Argentina receives 10 per cent of the total aid disbursed by the DAC members, the model predicts that Argentina will receive 10 per cent of whatever percentage each donor contributes to the total volume of aid disbursed by all of the DAC members. Because the model is indifferent to all other factors or variables, it is sometimes referred to as an indifference, or null, model.³⁵

We know, of course, that foreign aid donors are *not* indifferent to all factors other than the total magnitude of aid distributed among developing nations. By making this assumption, however, we can establish a benchmark against which to assess the actual distribution of aid by each donor. In effect, the transaction-flow model serves only to highlight greater than (or less than) expected aid flows.³⁶ Explaining deviations from the expected values is a separate task. For this purpose, a measure of voting agreements in the UN directly analogous to the aid RA indices is used.

Voting agreement indices. The first measure of the roll-call agreements between each donor and its aid recipients utilized is the percentage

³⁵ For a more complete discussion of the transaction-flow model, see Steven J. Brams, "Transaction Flows in the International System," *American Political Science Review*, 60 (December, 1966), 882-86; Steven J. Brams, "Trade in the North Atlantic Area: An Approach to the Analysis of Transformations in a System," *Peace Research Society Papers*, VI, Vienna Conference (1966), pp. 144-48; and E. Wittkopf, "The Concentration and Concordance of Foreign Aid Allocations." My thanks to Professor Brams for the considerable guidance and encouragement he lent to the application of the transaction-flow model and his computer program INDIFF to the data on foreign aid flows.

³⁶ The principal difference between the original Savage-Deutsch model and the Goodman-Brams modification is that the former excludes from the calculation of E_{ij} 's only a country's transactions with itself, while the latter excludes all pairs not actually linked by known transaction flows. In the case of the foreign aid data, three of the four logical portions of the square foreign aid matrix are, by definition, blank—recipient to recipient, recipient to donor, and donor to donor aid flows. With so many zero entries, it was necessary to assign an arbitrarily small value (equal to \$.01) to one cell within each row and column that would otherwise have been entirely blank in order to ensure calculation of E_{ij} 's for the actual non-blank cells.

of all roll-call votes taken in each of the Assemblies, with both the aid donor and recipient present and voting, on which both donor i and recipient j vote identically.³⁷ Abstentions as well as yeas and nays are included in the calculation. For each donor this variable is correlated with the absolute amount of foreign aid the donor allocates among its aid recipients.

The second measure of voting agreements employs a model developed by Steven J. Brams and Michael K. O'Leary for the comparative analysis of voting bodies.³⁸ Specifically, we will employ the Brams-O'Leary relative agreement index, defined as³⁹

$$VRA_{ij} = \frac{AG_{ij} - E(AG_{ij})}{E(AG_{ij})},$$

where

VRA_{ij} = the relative agreement between UN members i and j ,

AG_{ij} = the actual number of times members i and j agree across a set of roll calls, and

$E(AG_{ij})$ = the expected number of roll calls on which a randomly-selected pair of Assembly members, for all roll calls on which members i and j are both present and voting, agree.

The expected agreement between i and j , $E(AG_{ij})$, is defined as the sum of the probabilities of agreement between i and j , on each of the roll calls with both i and j present and voting, included in the set of roll calls analyzed. These probabilities are based on the actual distribution of yeas, nays, and abstentions in the General Assembly on each roll-call vote.⁴⁰

What the Brams-O'Leary model does is establish a statistical norm, based on the behavior of all members in the General Assembly, against which to measure the actual number of voting agreements between specified pairs of

³⁷ Todd uses this measure of voting agreements in his study of voting in the Security Council. James E. Todd, "An Analysis of Security Council Voting Behavior," *Western Political Quarterly*, 22 (March, 1969), 61-78.

³⁸ Steven J. Brams and Michael K. O'Leary, "An Axiomatic Model of Voting Bodies," *American Political Science Review*, 64 (June, 1970), 449-70.

³⁹ Brams and O'Leary, p. 463.

⁴⁰ Brams and O'Leary, pp. 452-53. The Brams-O'Leary model assumes that the voting behavior of legislative members is independent from one roll call to the next. It can be demonstrated, however, that the value of the expected term in the calculation of VRA is not dependent on the assumption of independence. See Lawrence S. Mayer, "A Note on 'An Axiomatic Model of Voting Bodies'," *American Political Science Review*, 65 (September, 1971), 764-65, and the response in Steven J. Brams and Michael K. O'Leary, "Comment on Mayer's 'A Note on 'An Axiomatic Model of Voting Bodies'"', *American Political Science Review*, 65 (September, 1971), 766-67.

members. Like the pairwise measure discussed earlier, the *VRA* index also expresses agreement between two actors in percentage terms. The VRA_{ij} percentages, however, measure deviations from zero, the point at which *i* and *j* agree exactly as often as would be expected on the basis of the behavior of all members in the Assembly.⁴¹

In fact, for each donor country in both General Assemblies analyzed there is almost 100 per cent covariation between the percentage of pairwise agreements measure and the *VRA* index. Operationally, this means it makes virtually no difference which agreement variable is used in the correlation analyses.⁴² However, because the *VRA* index is conceptually analogous to the foreign aid relative allocation indices used as one of the foreign aid variables, the *VRA* index will be retained as the agreement measure when the aid *RA* indices are also used.

In addition, the Brams-O'Leary relative agreement index also provides a technique for assessing the difference in the level of agreements between each aid donor and its aid recipients and nonrecipients—the “between group” differences discussed earlier. Group relative agreement scores are defined as⁴³

$$VRA_{IJ} = \frac{\sum_{i,j} A_{ij} - \sum_{i,j} E_{ij}}{\sum_{i,j} E_{ij}},$$

where

VRA_{IJ} = the relative agreement of member subsets *I* and *J*,

A_{ij} = the actual number of agreements between members *i* and *j*, with the summations ranging over all individual members *i* and *j* belonging to the (disjoint) subsets *I* and *J*, and

E_{ij} = the expected number of agreements between members *i* and *j*, with the summations ranging over all individual members *i* and *j* belonging to the (disjoint) subsets *I* and *J*.

For the purpose of this analysis, subset *I* always refers, with the exception of the analysis

⁴¹ Conceptually, the *VRA*'s are virtually identical with the aid *RA*'s. The two indices differ only in the calculation of the expected term; as indicated, the expected values for the aid *RA*'s depend on the row and column marginal totals in the aid matrix, while the expected values for the *VRA*'s depend upon the probabilities of agreement between a randomly-selected pair of Assembly members across a set of roll-call votes.

⁴² Substantively, this means that the patterns of agreements between particular pairs of states are not affected by the overall divisions in the Assembly. See Brams and O'Leary, “An Axiomatic Model of Voting Bodies,” p. 463.

⁴³ Brams and O'Leary, p. 464.

for the Soviet bloc, to an individual UN member, namely, foreign aid donor *i*. For each *I* subset, two *J* subsets are defined, one which includes all UN members that receive foreign aid from the *i*th donor, the other which includes all UN members that are *potential candidates* for foreign aid but do not receive aid from the *i*th donor. For the Soviet bloc, subset *I* refers to the Soviet Union and all Eastern European nations previously identified as aid donors.⁴⁴

The concept *potential candidates* for foreign aid was operationalized as follows. It seems intuitively unreasonable to include such countries as the United States, the Soviet Union, Canada, the United Kingdom, South Africa, and other similar states in the calculation of group *VRA* scores for nonrecipients of aid. Similarly, in none of the data collected for purposes of the present study, including data on Soviet bloc commitments, were receipts of foreign aid by Eastern European or Asian Communist nations reported. Therefore, in calculating group *VRA* scores for each donor country *i*, nonrecipients of foreign aid were defined as those UN members that received foreign aid in one or more forms from one or more of the *other* donors, whether a DAC donor or the Soviet bloc.

Given the stability over time in the relationship between aid and voting demonstrated in the correlation analyses presented below, group *VRA* scores were calculated only for the years 1963 and 1966. According to the definition elaborated in the preceding paragraph, 80 of the 113 members of the United Nations in 1963 were defined as potential recipients of foreign aid in that year; of the 122 members of the United Nations in 1966, 90 were defined as potential recipients of foreign aid in that year.⁴⁵

Findings

Percentage of pairwise agreements and absolute amounts of foreign aid. The results of the analyses of each donor's aid measured in absolute terms and the percentage of pairwise agreements in the 1963 and 1966 General Assemblies, displayed in Table 2, are indeed striking. Although in certain cases, such as the results for the United States based on the 1963 Assembly, there is a positive association between the

⁴⁴ As a practical matter it would make virtually no difference if subset *I* were defined to include only the Soviet Union, since the U.S.S.R. and its Eastern European allies exhibit almost identical behavior in their UN voting patterns.

⁴⁵ Developing nations making net repayments of aid (i.e., negative aid recipients) to a particular donor, as well as those that missed more than 40 per cent of the UN roll calls, were subtracted from these numbers for purposes of calculating group *VRA*'s for that donor.

Table 2. Correlations Between 1963 and 1966 General Assembly Pairwise Voting Agreements
(Per cent) and 1962-1964 and 1965-1967 Foreign Aid Disbursements^a
(Number of foreign aid recipients [N] shown in parentheses)

Donor Country	Total foreign aid			Total foreign aid		
	1962	1963	1964	1965	1966	1967
Australia	—	—	—	-.01 (19)	-.004 (20)	-.16 (21)
Austria	—	.41 (16)	-.04 (26)	.09 (35)	-.18 (32)	.03 (41)
Belgium	—	—	—	-.06 (9)	-.11 (26)	-.04 (53)
Canada	.35 (22)	.27 (27)	.16 (33)	-.09 (41)	-.17 (45)	-.17 (45)
Denmark	—	—	—	.13 (17)	-.18 (25)	-.08 (26)
France	-.19 (21)	-.15 (20)	-.24 (23)	-.28 (20)	-.28 (26)	-.20 (26)
Italy	.02 (21)	-.31 (20)	-.16 (42)	-.11 (44)	-.12 (44)	-.21 (44)
Japan	-.25 (29)	-.12 (35)	-.03 (34)	-.10 (38)	-.18 (41)	-.07 (46)
Netherlands	—	—	—	-.25 (11)	-.38 (13)	-.37 (14)
Norway	—	—	.10 (17)	-.22 (16)	-.28 (23)	-.43* (24)
Sweden	-.28 (14)	-.004 (23)	.39 (15)	-.08 (19)	-.27 (23)	-.24 (19)
United Kingdom	-.18 (27)	-.03 (29)	.02 (56)	-.17 (56)	-.21 (60)	-.22* (64)
United States	.08 (67)	.04 (68)	.03 (63)	-.07 (73)	-.06 (71)	-.06 (69)
Soviet bloc ^b	-.24 (9)	.28 (8)	.06 (16)	-.08 (16)	-.18 (12)	-.32 (11)

^a Aid data for the Soviet bloc are commitments, not disbursements. Recipients of negative foreign aid and recipients that missed more than 40 per cent of the Assembly roll calls are excluded. A dash indicates that *N* is less than eight. Data on Australian disbursements prior to 1965 are not available. Correlation coefficients significant at the .05 level are indicated by an asterisk (*).

^b Correlations between the per cent of agreements of each aid recipient with the Soviet Union and commitments of economic assistance from the Soviet bloc.

variables, the magnitudes of the correlations are virtually negligible. In fact, if we use statistical significance as a decision-rule to assess the strength of the associations,⁴⁶ none of the cor-

⁴⁶ The appropriateness of significance tests in cases where the sample and the universe are the same, typi-

cally the case in cross-national research, remains an unresolved debate among political science and international relations scholars. The use of such tests here is a conservative one and follows the suggestions of Winch and Campbell, who argue that in the case of nonsampled data, tests of significance are appropriate to an analysis of the (null) hypothesis that the magni-

Table 3. U. S. Aid Allocations and U. N. Voting Agreements: Correlations Between 1963 and 1966 Relative Agreement Indices and 1962-1964 and 1965-1967 Relative Allocation Indices*

(Number of foreign aid recipients [N] shown in parentheses)

	1962	1963	1964	1965	1966	1967
Total foreign aid: excluding Soviet bloc commitments	.29* (67)	.31* (68)	.45* (63)	.38* (73)	.37* (71)	.42* (69)
Total foreign aid: including Soviet bloc commitments	.31* (67)	.29* (68)	.51* (63)	.30* (73)	.33* (71)	.36* (69)

* Recipients of negative foreign aid and recipients that missed more than 40 per cent of the Assembly roll calls are excluded. Correlation coefficients significant at the .05 level are indicated by an asterisk (*).

relations indicates a significant relationship between the variables in the hypothesized positive direction. Indeed, the only significant associations are the inverse ones for Norway and the United Kingdom in 1967. Moreover, the overwhelming majority of the correlations, including those for the United States based on the 1966 Assembly, point toward an inverse relationship between foreign aid and UN votes for virtually all aid donors. The number of statistically significant negative correlations is admittedly small. But substantively, the overall pattern of the results, far from being consistent with the hypothesized relationship between aid and votes, suggests that most foreign aid donors are prone to reward their enemies rather than their friends. We will return to this inference in the discussion of the correlations between the voting and foreign aid *RA*'s.

Voting relative agreement and foreign aid relative allocation indices. The results of the analyses using the *VRA* and aid *RA* indices are displayed in Tables 3-5. Looking first at the correlations for the United States (Table 3), we can see that there is a dramatic change from the patterns summarized in Table 2.⁴⁷ All of

the correlations for both Assembly sessions now indicate a positive association between aid and votes. Further, when statistical significance is used to assess the strength of the associations, all of the correlations fit the hypothesis that the larger the (relative) number of voting agreements exhibited by a developing nation with the United States, the greater will be the (relative) amount of aid received by that state from the U.S.

At the same time, however, it is also true that in terms of shared variance none of the correlations is overwhelming in magnitude. In the case of the 1962 aid *RA*'s and the 1963 *VRA*'s, for example, less than 10 per cent of the variation in one variable is explained by the other, while in the case of the largest correlations in the table, those for 1964, only between 20 and 26 per cent of the variation is thus explained.⁴⁸ In addition, the results provide little

from those shown in Table 2 can be attributed largely to the transaction-flow model, which, in effect, "transforms" the aid data by adjusting for the size of aid recipients assumed to be reflected in the relative magnitudes of foreign aid transactions between donors and recipients. The magnitude of foreign aid transactions may also indicate the importance of recipients to donors, but this will be reflected in the aid *RA*'s only if the foreign aid transactions for a particular donor are greater than what would be expected on the basis of the proportionate distribution of transactions of all aid donors included in the system analyzed. Essentially, then, the indifference model is an alternative to the more usual ways of "normalizing" foreign aid data, such as by the population size, GNP, or imports of developing nations. Interestingly, in the analysis of U.S. aid and developing nations' voting behavior on the China issue cited earlier (footnote 19), an aid/imports ratio was used as one of the foreign aid variables, but the results were virtually identical to those obtained when only the absolute amounts of aid dollars were used.

⁴⁸ These generally low levels of shared variance are perhaps not surprising given the large number of both political and development considerations presumed to undergrid U.S. aid allocations. For a discussion and empirical analysis of some of these additional con-

tude of the relationship between two variables is due to chance. (Robert F. Winch and Donald T. Campbell, "Proof? No. Evidence? Yes. The Significance of Tests of Significance," *The American Sociologist*, 4 [May, 1969], 143). Cf. Bruce M. Russett et al., *World Handbook of Political and Social Indicators* (New Haven: Yale University Press, 1964), p. 263; and J. David Singer and Michael Wallace, "Intergovernmental Organization and the Preservation of Peace, 1816-1964: Some Bivariate Relationships," *International Organization*, 24 (Summer, 1970), 533. Significance tests are also particularly useful in situations where there are large variations in the number of observations, as is the case here, which otherwise would make comparisons across foreign policy actors very difficult.

⁴⁷ Because the percentage of pairwise voting agreements and the *VRA*'s are very highly correlated, as noted earlier, the change in the results for the U.S.

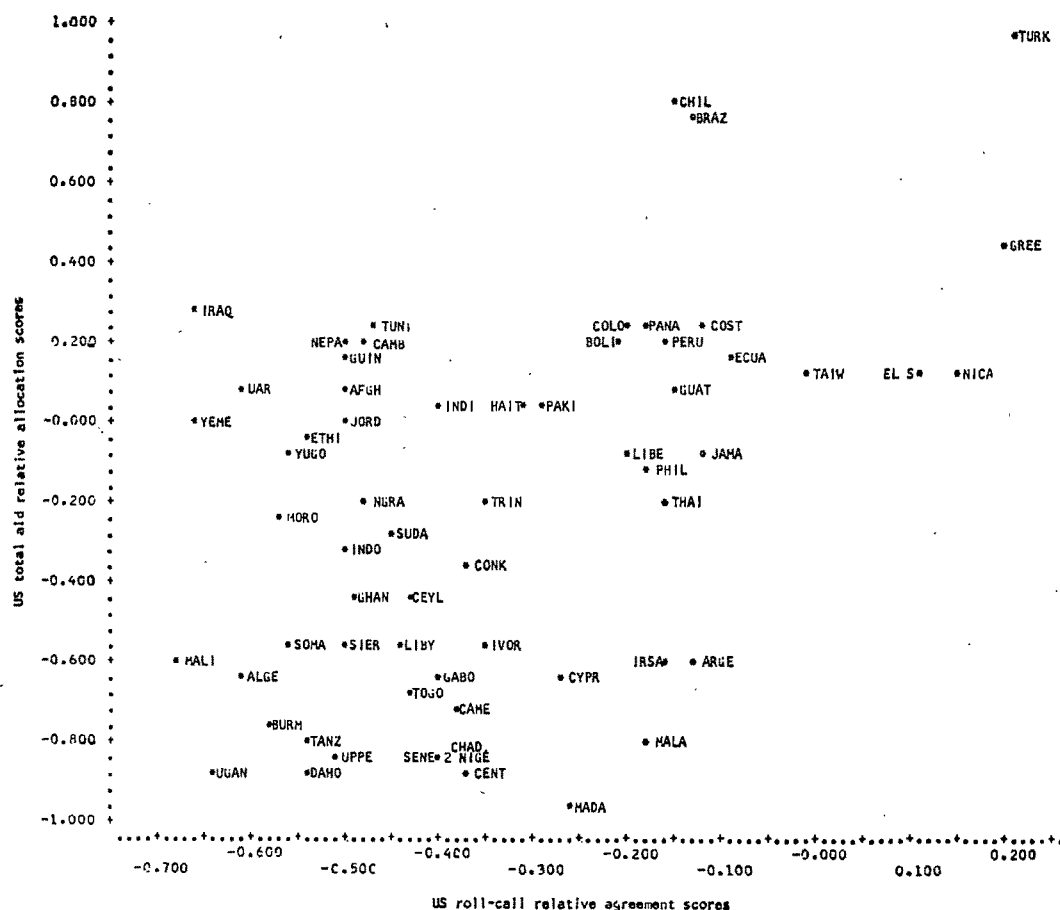


Figure 1. Relationship Between United States Total Aid Relative Allocation Indices, 1964, and General Assembly Roll-Call Relative Agreement Scores with the United States, 1963

basis for drawing an inferential distinction between the use of aid as an inducement to future behavior and its use as a reward (or punishment) for past behavior in the UN. True, in both Assemblies the relationship between aid in year $t + 1$ and voting agreements in year t is moderately stronger than in those cases in which the aid data precede the voting data. But the differences are not striking in either Assembly. All we can safely say at this point is that there is a moderately strong, statistically significant association between the relative allocation of U.S. foreign aid and the relative number of voting agreements between the U.S. and its aid recipients in the General Assembly.

siderations, see E. Wittkopf, "The Concentration and Concordance of Foreign Aid Allocations," and E. Wittkopf, *Western Bilateral Aid Allocations: A Comparative Study of Recipient State Attributes and Aid Received* (Beverly Hills, Calif.: Sage Publications, 1972).

Are these results for the United States simply due to a small number of extreme cases? In general, the answer appears to be no. Scatter diagrams for two of the strongest associations in Table 3, those for 1964 and 1967 (excluding Soviet bloc commitments), are displayed in Figures 1 and 2.⁴⁰ The diagram for 1964 (Figure 1) does suggest that the extreme cases of Chile, Brazil, Turkey, and Greece make a substantial contribution to the moderately strong, positive association between aid and votes. At the same time, however, the relatively large concentration of Latin American states, plus Taiwan, Liberia, Thailand, and the Philippines in the upper right-hand quadrant of the diagram, and in addition, the large number of Af-

⁴⁰ Nations are identified according to their abbreviated names given in Table 1. The reader is cautioned to note that the axes in the two scattergrams are not identical. This is also true for Figure 3 shown on p. 884.

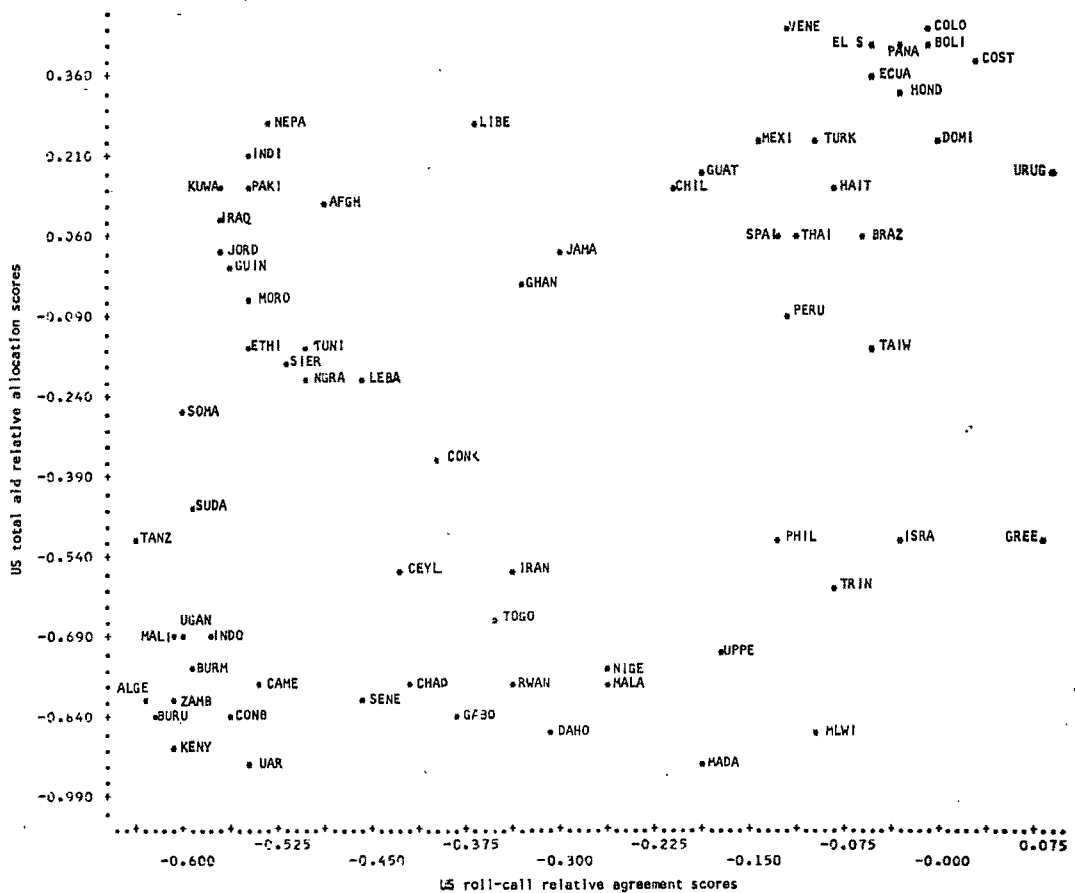


Figure 2. Relationship Between United States Total Aid Relative Allocation Indices, 1967, and General Assembly Roll-Call Relative Agreement Scores with the United States, 1966

rican states (those which derive the bulk of their aid from Britain and France) in the lower left-hand portion of the diagram together point toward a positive association between aid and votes across a large range of developing nations. Similarly, in 1967 (Figure 2) the pattern of a large number of Latin American nations in the upper right-hand portion of the graph and a large number of African nations in the lower left-hand portion again accounts for the positive association between aid and votes. An interesting feature of both diagrams, one most apparent in 1967, is the large number of Asian and Arab nations in the upper left-hand portion of the diagram. If the hypothesis were correct that aid motivates voting behavior, or vice versa, then we would expect either: (1) that these states would agree more often with the United States than they in fact did, or (2) that the U.S. would not allocate as much relative aid to these nations as it actually did. Because

neither of these outcomes is reflected in the diagram, U.S. behavior in these instances was probably motivated by other factors, such as political or security considerations in the case of the Asian nations, and perhaps the desire to maintain some semblance of political influence in the region in the case of the Middle Eastern nations. Similarly, the results suggest that factors other than U.S. aid, such as anti-Israeli or anticolonial sentiments, are more important explanations of the recipients' political behavior in the UN.

How do these findings for the United States compare with those for other aid donors? The results shown in Tables 4 and 5 clearly indicate that, in contrast to the United States, for most donors use of the aid *RA* and *VRA* indices does not yield results dramatically different from those displayed in Table 2. Judging by the signs of the coefficients, a somewhat smaller proportion of the correlations is consistent with

Table 4. Correlations Between 1963 General Assembly Roll-Call Relative Agreement Indices and 1962-1964 Foreign Aid Relative Allocation Indices^a

(Number of foreign aid recipients [N] shown in parentheses)

Donor Country	Total foreign aid: excluding Soviet bloc commitments			Total foreign aid: including Soviet bloc commitments		
	1962	1963	1964	1962	1963	1964
Austria	—	.06 (16)	.36* (26)	—	.28 (16)	.37* (26)
Canada	.24 (22)	.34* (27)	.12 (33)	.27 (22)	.34* (27)	.14 (33)
France	-.52* (21)	-.38 (20)	-.61* (23)	-.51* (21)	-.37 (20)	-.55* (23)
Italy	.0003 (21)	-.38* (20)	-.12 (42)	-.003 (21)	-.39* (20)	-.10 (42)
Japan	-.24 (29)	-.11 (35)	-.18 (34)	-.25 (29)	-.14 (35)	-.17 (34)
Norway	—	—	-.14 (17)	—	—	-.11 (17)
Sweden	-.17 (14)	-.22 (23)	-.05 (15)	-.18 (14)	-.25 (23)	-.03 (15)
United Kingdom	-.22 (27)	-.21 (29)	-.08 (56)	-.22 (27)	-.22 (29)	-.04 (56)
Soviet bloc ^b	NA	NA	NA	-.02 (9)	-.002 (8)	.56* (16)

^a Recipients of negative foreign aid and recipients that missed more than 40 per cent of the Assembly roll calls are excluded. A dash indicates that *N* is less than eight. Belgium, Denmark, and the Netherlands are thus excluded entirely. Data on Australian disbursements prior to 1965 are not available. NA indicates not applicable. Correlation coefficients significant at the .05 level are indicated by an asterisk (*).

^b Correlations between the relative agreement scores of each aid recipient with the Soviet Union and foreign aid relative allocation indices for commitments of economic assistance from the Soviet bloc.

the hypothesized positive association between aid and votes in the 1963 Assembly, and a somewhat greater proportion is consistent with the hypothesis in the 1966 Assembly. Of the 51 correlations calculated on the basis of the 1963 voting data (including those for the U.S.), 35 per cent are in the predicted positive direction (compared with 48 per cent in Table 2); of the 81 correlations based on the 1966 voting data, 36 per cent are in the predicted direction (compared with 7 per cent in Table 2). The positive correlations, however, are statistically significant for only four donors—Austria (1964), Canada (1963), Australia (1966, including Soviet bloc aid commitments), and the Soviet bloc (1964). And for none of these donors is the relationship consistently strong over time, as it is for the United States. Thus the overall

pattern of either a negative or a weak positive association between aid and votes remains.

Turning to the question of the use of aid as an inducement or as a reward, we can note that like the results for the United States, the results for most other donors fail to support any inferences about these two conceptually distinct uses of foreign aid. The case that appears to be an exception is the Soviet bloc, for which the correlations based on the 1963 voting data might be interpreted as indicating the use of aid as a reward. However, this pattern is not repeated in the 1966 Assembly, and the small number of cases for each correlation would in any event make such an inference suspect. Further, given the generally small number of cases, the correlations can be especially sensitive to extreme data points, a fact which appears to account for

Table 5. Correlations Between 1966 General Assembly Roll-Call Relative Agreement Indices and 1965-1967 Foreign Aid Relative Allocation Indices^a
(Number of foreign aid recipients [N] shown in parentheses)

Donor Country	Total foreign aid: excluding Soviet bloc commitments			Total foreign aid: including Soviet bloc commitments		
	1965	1966	1967	1965	1966	1967
Australia	.31 (19)	.37 (20)	.15 (21)	.30 (19)	.41* (20)	.14 (21)
Austria	-.19 (35)	.05 (32)	.02 (41)	-.12 (35)	.12 (32)	.02 (41)
Belgium	-.44 (9)	-.31 (26)	-.12 (53)	-.44 (9)	-.31 (26)	-.13 (53)
Canada	.09 (41)	.11 (45)	.20 (45)	.17 (41)	.11 (45)	.19 (45)
Denmark	-.17 (17)	-.31 (25)	.01 (26)	-.05 (17)	-.15 (25)	-.03 (26)
France	-.33 (20)	-.14 (26)	-.08 (26)	-.33 (20)	-.17 (26)	-.08 (26)
Italy	-.02 (44)	-.28* (44)	-.27* (44)	-.07 (44)	-.25* (44)	-.28* (44)
Japan	-.04 (38)	-.13 (41)	.01 (46)	-.09 (38)	-.10 (41)	.01 (46)
Netherlands	.01 (11)	.08 (13)	-.44 (14)	.02 (11)	.08 (13)	-.41 (14)
Norway	-.31 (16)	-.40* (23)	-.39* (24)	-.36 (16)	-.41* (23)	-.38* (24)
Sweden	-.07 (19)	-.17 (23)	-.29 (19)	-.11 (19)	-.17 (23)	-.30 (19)
United Kingdom	-.16 (56)	-.17 (60)	-.17 (64)	-.17 (56)	-.17 (60)	-.18 (64)
Soviet bloc ^b	NA	NA	NA	-.20 (16)	-.31 (12)	-.42 (11)

^a Recipients of negative foreign aid and recipients that missed more than 40 per cent of the Assembly roll calls are excluded. NA indicates not applicable. Correlation coefficients significant at the .05 level are indicated by an asterisk (*).

^b Correlations between the relative agreement scores of each aid recipient with the Soviet Union and foreign aid relative allocation indices for commitments of economic assistance from the Soviet bloc.

the unusually high correlation for 1964. Finally, it bears repeating that since the Soviet data are commitments rather than disbursements, the Soviet bloc aid RA's are considerably more disparate than would probably be the case if the Soviet data measured the actual amount of resources flowing from the Soviet bloc to developing nations (as do the data for the DAC donors).

It is interesting that a shift in the geographic location of Soviet aid recipients beginning in 1965 appears to account for the negative (but not significant) correlations for the Soviet bloc based on the 1966 voting data. Prior to 1965 Soviet commitments were extended only to Asian and African developing nations, but beginning in that year Soviet aid commitments to European and Latin American nations are re-

ported as well.⁵⁰ The magnitudes of the Soviet aid commitments to these nations have usually yielded relatively high aid *RA*'s. Moreover, these nations have traditionally been more pro-Western in their UN voting behavior than have most Asian and African developing nations. Therefore, the resulting upward pull on the correlation coefficients suggests that, in simple linear terms, the Soviet bloc too tends to reward its Assembly enemies more than its friends. This is particularly apparent in 1967, for which a scattergram indicates that the generally high positive aid *RA*'s of Turkey, Uruguay, and Chile coupled with their negative *VRA*'s with the Soviet bloc make a substantial contribution to the *r* of $-.42$.

These results for the Soviet bloc raise the question of whether similar patterns in the aid allocations of other donors can be used to explain the large number of negative correlations shown in Tables 4–5. Scatter diagrams of the statistically significant negative correlations for Italy and Norway based on the 1966 voting data do suggest that a relatively small number of cases (particularly for Norway) contribute heavily to the negative associations. Again the substantive interpretation would seem to be that enemies are rewarded more than friends. Put differently, the relative amount of aid these nations have received from the respective donors has had little impact on their propensity to vote with the donor in the UN, or, conversely, the position these nations have taken on the range of issues before the UN vis-à-vis each donor has had relatively little impact on the allocation of aid by the donor. Particularly important in this respect seems to be the position on decolonization and related issues taken in the UN by a number of developing nations, notably in Asia and Africa.⁵¹ This is especially apparent for France, for which the correlations based on the 1963 Assembly indicate a strong inverse association between aid and votes.

The position France's former African colonies have taken on many UN issues in opposition to France is reflected in their high negative

VRA's with their former ruler. At the same time, however, these nations receive the largest proportion of their aid from France and therefore have correspondingly high aid *RA*'s. In contrast to these African states, some recipients of French foreign aid, notably those in Europe and Latin America, are among those developing nations least prone to disagree with Western states on decolonization and related UN issues.⁵² Given the proportionately larger share of aid received by these states from the United States, however, the aid *RA*'s of France's European and Latin American aid recipients tend to be much lower than the *RA*'s of the African states. Consequently, a line fitted through these data points shows that aid and voting agreements are inversely rather than positively associated.

The interaction of these factors is illustrated in the scatter diagrams of the relationship between the 1964 aid *RA*'s (excluding Soviet bloc commitments) and the 1963 *VRA*'s for France and the United States displayed in Figures 3 and 1, respectively. It is readily apparent from the diagram for France (Figure 3) that the inclusion of Chile, Brazil, Greece, and Turkey accounts for the high negative correlation between the two variables ($r = -.61$). With the exception of Madagascar, none of France's former African colonies agreed more often with France (in relative terms) than did these four non-African aid recipients. At the same time, none of these four nations received as much foreign aid from France (in relative terms) as did France's former African colonies. In fact, the aid *RA*'s are less than an indifferent amount (i.e., less than zero), partly because these nations received a relatively large proportion of the foreign aid disbursed by the United States. This fact is vividly illustrated in the scatter diagram for the U.S. (Figure 1).

Again, then, we are faced with the temptation to argue that France (like other donors) is prone to reward its enemies more than its friends.⁵³ There are alternatives, however. On

⁵⁰ Estimates of Soviet bloc aid commitments in millions in U.S. dollars to European and Latin American developing nations are as follows:

	1965	1966	1967		1965	1966	1967
ARGE	15			ECUA			5
BRAZ		128		GREE	84		
CHIL			55	TURK	200		200
				URUG			10

⁵¹ For a discussion of decolonization issues, with special reference to African states, see David A. Kay, *The New Nations in the United Nations, 1960–1967* (New York: Columbia University Press, 1970), pp. 80–85.

⁵² Kay, p. 84.

⁵³ In a separate analysis of the two basic component categories of foreign aid, grants and gross official loans, it was found that there is a positive (but not significant) relationship between France's *VRA*'s and aid *RA*'s when the latter are calculated only on the basis of grants, and a negative relationship similar to those shown in Tables 4 and 5 when the aid *RA*'s are calculated on the basis of gross loans. The reason lies in the fact that France chose not to allocate grants to its European and Latin American aid recipients, thus excluding those nations most prone to agree with her in the UN as the above results indicate. For details, see E. Wittkopf, "The Distribution of Foreign Aid in Comparative Perspective," pp. 124–33.

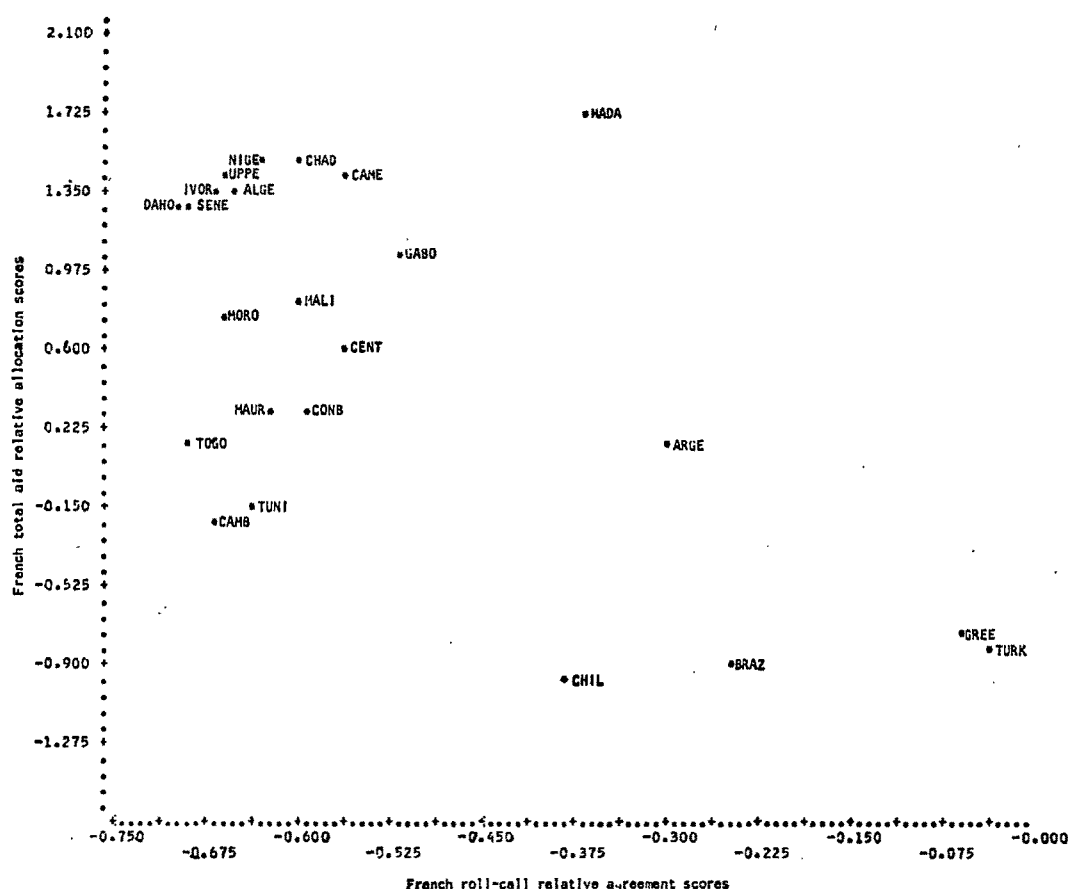


Figure 3. Relationship Between French Total Aid Relative Allocation Indices, 1964, and General Assembly Roll-Call Relative Agreement Scores with France, 1963

the one hand, it could be argued that "matters could be worse." This is very close to the notion suggested by Mason when he observes that "[d]iplomats are frequently worried by voting records in the United Nations and suggest that an increase in aid to their particular country would improve this record."⁵⁴ The implication of this argument is that aid does serve, at least minimally, as a form of influence by moderating the positions of developing nations in the UN; that without this aid, frequently high in relative terms, the positions of aid recipients would be even more antidonor (meaning in most cases anti-Western) than they already are.⁵⁵ The corollary of this notion is that those

states that appear generally more prodonor in terms of their voting preferences can be considered "safe."

On the other hand, it could also be argued that a large number of considerations and motivations undergird the aid programs of developed nations, of which the international political behavior of aid recipients in the UN is only one. Similarly, the UN voting behavior of developing nations is also conditioned by domestic as well as international political considerations other than those concerning foreign aid. Therefore we should not be surprised by the generally weak, albeit inverse, association between voting agreements and aid allocations observed for most donors. What this argument implies, of course, is the need for multivariate analysis of both voting preferences and aid allocations. But it also suggests that insofar as we can generalize from the present data and meth-

⁵⁴ Mason, p. 33.

⁵⁵ As Benjamin J. Cohen has put it in discussing the rationale for a U.S. development aid program, "In at least one fundamental respect . . . the promotion of the growth of poor countries is in our national interest: economic concessions on our part serve as a kind of insurance against belligerency on the part of the less developed countries" ("The Third World: The Issues," in *American Foreign Economic Policy*, ed.

Benjamin C. Cohen [New York: Harper and Row, 1968], p. 339).

odology, the preferable substantive interpretation of the findings seems to be that in terms of their allocation of foreign aid most donor countries are indifferent to the roll-call voting behavior of developing nations in the United Nations.⁵⁶ The reason for this may well be quite simple—the types of foreign policy positions and behavior represented by UN roll calls may be largely irrelevant to the central concerns of decision-making elites dealing with foreign aid allocations.

The exception to this generalization is the United States. Even here, however, care must be used. The fact that the results for the United States were found to be statistically significant might lead one to conclude that foreign aid is indeed used as an instrument of influence in the UN. But this is far from conclusive. It may be that the results reflect little more than the co-occurrence of two indicators of states' general foreign policy orientations, which means the question of a causal link between the indicators remains largely unanswered. Even in the case of the United States, then, additional and alternative lines of inquiry are in order.

Between-group differences. The final question to be addressed is whether the exclusion of nonrecipients of aid for each donor is in part responsible for the results discussed above. For this purpose, group *VRA* scores have been calculated for each donor's set of recipients and nonrecipients of aid in 1963 and 1966. The results of the analyses are displayed in Table 6.

Looking first at the United States, we see

⁵⁶ This conclusion must, of course, be used advisedly. From the perspective of aid donors, what is being said is if foreign aid donors are motivated to allocate their aid on the basis of how developing nations vote in the UN, then we would expect to find patterns of association different from those found for most donors. The United Kingdom is an illustrative case similar to France. In 1964, Kenya, Tanzania, and Uganda had among the highest aid *RA*'s of any British aid recipient. These states were also among those least prone to agree with Britain in the 1963 General Assembly. The result is an inverse association. If, however, a primary consideration was how these nations voted in the UN vis-à-vis the United Kingdom, then we would expect that Britain would not have allocated as much of its aid proportionately to these nations as it in fact did. Although it is generally the case that developing nations agree with most aid donors (notably DAC donors) relatively infrequently (i.e., virtually all DAC donors' *VRA*'s are negative), clearly some developing nations among those that Britain chose to aid in 1964 agreed relatively more often with the United Kingdom than either Kenya, Tanzania, or Uganda. Accordingly, our hypothesized relationship between aid and votes suggests that these other states should have received a relatively greater amount of British aid than they did.

that in 1963, the 68 recipients of U.S. aid as a group agreed with the United States 34 per cent less often than would be expected on the basis of the behavior of all members of the General Assembly. In comparison, the three nonrecipients agreed, as a group, 38 per cent less often than would be expected. In 1966, the corresponding group *VRA*'s indicate that the 71 recipients of U.S. aid agreed with the United States 34 per cent less often than would be expected, while the four nonrecipients agreed 59 per cent less often than would be expected.

If the hypothesis is correct that the exclusion of nonrecipients of aid results in a stronger relationship between aid allocations and voting agreements than would otherwise be the case, then we would expect the group *VRA* scores of the aid recipients to be substantially higher than the group *VRA*'s of the nonrecipients. In general, however, this is not the case. The difference of twenty-five percentage points between the recipients and nonrecipients of U.S. aid in 1966 is the largest difference (in the predicted direction) between the two percentages for any donor. But the number of nonrecipients is so small as to make this difference between the groups of questionable substantive interest. Other than the United States, the only differences approximating the same magnitude are those for Belgium and the Soviet bloc in 1966. In the case of the Soviet bloc the number of nations comprising the recipient group is so small as again to cast doubt upon the substantive meaning of the differences between the two groups.⁵⁷

For many of the remaining donors the difference between the two group *VRA*'s is slight; in fact, in ten of the remaining twenty cases the difference is less than six percentage points. Moreover, in half of all the cases based on the 1966 data, and in just under half of the cases based on 1963, the relative agreement scores of the group receiving aid from the donor are in fact less than the *VRA*'s for the nonrecipients. In general, then, exclusion of the nonrecipients of aid for each donor does not appear to qualify significantly the findings regarding the association—or lack of it—between the allocation of foreign aid and patterns of agreement in General Assembly voting. “Between-group” differences, in other words, do not appear to be more important than “within-group” differ-

⁵⁷ Because the Soviet bloc data are commitments rather than disbursements, this is the one donor for which group *VRA*'s could show more than minimal fluctuations from one year to the next if they were calculated for all six years included in the study.

Table 6. General Assembly Group Relative Agreement Scores of Developing Nations Receiving and Not Receiving Foreign Aid, 1963 and 1966^a

(Number of nations in each group shown in parentheses)

Donor Country	1963		1966	
	Group receiving total aid from donor	Group not receiving total aid from donor	Group receiving total aid from donor	Group not receiving total aid from donor
Australia	NA	NA	-35 (20)	-25 (58)
Austria	03 (16)	-06 (43)	-15 (32)	-21 (32)
Belgium	—	—	-21 (26)	-35 (52)
Canada	-17 (27)	-16 (47)	-26 (45)	-15 (29)
Denmark	—	—	-16 (25)	-14 (53)
France	-58 (20)	-50 (53)	-41 (26)	-42 (49)
Italy	-06 (20)	-16 (45)	-25 (44)	-21 (21)
Japan	00 (35)	-05 (38)	-04 (41)	-11 (37)
Netherlands	—	—	-22 (13)	-23 (65)
Norway	—	—	-15 (23)	-14 (55)
Sweden	-07 (23)	-05 (51)	-21 (23)	-14 (54)
United Kingdom	-42 (29)	-41 (42)	-45 (60)	-42 (15)
United States	-34 (68)	-38 (3)	-34 (71)	-59 (4)
Soviet bloc ^b	-02 (8)	-17 (66)	21 (12)	00 (66)

^a Recipients of negative foreign aid and recipients that missed more than 40 per cent of the Assembly roll calls are excluded. A dash indicates that the group receiving aid comprises less than eight Assembly members. NA indicates that foreign aid data are not available.

^b Soviet bloc members are Bulgaria, Czechoslovakia, Hungary, Poland and the Soviet Union in 1963; and Bulgaria, Czechoslovakia, Hungary, Poland, Rumania, and the Soviet Union in 1966.

ences in terms of roll-call agreements between developing nations and foreign aid donors.

Summary and Conclusion

A striking feature of the analysis is that the hypothesized association between aid and votes was found to hold only for the United States, and only in that portion of the analysis using deviations from idealized norms as the operational measures of aid allocations and voting agreements. Although positive associations were discerned for other donors as well, such as the Soviet bloc in 1964, for no aid donor other than the U.S. was the association found to be consistently strong over time. In some respects, then, the results support Keohane's observation that the use of extra-parliamentary instruments of influence in the United Nations is confined almost exclusively to the United States and the Soviet Union.⁵⁸ This appears to be the case even though for other donor countries (notably Britain and France vis-à-vis their former colonies) foreign aid is clearly part of a general dependency relationship between developed and developing nations. Apparently, then, the voting behavior of developing nations is largely irrelevant to the concerns of most foreign aid donors.

Even in the relationship between aid allocations and voting behavior observed for the United States, it is unclear which of the two variables should be considered a cause and which a consequence. With the possible exception of the analyses of Soviet bloc aid commitments, most of the results based on a lagged relationship between the variables examined have failed to point toward significantly different relationships between aid and votes within either of the two three-year periods analyzed.

Certainly the association between aid and votes is sufficiently strong in the case of the United States to warrant further research into the exact nature of the causal nexus—if, indeed, it exists at all. As suggested above, multivariate analysis would be a logical next step. Alternatively, longitudinal rather than cross-national analysis of some selected subset of developing nations might be pursued. Or one might attempt to measure the perceptions that UN participants have of the use of aid as influence. Whatever the particular strategy pursued, from the viewpoint of the student of international organization the results for the United

States do suggest that continued examination of the extraorganizational links between UN members may prove useful in describing and explaining the outcomes of the political processes in the United Nations.

A word of caution is in order, however. Although the results do show a statistically significant positive association, neither 10 per cent nor even 25 per cent covariation is sufficiently large to lead one to believe that extraorganizational links alone will suffice to explain organizational politics. Indeed, another researcher viewing these results may find here a case for directing greater attention to intraorganizational processes and influence patterns in an effort to explain political outcomes.⁵⁹ There is merit in this position.

Similarly, the foreign policy analyst could interpret the results for the United States as suggesting the need for continued examination of the impact of external variables on foreign policy outputs, for both developed and developing nations. When we move beyond the United States, however, the results also suggest that external variables—at least insofar as they are tapped in this study—may well be insufficient explanations of foreign policy behavior; that we may have to examine role, governmental, and societal variables⁶⁰ as well in order to explain foreign policy outcomes.

Should there be a relationship between foreign aid and United Nations votes? A dual interpretation of the findings can also be made by those interested in this normative question. Those who prefer that "appreciation" for foreign aid be reflected in the United Nations will find little solace in the generally moderate levels of association between aid and votes. Conversely, those who subscribe to the opposite view may well find discomfort in the fact that there is *any* association whatsoever between the variables, particularly considering that the association appears to be confined almost exclusively to the United States. Which perspective is adopted probably depends on one's view of

⁵⁸ The utility of examining intraorganizational processes is demonstrated in Chadwick F. Alger, "Interaction in a Committee of the United Nations General Assembly," in *Quantitative International Politics*, ed. J. David Singer (New York: The Free Press, 1968), pp. 51-84; and Kathleen Maas Weigert and Robert E. Riggs, "Africa and United Nations Elections: An Aggregate Data Analysis," *International Organization*, 23 (Winter, 1969), 1-19.

⁵⁹ The potential impact of these factors on foreign policy is discussed in James N. Rosenau, "Pre-theories and Theories of Foreign Policy," in *Approaches to Comparative and International Politics*, ed. R. Barry Farrell (Evanston, Ill.: Northwestern University Press, 1966), pp. 27-92.

⁶⁰ Because the Soviet bloc and DAC data do differ in kind, it seems reasonable to tentatively regard the absence of comparable results for the United States and the Soviet bloc (a surrogate for the Soviet Union) as at least partially attributable to the absence of comparable data.

the role of foreign aid as an instrument of influence and the role of the United Nations as an instrument of conflict management and resolution. One perspective is represented by Kaplan's normative position that "... the United States should not seek to starve a poor country into support for its foreign policy. An incentive system cannot operate in the case of every vote in the United Nations or every meeting of de-

veloping nations."⁶¹ The other perspective is represented by Westwood: "It seems likely that one of the major questions for the future is whether aid should not be included within the broad process of give and take on concrete issues which produces cooperation among mature, independent, and equal nations."⁶²

⁶¹ Kaplan, p. 248.

⁶² Westwood, p. 105.

Democratic Committee Assignments in the House of Representatives: Strategic Aspects of a Social Choice Process*

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Since Woodrow Wilson wrote *Congressional Government*, the dominance of the standing committees in the House of Representatives in determining legislative outcomes has been accepted by students of Congress. In light of recent studies of groups of House committees¹ and in-depth studies of individual committees,² the generalization about the central role of committees in the legislative process remains intact. These studies, however, indicate that there are major differences among these "little legislatures" in regard to their organization and behavior, and that these differences are in part a function of differences in their memberships. Furthermore, as Charles Clapp has said,

Not only is the fate of most legislative proposals determined in committee: to an important degree the fate of individual congressmen may be decided there too. A person's congressional career may rest largely on the kind of committee post he is given.³

Therefore, given the importance of committee membership, both in policy making and in

determining the success of individual congressmen's careers, the process by which members are assigned to committees is of the greatest importance.

A good deal of research has been devoted to the committee assignment process in the House.⁴ Most of this research, however, has been based on the *results* of the process, i.e., the committee assignments that were actually made. For example, the desirability of committees has been measured in terms of the proportion of the freshmen on each committee, or the number of transfers from a committee versus the number of transfers to it. The variable missing from such treatments has been the desires of the members. Also, success in achieving committee assignments has been treated in only the most general terms, again because of the absence of data on what committees members requested.

The purpose of this paper is to analyze committee assignments by focusing on this "missing link." Our subject will be the committee assignments of Democratic members of the House of Representatives. Committee assignments, as Clapp's observation above suggests, are valued, are in limited supply, and are allocated by

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¹ Richard F. Fenno, Jr., "Congressional Committees: A Comparative View," a paper presented at the 66th Annual Meeting of the American Political Science Association, Los Angeles, September 8-11, 1970; George Goodwin, Jr., *The Little Legislatures* (Amherst: University of Massachusetts Press, 1970).

² Richard F. Fenno, Jr., *The Power of the Purse: Appropriations Politics in Congress* (Boston: Little, Brown and Co., 1966); John Manley, *The Politics of Finance: The House Committee on Ways and Means* (Boston: Little, Brown and Co., 1970).

³ *The Congressman: His Work as He Sees It* (New York: Doubleday and Co., 1963), p. 207.

⁴ The classic treatment of the process is Nicholas A. Masters, "Committee Assignments in the House of Representatives," *American Political Science Review*, 55 (June, 1961), 345-357. Assignments are discussed at length in Clapp, *The Congressman*, pp. 207-240, and Goodwin, *The Little Legislatures*, pp. 64-79. Some recent studies include Louis C. Gawthrop, "Changing Membership Patterns in House Committees," *American Political Science Review*, 60 (June, 1966), 366-373; Charles Bullock and John Sprague, "A Research Note on the Committee Reassignments of Southern Democratic Congressmen," *Journal of Politics*, 31 (May, 1969), 493-512; Charles Bullock, "Correlates of Committee Transfers in the United States House of Representatives," a paper delivered at the Annual Meeting of the Midwest Political Science Association, Chicago, April 29-May 1, 1971; Charles Bullock, "The Influence of State Party Delegations on House Committee Assignments," *Midwest Journal of Political Science*, 15 (August, 1971), 525-546; and Charles Bullock, "Freshman Committee Assignments and Re-election in the United States House of Representatives," *American Political Science Review*, 66 (September, 1972), 996-1007.

rather well-defined mechanisms and procedures. By viewing committee assignments as the culmination of a special sort of allocation process, we shall be in a position to *explain* the results of this process, the *descriptions* of which have constituted the bulk of research on this topic to date. It is explanations we seek, and we find more general theories of social choice particularly well-suited for this purpose. More specifically, we examine the data of committee assignments in terms of actors in pursuit of personal goals, constrained only by scarcity and institutional procedures. We will consider the process from the point of view of the members requesting assignment and the members who make the assignments: who wants what committees, and whose requests are satisfied. The basic data which will be employed are the requests for committee assignments, by all House Democrats, made to the Democratic Committee on Committees in the 86th-88th and 90th Congresses.

Democratic Committee Assignments⁵

In the House, the Democratic Committee on Committees is made up of the Democratic members of the Ways and Means Committee.⁶ At the beginning of each Congress they are faced with the task of filling vacancies on the other standing committees of the House. Each member of the Committee is assigned a geographic zone, containing his own state and perhaps others, and he is responsible for handling requests for assignments from members in his zone.⁷

Some time after the November election, new Democratic congressmen submit to the Committee on Committees their requests, in order of preference, for committee assignments. At

the same time, returning congressmen may submit requests for second assignments or for transfers from committees they held in the previous Congress.

After members submit requests, lobbying for assignments begins. Many representatives seeking assignments write letters to some or all members of the Committee on Committees, setting forth arguments on their own behalf. Many also pay personal visits to the members of the Committee (especially their zone representative), to the Democratic leaders, and to the chairmen of the committees they are requesting. Often letters are written to members of the Committee on Committees to support the cause of some requesters. Typically these letters came from the deans of state delegations (either from themselves alone or on behalf of the whole delegation), from party leaders or office-holders outside the House, from committee chairmen and from leaders of interest groups relevant to the work of the committees requested.

At the beginning of the Congress, the size of committees as well as party ratios are set by negotiation between the leaders of both parties.⁸ Thus the members of the Committee on Committees are faced with the task of filling the vacancies on the standing committees which have resulted either from the failure of some Democratic members to return or from the creation of new positions. The Committee meets in executive session, and each of the committees is called up (usually alphabetically). For each committee the members proceed in order of seniority to nominate candidates from their zone.⁹ The names of nominees are written on a blackboard, and they are discussed by the members. After nominations are completed, the members vote by secret ballot.

Finally, after the Committee on Committees has filled all the vacancies, their decisions are placed before the Democratic Caucus for ratification. This is, however, almost always a *pro forma* action.

For our purposes, then, the time sequence of the committee assignment process, which culminates in the creation of a committee structure for a new Congress, may be characterized by the following stages:

⁵ While party ratios usually reflect the partisan division in the House, it is not unlikely that the decisions on both these questions are influenced by the leadership's knowledge of the requests of the members of their party.

⁹ It is important to note that the only way a requester can be nominated for a committee post is to be nominated by his zone representative.

⁵ In addition to our own data, this description relies heavily on Masters, "Committee Assignments," and on Clapp, *The Congressman*, Chapter 5.

⁶ The Democratic delegation on Ways and Means is made up almost entirely of members drawn from the South and Border States and from the large industrial states. They are elected to membership by a vote of the full Democratic caucus. During the period covered by this study the committee was dominated by the Southern and Border State group, who were greatly over-represented compared to their proportion of the Democratic delegation in the House. In the 86th-88th Congresses, they had 8 members out of the total of 15, and in the 90th Congress they had 7 of 15. For a discussion of assignments to Ways and Means, see Manley, *Politics of Finance*, pp. 22-38.

⁷ Zone assignments in the 86th Congress are listed in Masters, "Committee Assignments," p. 347. The number of states represented varies greatly, with Keogh of New York, for example, representing only his own state, while Metcalf of Montana represents seven small Western and Midwestern states.

(1) the committee configuration in the previous Congress;

(2) an election;

(3) requests for assignments by newly elected members and by returning members who held assignments in the committee configuration of the previous Congress;

(4) the establishment of size and party ratios for committees in the new Congress;

(5) committee assignments by the Democratic Committee on Committees for the new Congress.

Stage 1 provides initial conditions which are "disturbed" by an election (stage 2). The election disturbs the initial committee structure in several obvious ways. First aggregate party proportions in the chamber are altered, requiring the renegotiation of the committee structure by party leaders: committee sizes and party ratios are fixed at this stage (stage 4). Second, individual and aggregate election results effect a change in the opportunity structure in the chamber. Individual defeats of party members in the election create party committee vacancies (which may or may not be lost to the party depending on the aggregate election results and its effect on stage 4). At the outset of the new Congress, then, there are unfilled committee vacancies and demand for those slots by newly elected and returning members (as expressed at stage 3).

Our concern in this paper is with the process of committee assignments for the Democrats. We take the first, second and fourth stages of the process as exogenous to our concerns (though, as we suggested earlier, stage 4 may well be conceived as as endogenous, since it is probable that deviations from the aggregate party ratio on any given committee are partly a result of the configuration of requests at stage 3). Before getting into the details of our analysis, we should first outline the general theoretical context within which we view the requests of the members and the decisions of the Committee on Committees.

Member Goals and Committee Assignments

As the title of this paper implies, we view the committee assignment process as an instance of "social choice" (or "collective decision making"). That is, a group (the Committee on Committees) is charged with allocating the valuable resources (places on committees) of the collectivity (the Democratic members of the House). We view the participants in the process as rational actors, that is, actors who have goals that they want to achieve, and who, when confronted with a decision-making situa-

tion, examine the available alternatives and choose the alternative which seems most likely to lead to the achievement of those goals.

On the question of goals, Richard Fenno has argued that "of all the goals espoused by members of the House, three are most basic. They are re-election, influence within the House and good public policy. There are others; but research on the House acknowledges these to be the most consequential."¹⁰ If this view is accepted, we may now consider each of the participants in the assignment process and show how the outcomes of that process affect their goals.

(1) *Requesters*. The impact of committee assignments on the members requesting those assignments is so obvious that it hardly needs to be discussed. If a congressman holds any or all of the goals mentioned above, his committee assignment will have a substantial impact on the probability of achieving them.

A "good" assignment may greatly enhance his value to his constituents and provide unusual opportunities to publicize his activities in Congress; here he can develop the expertise and the reputation as a "specialist" that will enable him to influence his colleagues and important national policies.¹¹

(2) *The Committee on Committees*. The interest of members of the Committee on Committees in what assignments are made is less direct than that of the requesters, but important nonetheless. If a member has a general interest in the policy area of a particular committee, he may support the assignments of congressmen whose views are most congruent with his own. Furthermore, when a piece of legislation important to a member of the Committee on Committees comes before the House, he may call in debts owed to him by congressmen he has sponsored for committee assignments in order to influence such legislation.¹²

(3) *The Leadership*. The filling of committee vacancies is also important to the Democratic leadership, for much the same reasons as it is to the members of the Committee on Committees.

¹⁰ Fenno, "Congressional Committees," p. 3.

¹¹ Clapp, *The Congressman*, p. 207.

¹² It is clear that such debts are recognized, and that they are called in. One member, commenting on the influence another member had within the House, said, "Much of his power rests with the fact he is on Ways and Means. Since that Committee determines committee assignments, he is in a very important and strategic spot. He makes his deals with various groups as to which people he will support for certain spots. Naturally when the time comes that he wants something, he can make a request and people reciprocate." Quoted in Clapp, *The Congressman*, pp. 29-30.

Committee assignments are vital to the leadership in two ways. First, to the degree that the leadership affects assignments it has an important resource for doing favors for members, for rewarding members for past favors, and for establishing bonds with members that may provide some leverage in future legislative situations. Second, committee assignments are vital to the policy for which the leadership is responsible.¹³

(4) *State Delegations.* Members of a state delegation have an interest in other delegation members serving on committees which affect the interests of their state. Such members serve as a source of information about the committee's business and a resource for influencing the course of legislation on that committee.¹⁴

This discussion could be extended to include the committee chairmen and persons outside the House (e.g., interest groups), but the essence of our point is established. How committee slots are assigned is of vital interest to many individuals and groups, because those assignments will have an impact on the achievement of their goals within the House.

Data and Analysis

The empirical analysis, which constitutes the bulk of this current study, employs *request* data from four Congresses. In all, we have data on the committee requests, in order of preference, of 106 freshmen and 89 nonfreshmen Democratic representatives. It is important to reiterate the distinctiveness of these data. Ordinarily, analyses of committee assignments are based on the *results* of the process, from which inferences about the process are drawn.¹⁵ By using request data, however, we are in a position to assess the extent to which the process links requests on the one hand with final assignments on the other. Moreover, we may now determine who benefits and who is harmed by the assignment process, not in terms of so-called objective standards of "good" and "bad" assignments, but rather in terms of the subjective preferences of the actors affected. All these possibilities permit somewhat keener insights into the characteristics of this important internal process.

The first aspect of the assignment process we will consider is the requests themselves: what committees are requested most often, and how many requests are made. For this part of the

analysis we will consider only the requests of freshman Congressmen. Nonfreshmen already hold committee assignments and, because of service restrictions,¹⁶ are prevented from applying for certain committees (unless they are trying to transfer from the committee they hold).¹⁷

Table 1 presents these data on freshman requests. The first column gives the total number of requests for each of the standing committees. The second column gives the percentage of all requesters who applied for a slot on each of the committees. The last column indicates the percentage (and number) of all requests that were the requesters' first choices. Thus, 15 congressmen (nearly 14 per cent of all freshmen) requested Agriculture Committee slots. It may be further noted, however, that of these 15 requests, 7 were first preference requests.¹⁸

Although this table does indicate the distribution of preferences for the various standing committees, consideration of aggregate demand to determine committee desirability is potentially misleading. First, the distribution of pref-

¹⁶ The standing committees of the House are divided into three classes: exclusive, semi-exclusive, and non-exclusive, and there are rules which govern assignments to each class. Members of exclusive committees may serve on no other committee. Members of semi-exclusive committees may be given a second assignment only on a nonexclusive committee; and members of nonexclusive committees may be given second assignments on any semi- or nonexclusive committee. During the period covered by this study the committees in these classes were: *exclusive*: Appropriations, Rules, and Ways and Means; *semi-exclusive*: Agriculture, Armed Services, Banking and Currency, Education and Labor, Foreign Affairs, Interstate and Foreign Commerce, Judiciary, Public Works, and Science and Astronautics; *nonexclusive*: District of Columbia, Government Operations, House Administration, Interior and Insular Affairs, Merchant Marine and Fisheries, Un-American Activities, and Veterans Affairs. In addition, Post Office and Civil Service was changed from semi-exclusive to non-exclusive status at the beginning of the 88th Congress. Finally, the twenty-first standing committee, Standards of Official Conduct (established in 1967) seems to occupy a special status since it is a third assignment for a number of members.

¹⁷ We do, however, include the requests of two nonfreshmen who were first elected in special elections near the end of a Congress. They were not assigned to any committees then, and thus they are in the same position as freshmen.

¹⁸ The reader will note that only 18 committees are listed in Table 1. Ways and Means is excluded here, and throughout the rest of the analysis, because its vacancies are filled by the caucus. Rules and Standards of Official Conduct are excluded because they were never requested, by either freshmen or nonfreshmen. In regard to Rules, this absence of requests probably reflects the special importance this committee has to the leadership, since almost never is a vacancy on Rules filled by a member not first sponsored by the leadership. See Clapp, *The Congressman*, p. 218; and Manley, *Politics of Finance*, p. 77.

¹³ Manley, *Politics of Finance*, p. 24.

¹⁴ See Bullock, "The Influence of State Party Delegations."

¹⁵ Most of the individual committee studies (e.g., Fenno, Manley) also draw on interviews with committee members to generalize about what led them to seek assignment to the committee and why they were successful.

Table 1. House Committee Requests and First-Choice Preferences of Freshmen^a

Committee	Total Requests	Percentage Requesting ^b	Percentage of Requests First Choice (N) ^c
Agriculture	15	14	47 (7)
Appropriations	12	11	42 (5)
Armed Services	26	24	26 (12)
Banking and Currency	34	32	41 (14)
District of Columbia	3	3	0 (0)
Education and Labor	17	16	29 (5)
Foreign Affairs	23	21	61 (14)
Government Operations	8	7	13 (1)
House Administration	1	1	0 (0)
Interior and Insular Affairs	20	19	50 (10)
Interstate and Foreign Commerce	34	32	38 (13)
Judiciary	25	23	36 (9)
Merchant Marine and Fisheries	5	5	20 (1)
Post Office and Civil Service	13	12	23 (3)
Public Works	29	27	31 (9)
Science and Astronautics	15	14	33 (5)
Un-American Activities	2	2	0 (0)
Veterans Affairs	8	7	13 (1)

^a Number making requests = 108.

^b Since requesters submitted multiple requests, this column sums to more than 100%.

^c The number totals to 109 because one requester asked for "Foreign Affairs or Agriculture," and so both requests were considered first choices.

erences reflects the *opportunity structure* as well as the preference structure, so that observations about committee popularity on this basis are necessarily ambiguous.¹⁹ Second, even if we discount the effects of opportunity, the aggregate demand for a committee vacancy may not accurately reflect the intensity of preference for that vacancy. To take one example, although aggregate demand for Agriculture and Post Office-Civil Service is approximately the same (14 per cent and 12 per cent, respectively), there is a great difference in the proportion of those requests that are first choices (47 per cent and 23 per cent, respectively).

More important than the two previous points, however, is that attempts to provide an aggregate measure of committee desirability are inconsistent with the conception of the legislator's behavior as goal-directed. All congress-

men are not the same. Their goals differ, and the kinds of constituencies they represent vary. Thus, we would expect committees to have *differential* appeal to different types of congressmen.

The next two tables lend credence to this expectation. While it is difficult to measure the personal goals of legislators, it is probably safe to assume that most freshmen are initially concerned with firming up relationships with their constituencies. This is probably a minimal requirement for the pursuit of policy goals and internal House influence, and is, of course, directly relevant to the goal of reelection. Thus, we can classify members according to the kinds of districts they represent in order to demonstrate the extent to which "committee popularity" varies with constituency types.

In Table 2 we have classified districts according to region and population per square mile. Although the classification is crude and the choice of variables somewhat arbitrary,²⁰ this

¹⁹ Thus a problem with using requests or appointments as a measure of committee desirability results from anticipated reactions. A freshman will probably refrain from requesting the committee he most wants if he believes there is no chance of getting it. Table 1 seems to support this view in regard to requests for Appropriations, which (along with the other two exclusive committees) is generally recognized to be the most sought-after committee. It is, however, also recognized that freshmen have little chance to be appointed. (Indeed, in the four Congresses analyzed here, no freshman was appointed.) There does not, however, seem to be any evidence of anticipated reaction in regard to requests for other committees.

²⁰ Differences among districts on a regional basis are well known, hence the selection of that variable. Regions are defined as follows: *Northeast*: Conn., Del., Me., Mass, N.H., N.J., N.Y., Penn., R.I., Vt.; *Border*: Kent., Md., Mo., Okla., W.Va.; *South*: Ala., Ark., Fla., Ga., La., Miss., N.C., S.C., Tenn., Tex., Va.; *Midwest*: Ill., Ind., Ia., Kan., Mich., Minn., Neb., N.D., Ohio, S.D., Wisc.; *West*: Alaska, Ariz., Cal., Colo., Haw., Ida., Mont., Nev., N.M., Ore., Utah, Wash., Wyo.

Population per square mile was selected to tap the relatively urban or rural nature of districts. While per-

Table 2. House Committees Most Requested by Freshmen
(Controlling for Region and Population Density)

Region	Density ^a	Committee	Percentage Requesting
South (N=18)	Sparse	Interstate Comm.	45
		Banking and Curr.	39
		Public Works	33
Midwest (N=7)	Sparse	Interior	57
		Interstate Comm.	57
		Public Works	43
West (N=16)	Sparse	Interior	75
		Public Works	38
East (N=8)	Medium	Armed Services	50
South (N=9)	Medium	Interstate Comm.	44
		Foreign Affairs	33
Midwest (N=9)	Medium	Judiciary	44
		Public Works	44
		Ed. and Labor	33
		Interstate Comm.	33
East (N=15)	Concentrated	Banking and Curr.	40
		Interstate Comm.	40
		Judiciary	33
West (N=7)	Concentrated	Banking and Curr.	71
		Interstate Comm.	71
		Ed. and Labor	57
		Foreign Affairs	43

^a See footnote 20 for definitions.

scheme suffices to exhibit the very real differences in committee appeal. We have listed those committees requested (at any preference level) by 30 per cent or more of the members in each category containing at least seven requesters.²¹ Immediately one notes that com-

percentage urban would have been a preferable measure in this regard, such data were available on a district basis only for the 88th and 90th Congress. To operationalize this variable the districts were divided into three categories: sparse (less than 100 persons per sq. mi.), medium (100-999 persons per sq. mi.), and concentrated (1,000 or more persons per sq. mi.). Population data were obtained from *Congressional District Data Book: Districts of the 87th Congress* (for the 86th and 87th Congresses) and from *Congressional District Data Book: Districts of the 88th Congress* and its supplements (for the 88th and 90th Congresses). Both books and the supplements are published by the Bureau of the Census.

²¹ The excluded categories and the number of members in each were: East, sparse (3); Border, sparse (4); Border, medium (5); West, medium (3); Border, concentrated (0); South, concentrated (0); Midwest, concentrated (4).

mittees differ greatly in their relative attractiveness to the various groups of representatives. Interior is one striking example. That committee was the most requested for both Midwestern and Western congressmen from sparsely populated districts. These members, moreover, accounted for 80 per cent of the total requests for that committee (16 of 20). The reason is clear: Congressmen from these constituencies can probably serve the interests of their districts better on that committee than on any other. In the words of one Western representative on the committee:

I was attracted to it, very frankly, because it's a bread and butter committee for my state. I guess about the only thing about it that is not of great interest to my state is insular affairs. I was able to get two or three bills of great importance to my state through last year. I had vested interests I wanted to protect, to be frank.²²

²² Quoted in Fenno, "Congressional Committees," p. 6.

Table 3. Requests by Freshmen for Selected House Committees

(Controlling for Interesteds and Indifferents)^a

	Requested		Not Requested		Total		Percentage of Requests by Interests
	N	%	N	%	N	%	
<i>Banking and Currency</i>							
Interests	17	47	19	53	36	100	65
Indifferents	9	20	36	80	45	100	
Total	26	32	55	68	81	100	
<i>Education and Labor</i>							
Interests	11	30	25	70	36	100	79
Indifferents	3	7	42	93	45	100	
Total	14	17	67	83	81	100	
<i>Interior and Ins. Aff.</i>							
Interests	11	65	6	35	17	100	69
Indifferents	5	8	59	92	64	100	
Total	16	20	65	80	81	100	
<i>Armed Services</i>							
Interests	9	30	21	70	30	100	50
Indifferents	9	18	42	82	51	100	
Total	18	22	63	78	81	100	
<i>Agriculture</i>							
Interests	11	33	22	67	33	100	92
Indifferents	1	2	47	98	48	100	
Total	12	15	69	85	81	100	

^a See footnote 23 for definitions.

Other specific examples may be cited. Public Works is requested by 17 of the 41 representatives from sparsely populated districts in the South, Midwest, and West, and they account for 59 per cent of the requests for that committee. Banking and Currency, part of whose jurisdiction includes housing legislation, was the most requested committee by members from districts with concentrated populations in both the East and West.

Thus, as these data demonstrate, there is indeed a difference in the attractiveness of committees to various groups of representatives, and in most instances there is a clear relationship between the type of district represented and the committees most requested.

Table 3 provides even more direct evidence on this score. For five selected committees, we partitioned members into two groups—interesteds and indifferents—depending on constituency characteristics.²³ Although the relation-

²³ These committees were selected because of the availability of constituency data which seemed reason-

ably related to representatives' probable interest in the committee. We borrowed the terms "interested" and "indifferent," and the basic ideas on measuring interest, from Charles Bullock (see "Correlates of Committee Transfers," pp. 22-23), although our specific measures are somewhat different. Bullock, in turn, adopted the terms from David R. Mayhew, *Party Loyalty Among Congressmen* (Cambridge: Harvard University Press, 1966).

Data on constituency characteristics were obtained from the *Congressional District Data Book: Districts of the 88th Congress* and its supplements. In this source data were available on freshmen representatives in the 86th and 87th Congress only if they came from states which were not redistricted after the 1960 census. Therefore the number of members considered in Table 3 is 81 instead of 108.

For each committee a constituency measure was selected, and each representative's district was ranked as either above or below the national average on that measure. If the district was above the national average, the congressman was classed as an interested; if the district was below the national average, the congressman was classed as an indifferent. The measures of the committees are as follows (national averages are in parentheses); Banking and Currency and Education and Labor, percentage of population residing in urban areas

ship between ascribed interest and request behavior varies from committee to committee, it is always in the predicted direction and quite strong. The ratio of the proportion of interests applying for a committee to the proportion of indifferents applying varies from a low of nearly 2 to 1 (Armed Services) to a high of more than 16 to 1 (Agriculture). Moreover, a high proportion of requests for each committee is accounted for by interested requesters.²⁴

These findings lead us to the following conclusions: Since the attractiveness of a committee does vary from member to member, a broad-gauged, systemic property like "committee desirability" may not be appropriate for an understanding of congressional behavior. A few committees (e.g., Appropriations, Ways and Means) may be almost universally desired, but beyond these few, the attractiveness of a committee, and the value of an assignment to it, may depend solely on the interests and preferences of the member under consideration. While to an urban congressman an assignment to Agriculture might be viewed as disastrous, a farm-belt member might prefer it second only to an appointment to Appropriations.

To this point we have been characterizing the empirical request configuration. Before we turn to the data on assignments proper, we should report several additional features of this configuration as it relates to the opportunity structure of the House.

Clearly the decision about *which* committees to request is a complex strategic one for a member to resolve. Additionally, however, he must decide the strategic question of *how many* committees to include in his preference ordering. In many instances (at least with regard to freshmen) the final preference ordering submitted comprises the entire "information environment" of the Committee on Committees. In any event, it does provide one of the few pieces of hard data on members' desires. The extent to which a goal-directed member chooses to vary that information environment, then, becomes an important strategic consideration. (Shortly we examine the relationships among length of

(69.9 per cent); Interior and Insular Affairs, land area of district (8,159 sq. mi.); Armed Services, percentage of the total labor force who are members of the armed forces (2.5 per cent); and Agriculture, percentage of the employed civilian labor force employed in agriculture (6.6 per cent).

²⁴ The proportion of Armed Services requests made by interested is by far the lowest. This is probably because interest in this committee is determined by other things besides a relatively large number of servicemen in a district (e.g., large defense plants or a hope of attracting defense bases or plants to a district in the future).

Table 4. Number of Requests by Freshmen and Nonfreshmen^a

Number of Requests	Freshmen		Nonfreshmen	
	N	%	N	%
1	24	22	71	82
2	15	14	9	10
3	49	45	3	3
4 or more	20	19	4	5
Total	108	100	87	100

^a Here also the two unassigned nonfreshmen are included with the freshmen (see note 17).

preference ordering, assignment success, and other features of the opportunity structure.)

Initially we consider this decision as faced by freshmen and nonfreshmen. The strategic problems facing these two groups are very different. The freshman has no committee assignment at all. He desires a good committee, but he knows for certain that he will be assigned to *some* committee. That is, any committee assignment is possible, the range of alternatives open to the Committee on Committees is maximal, and the member is entirely at the Committee's mercy. The nonfreshman, on the other hand, already has a committee, one which he may keep as long as he wishes. Thus, he need not worry about being given a committee less desirable than the one he holds.

Therefore, if the member assumes that, circumstances permitting, the Committee on Committees will attempt to satisfy his request (obviously he must assume this, or else it would be irrational to bother making any requests at all), the two groups are likely to follow different strategies regarding the number of requests they make. Freshmen are likely to offer the Committee on Committees a wider range of alternatives (i.e., make more requests), while nonfreshmen will probably be much more specific in their requests. As Table 4 shows, the data are in accord with these expectations. Only 22 per cent of the freshmen make only one request, while 82 per cent of the nonfreshmen do so.

Within the freshman category in Table 4, there is considerable variation in the number of requests.²⁵ Some of this variation in request behavior reflects variations in opportunities con-

²⁵ We restrict the following discussion to freshmen because the argument about a nonfreshman's ability to be more specific in his requests applies as well here as it did above.

Table 5. Number of House Committee Requests by Freshmen^a
(Controlling for State Vacancy and State Competition on First-Choice Committee)

Number of Requests	State Vacancy				No State Vacancy			
	State Compet.		No State Compet.		State Compet.		No State Compet.	
	N	%	N	%	N	%	N	%
1	0	0	5	42	1	9	18	26
2	0	0	4	33	3	27	7	10
3	8	67	1	8	4	36	32	47
4 or more	4	33	2	17	3	27	11	16
Total	12	100	12	100	11	99 ^b	68	99 ^b

^a Five members whose most-preferred committee had no vacancies are omitted here.

^b Error is due to rounding.

fronting members.²⁶ The data of Table 5 are wholly consistent with this hypothesis. In this table we look at two features of the opportunity structure (unfortunately, our total N is too small to permit the meaningful introduction of other relevant features). The first is whether a member is confronted by competition for his first-choice committee by another member of his state delegation. Since the only way a member can get assigned to a committee is to be nominated by his zone representative, a member's probability of getting a particular assignment is substantially reduced if the zone representative or the Committee on Committees must choose among two or more members from the same state. Therefore, we would expect a member to "hedge his bets"—that is, make a greater number of requests—if another member from his state is requesting his most-preferred committee.

The second feature of the opportunity structure that may influence the decision concerning the number of requests a congressman makes is the source of vacancies. It is apparent that at least the large states regard themselves as entitled to one or more seats on important committees, and that members, when requesting assignments, feel that a claim made on "their state's" vacancy is a persuasive argument in their favor.²⁷ Thus a member whose most preferred committee (as revealed by him in his preference ordering) contains a state vacancy

may be more likely to list few options, *ceteris paribus*, than other members.

As Table 5 reveals, when a state vacancy exists, the presence of state competition makes a great deal of difference in the number of committees requested. With state competition, no member makes fewer than three requests, but without state competition, 75 per cent do so. When no state vacancy exists, state competition has no effect; 36 per cent make fewer than three requests with state competition, and 36 per cent make fewer than three requests without it. (There is a large difference, in the predicted direction, in the number that make a *single* request.) Furthermore, in the absence of state competition, a member is much more likely to submit fewer than three requests if there is a state vacancy than if there is not (75 per cent versus 36 per cent). Thus, as we expected, both the existence of state competition and the presence of a state vacancy exert an independent impact on the number of committees requested by freshmen.

So far, we have examined the committee assignment process from the point of view of the requester. We now consider who is successful in getting the assignments he requests, viewing the process in terms of the goals of the members of the Committee on Committees and the leadership.

Above we stated that the member must assume that, if circumstances permit, the Committee on Committees will try to satisfy his request. We also argued that the number of requests a member makes depends on certain strategic considerations, and thus we implicitly argued that a member would think that his probability of getting *some* requested committee depends (at least in part) on the number of requests made. In Table 6 we present data re-

²⁶ Here we consider opportunities to depend upon certain objective conditions, such as number of vacancies for a given committee (supply), number of requests (demand), and service restrictions regarding dual requests (formal rules), and upon informal norms which may guide the allocation of committee vacancies, such as "same-state" norms for appointment.

²⁷ See Bullock, "The Influence of State Party Delegations," and Clapp, *The Congressman*, pp. 220, 238.

Table 6. Number of House Committee Requests and Assignment Success
(Freshmen and Nonfreshmen)

<i>A. Freshmen</i>								
Number of Requests	Member Received							
	First Choice		Other Choice		No Choice		Total	
	<i>N</i>	%	<i>N</i>	%	<i>N</i>	%	<i>N</i>	%
1	18	75	—	—	6	25	24	100
2	6	40	5	33	4	27	15	100
3	16	34	15	32	16	34	47	100
4 or more	9	45	8	40	3	15	20	100
Total	49	46	28	26	29	27	106	99 ^a

<i>B. Nonfreshmen</i>								
Number of Requests	Member Received							
	First Choice		Other Choice		No Choice		Total	
	<i>N</i>	%	<i>N</i>	%	<i>N</i>	%	<i>N</i>	%
1	32	45	—	—	39	55	71	100
2	1	11	5	56	3	33	9	100
3 or more	2	22	5	56	2	22	9	100
Total	35	39	10	11	44	49	89	99 ^a

^a Error is due to rounding.

lating to these points. Freshmen and nonfreshmen are treated separately to prevent seniority from contaminating the results.

When the data are examined we find that in regard to freshmen our first statement is more correct than our second. That is, almost three-fourths of the freshmen do get *some* committee that they requested, and the probability of getting *no* choice is affected little by the number of requests. For nonfreshmen, on the other hand, more requests do increase chances of some success.

The most striking finding in Table 6, how-

ever, results from our treating freshmen and nonfreshmen separately. Contrary to what one might expect in a body in which seniority is often important, freshmen are much more successful in getting requested assignments than are nonfreshmen. This finding is reinforced by the data in Table 7, which shows assignment success by amount of seniority. We find that the probability of receiving *no* request monotonically *increases* as seniority increases.

This result should not be entirely unexpected, for many references in the literature on committee assignments claim that seniority is

Table 7. Seniority and Success in Assignment to House Committees

Number of Previous Terms	Member Received							
	First Choice		Other Choice		No Choice		Total	
	<i>N</i>	%	<i>N</i>	%	<i>N</i>	%	<i>N</i>	%
0	49	46	28	26	29	27	106	99 ^a
1	19	41	8	17	19	41	46	99 ^a
2	9	43	1	5	11	52	21	100
3 or more	7	32	1	5	14	64	22	100 ^a
Total	84	43	38	20	73	37	195	100

^a Error is due to rounding.

Table 8. State Competition, Seniority, and Success in House Committee Assignments
(All Committees with Vacancies)

	Member Assigned		Member Not Assigned		Total	
	N	%	N	%	N	%
No Same State Competition	103	36	187	64	290	100
Same State Competition; Competitor Not More Senior	20	26	56	74	76	100
Same State Competition; Competitor More Senior	1	7	13	93	14	100
Total	124	33	256	67	380	100

often ignored when circumstances dictate. For example, Clapp claims that

given a contest for an important committee assignment, in which returning members of Congress may wish to transfer from another committee and find themselves competing with each other and with freshmen congressmen, seniority is not infrequently brushed aside, if it will not bring about the outcome desired by those making the decision.²⁸

Another observer of the process stated:

Seniority may control if all other things are equal. But other things usually are not equal. Sometimes you begin to think seniority is little more than a device to fall back on when it is convenient to do so.²⁹

To assess the accuracy of this last statement, we examined a situation where it is more likely for "other things" to be "equal": the case when two members from the same state are competing for the same committee. We looked at all committees that had vacancies, and determined for each requester whether another member from the same state was applying for that committee, and if so, whether the other member was more senior. The relevant data are presented in Table 8. It is clear that a request for

which there is no same state competition has a likelihood of about one chance in three of being granted. When there is competition, however, a member has about one chance in four of success if his competitor is not more senior, while he has only one chance in fourteen if the competitor is more senior. Thus in this instance, where "other things" are more likely to be "equal," seniority may become very advantageous, although it is generally not.

What then are the things that in other situations are *not* "equal"? That is, in terms of the goals of members who determine committee assignments, what factors cause them to ignore seniority? Clearly, many kinds of interacting and even conflicting motivations may influence the members of the Committee on Committees. Thus it is dangerous to posit either a single motive for all members of the Committee on Committees or even for any one member. Still, it is not unreasonable to assess the extent to which the data support any of these motives.

The first motive or goal for Committee on Committees members we have posited might be termed the *management goal*. The Committee on Committees, in this view, is concerned solely with satisfying requester demands. Thus, it acts as an "impersonal" preference aggregation device in an effort to keep requesters happy. In reporting on this motive, we find it useful to contrast it with another, the *constituency interest goal*. According to this goal, committee makers ignore request data, concerning themselves instead with matching individual members to committee vacancies on the basis of constituency characteristics and interests.

The data relevant to this comparison are presented in Table 9. For the committees investigated in Table 3 we partitioned freshmen³⁰

²⁸ Clapp, *The Congressman*, pp. 226-227.

²⁹ Quoted in Clapp, *The Congressman*, p. 226. We should note here that the data in Tables 6 and 7 indicate only overall success and not success in those instances where the requests of freshmen and nonfreshmen are in direct competition. Masters states that "when two or more members stake a claim to the same assignment, on the ground that it is essential to their electoral success, both party committees usually, if not invariably, will give preference to the member with longer service" (Masters, "Committee Assignments," p. 354). We do not know what arguments were made about electoral success, but in our data 50 nonfreshmen were in competition with one or more freshmen for assignments to semi- or nonexclusive committees which had insufficient vacancies to satisfy all requests. Of these 50 members, 23 were passed over in favor of freshmen. This does not include instances where the passed-over member received another, more preferred, assignment.

³⁰ We restrict our attention here to freshmen not only because of the effects of seniority on both request behavior and success, but also because the situation is much more complex in regard to nonfreshmen. Mini-

Table 9. Interesteds, Requests, and Success in House Committee Assignments^a
(Freshmen)

Interested ^b Requested	Yes Yes	Yes No	No Yes	No No	Total
<i>Committee</i>					
Banking and Currency	47 (17)	0 (19)	33 (9)	3 (36)	15 (81)
Education and Labor	64 (11)	4 (25)	67 (3)	2 (42)	14 (81)
Interior and Ins. Aff.	64 (11)	17 (6)	20 (5)	3 (59)	14 (81)
Armed Services	33 (9)	0 (21)	11 (9)	2 (42)	6 (81)
Agriculture	27 (11)	14 (22)	100 (1)	4 (47)	11 (81)

^a Each cell gives percentage of *N* assigned to that committee (*N* in parentheses).

^b See footnote 23 for definition of interesteds.

members according to two criteria: whether they qualify as interested and whether they requested the committee. The results suggest that the management goal dominates the constituency interest goal. In most instances (the small *N*s make firm conclusions difficult), interested-requesters get the nod. Moreover, for every committee, requesting-indifferents are more successful than nonrequesting-interesteds. Minimally, we conclude that committee makers do not take member requests lightly—that, in requesting a committee, a member is not waltzing before a blind audience.

A third obvious candidate as a motivational hypothesis for members of the Committee on Committee in *party maintenance*. Whether a member is interested in influencing policy outcomes or in influence for its own sake, one thing that in part determines the amount of such influence is the majority or minority status of his party. A member has more influence if he is Speaker than if he is Minority Leader, more influence if he is chairman of a committee than if he is ranking minority member, and probably more influence if he is on the majority side of the committee than if he is in the mi-

nority. Therefore it is in the interest of most Committee members to help insure the reelection of party colleagues. Thus the question arises, who needs the most help? Clearly, the members most in need of help are those who were elected by the smallest margin. *Ceteris paribus*, a member elected with 51 per cent of the vote is more likely to be defeated than one elected with 61 per cent. Thus, we would expect members from marginal districts to be more successful than members from safe districts. (We have termed a district marginal if the member was elected with less than 55 per cent of the vote.)

As the data in Table 10 show, our expectations are correct. Marginal freshmen are slightly less likely to fail to receive a requested committee and are much more likely to receive their first choice than are safe freshmen.

Marginal nonfreshmen are *much* less likely to receive no choice than are safe nonfreshmen, but they are about equally likely to receive their first choice as are safe nonfreshmen. We also find that some of the difference in success between freshmen and nonfreshmen disappears here—that is, safe freshmen are treated about the same as marginal nonfreshmen. When one recalls that these nonfreshmen, even though they are marginal, have demonstrated (at least once) their ability to get re-elected, it does not seem surprising that these two groups are about equally successful.

The perceived stand of the individual member on specific issues is yet another basis on which committee makers may determine as-

minally we would have to control for whether a representative is already a member of a committee for which he would be classified as an interested. Further, we would probably want to control for whether the requests made are for transfers or dual service assignments. Also we would want to exclude prestige committee requests. These controls would make the *N*s so small and would break them into so many categories that a meaningful test would be impossible.

Table 10. Margin of Election and Success in House Committee Assignments
(Freshmen and Nonfreshmen)

A. Freshmen								
District is	First Choice		Member Received		No Choice		Total	
			Other Choice					
	N	%	N	%	N	%	N	%
Marginal Safe	31	56	10	18	14	25	55	99 ^a
	18	35	18	35	15	29	51	99 ^a
Total	49	46	28	26	29	27	106	99 ^a
B. Nonfreshmen								
District is	First Choice		Member Received		No Choice		Total	
			Other Choice					
	N	%	N	%	N	%	N	%
Marginal Safe	7	39	5	28	6	33	18	100
	28	39	5	7	38	54	71	100
Total	35	39	10	11	44	49	89	99 ^a

^a Error is due to rounding.

signments.³¹ Their prime concern here is the degree to which members will support the party position on issues which come before the standing committees (*party support goal*). While detailed considerations of individual committees are beyond the scope of an initial study such as ours, two pieces of evidence may shed some light on this question. Generally speaking, southern Democrats are less likely to support their party's position than are northern Democrats. Therefore, whether we consider the granting of a specific request a reward for past behavior or an attempt to gain influence over future behavior, the leadership should be less likely to intervene with the Committee on Committees on behalf of a southerner than a northerner. Moreover, since the Democratic members of the Ways and Means Committee are more liberal and more supportive of their party than is the average House Democrat,³² those members, even when acting on their own, are likely to be more favorably disposed toward the requests of northern Democrats than toward those of southerners. Therefore, we would expect southerners to be less successful than northerners in obtaining requested committees.

As Table 11 shows, the data support this hypothesis. For both freshmen and nonfreshmen, southerners are more likely than nonsoutherners to receive *no* request. They are less successful than their colleagues from other regions in getting their first choices as well. One might wonder, however, whether this finding of lesser success among southerners does not simply repeat our finding concerning marginal versus safe districts. That is, since southerners are more likely to come from safe districts,³³ they may be less successful than nonsoutherners simply because they are elected by larger margins. To examine the possibility we present in Tables 12 and 13 data on region, margin of election, and assignment success for freshmen and nonfreshmen, respectively.

Since there are few marginal freshmen southerners and no marginal nonfreshmen southerners, we will focus our discussion on members from safe districts. As the data demonstrate, even when marginal members are removed, southerners are less successful than are nonsoutherners. Among both freshmen and

³¹ In addition to our discussion of member goals above, see Masters, "Committee Assignments," pp. 354-355, and Clapp, *The Congressman*, pp. 228-230.

³² See Fenno, "Congressional Committees," pp. 33-34, and Manley, *Politics of Finance*, pp. 29-32.

³³ Using a different definition of safe seats, Wolfinger and Hollinger found that while southerners held only 38 per cent of the Democratic seats in the 88th Congress, they held 63 per cent of the safe seats. See Raymond E. Wolfinger and Joan H. Hollinger, "Safe Seats, Seniority, and Power in Congress," in *New Perspectives on the House of Representatives*, ed. Robert L. Peabody and Nelson W. Polsby, 2nd ed. (Chicago: Rand McNally, 1969), pp. 60-61.

Table 11. Region and Success in House Committee Assignments
(Freshmen and Nonfreshmen)

A. Freshmen								
Region ^a	Member Received							
	First Choice		Other Choice		No Choice		Total	
	N	%	N	%	N	%	N	%
South	8	31	7	27	11	42	26	100
Non-South	41	51	21	26	18	23	80	100
Total	49	46	28	26	29	27	106	99 ^b

B. Nonfreshmen								
Region ^a	Member Received							
	First Choice		Other Choice		No Choice		Total	
	N	%	N	%	N	%	N	%
South	7	27	2	8	17	65	26	100
Non-South	28	44	8	13	27	43	63	100
Total	35	39	10	11	44	49	89	99 ^b

^a The South includes the eleven states of the Confederacy.

^b Error is due to rounding.

nonfreshmen, safe southerners are more likely to receive no request than are safe nonsoutherners, and are less likely to receive a first request. The evidence is unequivocal: Region has a strong, independent impact on committee assignment success, even when election margin is controlled.³⁴

The second piece of evidence bearing on the

difference between the success of members who are from the same state as their zone representative and that of members who are not.

Table A. Assignment Success, Region, and
Zone Representative's State Delegation

	Member Received							
	First Choice		Other Choice		No Choice		Total	
	N	%	N	%	N	%	N	%
<i>A. Freshmen, Non-South</i>								
Same State ^a	16	52	8	26	7	23	31	101 ^b
Not	25	51	13	27	11	22	49	100
Total	41	51	21	26	18	23	80	100
<i>B. Freshmen, South</i>								
Same State	5	46	4	36	2	18	11	100
Not	3	20	3	20	9	60	15	100
Total	8	31	7	27	11	42	26	100
<i>C. Non-Freshmen, Non-South</i>								
Same State	13	46	3	11	12	43	28	100
Not	15	43	5	14	15	43	35	100
Total	28	44	8	13	27	43	63	100
<i>D. Non-Freshmen, South</i>								
Same State	3	30	2	20	5	50	10	100
Not	4	25	0	0	12	75	16	100
Total	7	27	2	8	17	65	26	100

^a Same State means that the member making the request and his zone representative are from same state.

^b Error due to rounding.

³⁴ This finding is all the more striking in light of the southern Democrats' dominance of the Committee on Committees during this period (see footnote 6). It is, admittedly, dangerous to treat a variable like region as a surrogate for, in this case, expected policy behavior. Support for party policies clearly varies within regions. Moreover, other informal, nonpolicy, behavioral norms are likely to cloud the relationship between region and appointment success. A clear indication of this intra-regional variation is the rather startling differential in success between members from the South and those from outside the South when we control for the state of their zone representative. As the data in Table A show, the southern member who is from the same state as his zone representative is much more likely to secure a requested assignment than are his southern colleagues who are not from the same state as their zone representatives. Whereas 60 per cent of the freshmen who are *not* from the same state as their zone representative fail to receive any committee request, only 18 per cent of those from the zone representative's state delegation are in the same unenviable position. The differential is somewhat smaller for nonfreshmen (75 per cent versus 50 per cent receive no request), but the pattern is the same. For members from outside the South, however, this differential does not appear. There is virtually no

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Table 12. Region, Margin of Election, and Success in House Committee Assignments
(Freshmen)

Region ^a	A. Marginal Districts							
	First Choice		Member Received		No Choice		Total	
	N	%	N	%	N	%	N	%
South	1	25	0	0	3	75	4	100
Non-South	30	59	10	20	11	22	51	101 ^b
Total	31	56	10	18	14	26	55	100

Region ^a	B. Safe Districts							
	First Choice		Member Received		No Choice		Total	
	N	%	N	%	N	%	N	%
South	7	32	7	32	8	36	22	100
Non-South	11	38	11	38	7	24	29	100
Total	18	35	18	35	15	29	51	99 ^b

^a The South includes the eleven states of the Confederacy.

^b Error is due to rounding.

Table 13. Region, Margin of Election, and Assignment Success in House Committee Assignments
(Nonfreshmen)

Region ^a	A. Marginal Districts							
	First Choice		Member Received		No Choice		Total	
	N	%	N	%	N	%	N	%
South	0	—	0	—	0	—	0	—
Non-South	7	39	5	28	6	33	18	100
Total	7	39	5	28	6	33	18	100

Region ^a	B. Safe Districts							
	First Choice		Member Received		No Choice		Total	
	N	%	N	%	N	%	N	%
South	7	27	2	8	17	65	26	100
Non-South	21	47	3	7	21	47	45	101 ^b
Total	28	39	5	7	38	54	71	100

^a The South includes the eleven states of the Confederacy.

^b Error is due to rounding.

Table 14. Party Support and Success in House Committee Assignments
(Nonfreshmen)

<i>A. 86th Congress</i>				<i>B. 87th Congress</i>			
	Assigned	Not Assigned	Total		Assigned	Not Assigned	Total
Supporter ^a	62 (8)	38 (5)	100 (13)	Supporter	50 (8)	50 (8)	100 (16)
Nonsupporter	40 (2)	60 (3)	100 (5)	Nonsupporter	45 (5)	55 (6)	100 (11)
Total	56 (10)	44 (8)	100 (18)	Total	48 (13)	52 (14)	100 (27)

<i>C. 88th Congress</i>				<i>D. 90th Congress</i>			
	Assigned	Not Assigned	Total		Assigned	Not Assigned	Total
Supporter	62 (8)	38 (5)	100 (13)	Supporter	60 (9)	40 (6)	100 (15)
Nonsupporter	40 (4)	60 (6)	100 (10)	Nonsupporter	0 (0)	100 (4)	100 (4)
Total	52 (12)	48 (11)	100 (23)	Total	47 (9)	53 (10)	100 (19)

<i>E. All Congresses</i>			
	Assigned	Not Assigned	Total
Supporter	58 (33)	42 (24)	100 (57)
Nonsupporter	37 (11)	63 (19)	100 (30)
Total	51 (44)	49 (43)	100 (87)

^a Supporters are members whose party support scores were above or equal to the mean for the party for the previous Congress. Cell entries are percentages; *Ns* are in parentheses.

party support goal is presented in Table 14. To construct this table we examined the party support scores of nonfreshmen requesters³⁵ for each of the Congresses in the sample, and compared the assignment success of those whose support surpasses the mean for the party in the previous Congress with those who gave the party less than the mean support. In each of the four Congresses, high party supporters were more successful in securing assignments (i.e.,

being granted any one of their requests) than low party supporters. Part E of this table gives the aggregate totals: 58 per cent of the high party supporters, as opposed to 37 per cent of the low party supporters, secured a requested committee.

Summary and Conclusions

In this paper we have viewed the Democratic assignment process as an instance of social choice. We have examined the process from the point of view of the members requesting assignments and of the members making assignments. We have assumed that both groups are goal-directed and that an understanding of their behavior derives from an analysis of alternative goals and alternative means (behaviors) of achieving them.

³⁵ We exclude the two nonfreshmen elected in special elections near the close of the preceding Congress for whom there was no party support score data. For each Congress, a member's party support score is tabulated from his voting behavior in the previous Congress. Party support data is found in the appropriate volumes of the *Congressional Quarterly Almanac*: vol. XIV (1958), pp. 124-125; vol. XVI (1960), pp. 140-141; vol. XV/III (1962), pp. 764-765; vol. XXII (1966), pp. 1030-1031.

In regard to those members requesting assignments, we have shown that their choice of committees is related to the type of district they represent. Also, the data indicate that the decision concerning the number of requests to make appears to be affected by certain strategic considerations such as whether the requester is a freshman, whether there is a vacancy from his state on his most preferred committee, and whether another member from his state is competing with him for his most preferred committee.

We next considered the making of assignments. Here we found that about three-fourths of the freshmen are granted some committee they requested. The data showed, however, that nonfreshmen are much less successful. Indeed, the probability of success decreased as seniority increased; therefore other factors were sought in order to explain assignment success. We considered several alternative goals for committee makers—the management goal, the constituency interest goal, the party maintenance goal, and the party support goal—and found strong support in the data for most of them. However, the interaction (and collinearity) among goals precludes any unqualified conclusions.

The assignment process in the House of Representatives is obviously complex, and it is affected by a host of factors, some of them detailed in the body of this paper. Our purposes here have been severalfold. First, we believe it is useful to view the assignment process as an institutionalized allocation process involving goal-seeking actors, scarce but valued commodities, and behavioral constraints. Second, given this view, we have sought to supply a “missing link” in the literature on committee assignments, namely the preferences of committee requests. Not only does this link provide some interesting insights about one group of actors in

the process (requesters); additionally, it provides empirical knowledge about the constraints and information confronting the other significant group in the process—the committee makers. Third, the heuristic use of a social choice construct and the new data on the preferences of committee requesters have brought into focus and provided some order to the complex of strategic factors involved in this process.

The questions for future empirical analysis appear to be almost limitless. For example, future research should consider, instead of surrogate variables like region, the position of members on specific issues before and after the assignment decision. Another aspect to be examined is the behavior of individual members of the Committee on Committees. What requesters from which zones are most successful? Do zone representatives appear to nominate members whose views are like their own? A third aspect of the process which deserves further consideration is the opportunity structure: Are members who are granted their first choice more likely to make further requests in the future, and if so are they very successful? Are members who receive no request initially granted a good assignment at some later time, or are they perpetually unsuccessful?

The research reported here is not distinctive in one respect: It raises as many questions as it answers. Nevertheless, we feel this study has provided some initial direction for a more comprehensive, formal understanding of the committee assignment process, and thus of internal relationships in the House as a whole. In this fashion, we believe, students of the public sector will begin to ascertain some of the operating characteristics of its institutions and thus be in a better position to make sound evaluations and prescriptions.

Candidates' Perception of Voter Competence: A Comparison of Winning and Losing Candidates*

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The purpose of the research reported in this article is to test the "congratulation-rationalization effect," a highly provocative hypothesis that John Kingdon has formulated in regard to politicians' beliefs about voters.¹ Comparing the beliefs that winning and losing candidates held about voters, Kingdon discovered that the winners manifested significantly more favorable beliefs than the losers.² Drawing upon a theory of cognitive dissonance of the Festinger variety, he offered an interesting interpretation of the findings.³ Both winners and losers, argued Kingdon, are likely to confront a dissonance-producing situation after an election and therefore are likely to alter their beliefs about voters. This reasoning led him to hypothesize that winners tend to develop complimentary beliefs about voters while losers tend to rationalize their defeats by downgrading voters' competence. The essence of the "congratulation-rationalization" hypothesis in Kingdon's original formulation is this: As a result of the election outcomes, winners tend to change their beliefs about voters in a favorable direction, whereas losers tend to alter such beliefs in an unfavorable direction.

The hypothesis is important, for it has theoretical implications for the role of elections in

the democratic process. This hypothesis implies, in the first place, that elections produce a favorable change in the beliefs that an elected official holds about voters' competence. Because of his victory, the politician believes even more than he did previously that the voters are well informed, genuinely interested, and capable of making intelligent choices. Consequently, the elected official may pay closer attention to the voters' opinions and be more responsive to them than if he were not elected. The manifest function of elections in this case is the inducement of responsible behavior on the part of the representative through a rearrangement of the candidates' cognitions.⁴

The hypothesis also has implications for constituency influence on the representative. The "congratulation-rationalization effect" may magnify such influence. The politician often acts according to his conception of the voters' reactions to alternative courses of action he might choose.⁵ The extent to which the representative anticipates the reactions of his constituency is likely to be related to his beliefs about the voters' competence. Hence, the tendency for a winning politician to develop beliefs complimentary to the voters implies that his anticipation of the voters' reactions will weigh heavily on his action. Constituency influence on a representative may therefore increase as a result of the "congratulation-rationalization effect."⁶

Although Kingdon demonstrated a significant difference between winners and losers in

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¹ John W. Kingdon, "Politicians' Beliefs About Voters," *American Political Science Review*, 61 (March, 1967), 137-45. See also his book, *Candidates For Office: Beliefs and Strategies* (New York: Random House, 1968).

² Kingdon, "Politicians' Beliefs," 139-42.

³ See, for example, Leon Festinger, *A Theory of Cognitive Dissonance* (New York: Row, Peterson, 1957); Leon Festinger and J. M. Carlsmith, "Cognitive Consequences of Forced Compliance," *Journal of Abnormal and Social Psychology*, 58 (March, 1959), 203-10; Robert B. Zajonc, "The Concepts of Balance, Congruity, and Dissonance," *Public Opinion Quarterly*, 24 (Summer, 1960), 380-96; Jack W. Brehm and Arthur R. Cohen, *Explorations in Cognitive Dissonance* (New York: John Wiley, 1962); and Florence L. Denmark and Brunhilde Ritter, "Differential Cognitive Dissonance and Decision Latency," *The Journal of Social Psychology*, 86 (February, 1972), 69-74.

⁴ Political theorists have attributed various functions to elections. Elections have been considered as devices for selecting leaders and legitimizing the regime, means of giving the citizens direct and indirect control over government policies, channels for the expression of public choices, and links between officials and the voting public through which influence is exchanged. However, cognitive effects of elections such as the one stated in this hypothesis have not been explicitly formulated as part of the functions of elections. See, for example, Richard Rose and Harve Mossawir, "Voting and Elections: A Functional Analysis," *Political Studies*, 15 (June, 1967), 173-201; and Gerald Pomper, *Elections in America* (New York: Dodd, Mead, 1968), pp. 16-40.

⁵ Carl J. Friedrich, *Constitutional Government and Democracy* (Boston: Ginn and Co., 1950), p. 49.

⁶ Kingdon, "Politicians' Beliefs," pp. 144-45.

their beliefs about voters, he was unable to test the hypothesis. Since his study was based on interview data collected after an election, it was not possible for him to compare candidates' beliefs about voters before and after the election, a comparison essential to test such a hypothesis.⁷ Therefore, whether winners really change their beliefs in a favorable direction and whether losers alter their beliefs in an unfavorable direction still remains an open question.

The Iowa Election Study provides an excellent opportunity to test the "congratulation-rationalization" hypothesis. Candidates for office were asked to indicate their beliefs about voters both before and after an election. All contested candidates who ran in the 1970 Iowa general election were included in the study. The data were collected by means of mail questionnaires.⁸ The pre-election survey, conducted in October and November 1970, included 262 candidates.⁹ Of these, 202 candidates returned usable replies, which represents a 77 per cent response rate. Given our before-and-after design, we sent out questionnaires after the general election only to those who had responded to the pre-election survey. One hundred seventy-three of the winning and losing candidates responded to the follow-up questionnaire, an impressive 86 per cent of all those who responded to the first questionnaire. In addition to various standard information on the candidates' social and career background characteristics, we also solicited information regarding their beliefs

about voters, their pre-election anticipations of the election outcomes, their political ambition, their support for the institution of elections, and the 23 paired-item *Rotter Internal Versus External Control Scale*.

Measurements of Key Variables

Candidates' beliefs about voters consist of their theories, implicit or explicit, about the manner in which voters make their electoral choices.¹⁰ An important part of such beliefs relates to candidates' perceptions of voters' competence, that is, the extent to which they believe the voters are deeply interested in campaigns, the extent to which they believe the voters are informed of key issues, and the extent to which they believe the voters make intelligent choices on the basis of the issues rather than on the basis of party labels or candidate characteristics. When elected, the candidates who believe that the voters are politically interested, informed, and capable of making intelligent choices are probably more likely to be constrained by the voters' opinions than are other candidates who have less favorable perceptions of voters' competence.¹¹

Another part of candidates' beliefs is their conception of what factors influence voters' choices. The voting studies have identified three major factors of this sort: party identification, issues, and candidate orientation.¹² The candidates were asked both to rate and to rank the relative importance of each of these three factors, before and after the election. If the "congratulation-rationalization effect" is indeed operative, one would expect that winners change in their rankings of the three factors in the direction complimentary to voters, whereas losers change in the direction deprecating to voters. More specifically, one might expect that the winners who ranked party or issues as first in importance before an election, would subsequently rank the personal characteristics of candidates as first.¹³ Conversely, the losers who

⁷ The "congratulation-rationalization" hypothesis involves changes in candidates' beliefs and the direction of such changes. To test the hypothesis, one needs data on attitude changes. Interview data collected at a single point in time, the kind of data used in Kingdon's study, do not provide an adequate basis to test the hypothesis.

⁸ Certain typical problems are associated with the use of mail surveys. Briefly, these problems relate to the type of data which can be gathered, the type of respondents who can be reached, and the response rate. In the present study, these problems were recognized and dealt with as fully as possible. First, the questionnaire used was brief, and the questions asked were simple and straightforward. Moreover, many of our questions had been used in several previous studies and their reliability had been tested. Second, our respondents were an ideal sample for a mail survey. They were candidates for public office, a relatively well-educated and articulate group. We found almost no "nonresponses" in the returned questionnaires. Finally, the response rate for the pre- and postelection surveys were impressive. For a good discussion of the problems associated with the use of mail surveys, see William J. Goode and Paul K. Hatt, *Methods in Social Research* (New York: McGraw-Hill, 1952), pp. 170-83.

⁹ This figure represents all contested candidates who ran in the 1970 general election in Iowa. There were also 32 candidates who were unopposed in the election. We did not include these unopposed candidates in our survey.

¹⁰ Kingdon, "Politicians' Beliefs," 137-38.

¹¹ Lewis A. Dexter, *The Sociology and Politics of Congress* (Chicago: Rand McNally, 1969), pp. 151-75; Warren E. Miller and Donald E. Stokes, "Constituency Influence in Congress," *American Political Science Review*, 57 (March, 1963), 45-56.

¹² See Angus Campbell, Philip E. Converse, Warren E. Miller, and Donald Stokes, *The American Voter* (New York: John Wiley, 1960); Angus Campbell, Gerald Gurin, and Warren E. Miller, *The Voter Decides* (Evanston: Row, Peterson, 1954); and David E. Re-pass, "Issue Salience and Party Choice," *American Political Science Review*, 65 (June, 1971), 389-400.

¹³ We are making a number of assumptions here. Stated explicitly, (1) we assume that when a candidate rates candidate characteristics as the most important factor, he is congratulating himself and the voters, and

Table 1. Change in the Candidates' Perception of Voters' Competence(Frequency)

After Election		Scale Scores							
Before Election	0	1	2	3	4	5	6	Total	
0	2	0	5	0	3	0	0	10	
1	2	0	3	0	0	0	1	6	
2	3	1	6	0	5	1	3	19	
3	1	0	0	1	3	1	2	8	
4	1	2	3	2	10	3	8	29	
5	0	0	0	5	6	10	13	34	
6	2	0	5	2	10	10	24	53	
Total	11	3	22	10	37	25	51	159	

Scale Scores

Increase in perception

Decrease in perception

initially ranked issues or candidate characteristics as first, would rank party labels as the most important factor after an election.

The operational measurement of candidates' perceptions of voters' competence is a summated scale, constructed on the basis of the candidates' responses to three questions.¹⁴ These questions have to do with the candidates' beliefs concerning voters' political interests,

their political knowledge, and their capacity to make intelligent electoral choices.¹⁵ Each candidate who responded was assigned a score based on the arithmetic sum of his coded responses. The scale scores range between 0 (least favorable) and 6 (most favorable).¹⁶ Table 1 displays data concerning the candidates' perceptions of voters' competence before and after the election. Of 159 candidates who re-

(2) we assume that when a candidate rates party labels as first in importance, he is rationalizing his defeat by blaming voters for blindly voting for party. These are the assumptions which Kingdon has made in his study. We recognize at this point that another plausible assumption which contradicts our first assumption can be made: when a candidate rates candidate characteristics as the most important factor, he might be downgrading the voters. To believe that the voters make their choices on the basis of candidates' personal characteristics could mean that a candidate is disparaging their political sophistication. However, our objective is to test the validity of Kingdon's hypothesis and therefore we adopted his assumptions. See his "Politicians' Beliefs About Voters," pp. 139-40.

¹⁴ When responses to several questions are aggregated to form a single measure as was the case in our summated scale, we might be in effect losing some information. Therefore we have also scrutinized responses to

individual questions. The results of the item-by-item analysis did not change our basic arguments. See Donald P. Racheter, "Representation and the Congratulation-Rationalization Effect" (M.A. thesis The University of Iowa, 1972).

¹⁵ The questions used were: (1) Would you indicate how important you think the candidates' issue positions are to the voters in determining their choices? (2) Generally speaking, how interested in the campaign do you think the voters in your district are? (3) How well informed do you think the voters in your district are about the candidates' issue positions? The same set of questions was repeated in the postelection survey with only the tense changed.

¹⁶ Responses to the three voter competence items were coded: (0) Not important (or informed), (1) Somewhat important (or informed), and (2) Very important (or informed). These coded responses were summed to form a simple index.

Table 2. Change in Candidates' Perception of Voters' Competence by Election Outcomes
(Percentages)

Election Outcomes	Change in Perception													Total (N)
	+6	+5	+4	+3	+2	+1	0	-1	-2	-3	-4	-5	-6	
Winner	0.0	0.0	2.7	1.3	13.5	12.2	36.5	17.6	10.8	1.3	4.1	0.0	0.0	100% (74)
Loser	0.0	1.2	4.7	2.3	10.6	15.3	30.6	9.4	15.3	4.7	3.6	0.0	2.3	100% (85)

turned usable questionnaires before and after the election, 51 (or 32 per cent) reported an increase in their favorable perceptions of voters, while 55 (or 35 per cent) reported a decrease. Almost one-third of the candidates indicated "no change" in their perceptions. From these results, a measure of the direction and the magnitude of changes in candidates' perceptions was constructed. This measure, which ranges between scale scores of +6 and -6, is designed to indicate the degree of increase or decrease in favorability of a candidate's perception of voters' competence.¹⁷

Findings

1. Changes in Candidates' Beliefs About Voters. The hypothesis that winners tend to develop complimentary beliefs about voters while losers tend to develop deprecating beliefs may be tested in two ways: (1) a comparison of winners and losers in terms of the direction and the magnitude of changes in their perceptions regarding the competence of voters, and (2) an analysis of changes that take place in candidates' rankings of the relative importance of three factors which influence voters' choices: party labels, issues, and candidate characteristics.

The data presented in Table 2 show little difference between winners and losers in terms of the direction and the magnitude of changes in their perceptions regarding the competence of voters. Among the winners, roughly 37 per cent did not change in their perceptions. Among the losers 31 per cent did not change, indicating a slight, but statistically insignificant, tendency for winners to have more stable perceptions than losers ($t = 0.53$, $df = 157$, *n.s.*). While some 30 per cent of the winners showed an increase in their complimentary beliefs, 34 per cent of the losers also did. Similar proportions of the winners and losers indicated a decrease

in such beliefs: 33.8 per cent and 35.3 per cent respectively. Thus, the evidence makes it quite clear that winners do not necessarily develop complimentary beliefs more than do losers. Conversely, losers do not necessarily develop more deprecating beliefs than winners.¹⁸

A supplementary analysis was performed to test the hypothesis. Instead of relying upon the candidates' perceptions of voters' competence, we focused on changes that took place in the candidates' rankings of the relative importance of the three factors influencing voters' choices. By comparing their pre- and postelection rankings, we were able to determine shifts that occurred in their relative rankings of party labels, issues, and candidate characteristics.

According to the "congratulation effect," one might predict that the winners who initially ranked party or issues first in importance are likely to rate their own characteristics as first after an election. Conversely, the losers who ranked issues or candidates as first in importance are likely to rank party as the most im-

¹⁸ Additional evidence is available. When we compared the winners and losers in terms of their pre- and postelection perceptions, we discovered as shown in Table A that the winners manifested much more favorable perceptions than the losers after the election, and that the subsequent winners also manifested significantly more favorable perceptions than the subsequent losers before the election. This means that the winners had more favorable perceptions than the losers from the outset.

Table A. Candidates' Perception of Voters' Competence Before and After General Election
(Percentages)

Scale Scores	Before Election		After Election	
	Winner	Loser	Winner	Loser
Least Favorable	0	3.4%	7.2%	2.7%
	1	0.0	6.2	0.0
	2	9.1	13.4	9.5
	3	3.4	7.2	6.8
Most Favorable	4	19.3	20.6	21.6
	5	28.4	15.5	29.7
	6	36.4	29.9	29.7
	6	36.4	29.9	29.7
Total (N)	100% (88)	100% (97)	100% (74)	100% (85)
$t = 2.04$		$p < .025$	$t = 2.72$	$p < .005$

¹⁷ The measure for the direction and the magnitude of changes in candidates' perceptions regarding the competence of voters was constructed from the two summated scales of voters' competence: the pre- and post-election scales. To obtain scores for changes in candidates' perceptions, the postelection scores were subtracted from the pre-election scores.

Table 3. Change in the Relative Ranking of Three Factors
(Percentages)

Change in Ranking	Most Important Factor		Least Important Factor	
	Winner	Loser	Winner	Loser
From Issues or Candidate to Party Label	25%	78%	28%	66%
From Party Label or Issues to Candidate	75	22	72	34
Total (N)	100% (20)	100% (27)	100% (40)	100% (29)
	$\chi^2 = 12.95,$	$P < .001$	$\chi^2 = 12.10,$	$P < .001$

Note: The total in the table does not add up to 173 candidates, because many did not fit the four types of change reported above. Of all candidates, 39 per cent did not change in their rankings; 19 per cent changed from party to issues; and 10 per cent changed from candidates to issues.

portant factor after an election, due to the "rationalization effect." With respect to the shifts that occur in the least important factors, one might expect that the winners who initially ranked issues or candidates as last in importance are likely to rank party labels as the least important after an election. In a similar vein, the losers who ranked party or issues as last in importance at the outset, are likely to rank candidate characteristics as the least important after the election.

There are six logically possible types of change in the candidates' rankings of the relative importance of party, issues, and candidates.¹⁹ Table 3 reports only that portion of the data most relevant to testing the hypothesis.²⁰ Focusing upon factors ranked as the most important, one finds that the winners more than the losers tended to alter their ranking from party or issues to their own characteristics, suggesting that the winners tend to congratulate voters for "making the right choices." By comparison, the losers more than the winners tended to change their ranking from issues or candidates to party label, which indicates that the losers tended to downgrade voters for "blindly voting for party labels." Looking at the shifts which occurred in the least important factors, however, one is struck by a finding contradictory to the prediction. The winners

who ranked party or issues as least important at the outset tended to rank candidate characteristics as least important after the election. Among the losers, those who ranked issues or candidate characteristics as least important tended to rank party labels "least important" after the election. Thus, the evidence about ranking shifts is also inconclusive, and does not offer confirmation of the hypothesis that winners develop complimentary beliefs about voters while losing candidates develop deprecating beliefs.

2. Effect of Candidates' Anticipation of Election Outcome. It is quite possible that candidates anticipate the election outcome long before election day. Because of such anticipations, the "congratulation-rationalization effect" might take place even before the actual outcome is known. The candidates were asked to rate their chances of winning in the first questionnaire.²¹ Fifty-seven per cent rated their chances favorably, while some 30 per cent gave pessimistic assessments. If the "congratulation-rationalization effect" does take place before the election as a result of candidates' anticipations of the outcome, one might expect that among the subsequent winners, those who anticipated an election victory at the outset as compared to those who anticipated a defeat, would manifest more favorable beliefs about voters. Again, among the subsequent losers, those who anticipated a victory as compared to those who anticipated a defeat at the outset, should have more favorable beliefs.

The data presented in Table 4 show no evidence to confirm such an effect of candidates' anticipations upon their beliefs about voters.

¹⁹ The six types of change in the candidates' rankings include: (1) from party to issues, (2) from party to candidates, (3) from issues to party, (4) from issues to candidates, (5) from candidates to party, and (6) from candidates to issues.

²⁰ Of the six types of change, only four are directly relevant to testing predictions derived from the hypothesis. Since we assume that changes from issues or candidates to party reflect the rationalization effect and that changes from party or issues to candidates reflect the congratulation effect, we report data on these four types of change. Nevertheless, we have also examined all six types of change separately, and have found no evidence to contradict the conclusion suggested in the text.

²¹ The question was: "How do you rate your chances in the upcoming election?" Responses were coded: (1) Excellent chance to win, (2) Good chance to win, (3) Some chance to win, (4) It's a toss-up, (5) Probably will lose, and (6) Almost sure to lose.

Table 4. Effect of Anticipation on Candidates' Pre-Election Beliefs
(Percentages)

Pre-election Perceptions of Voters' Competence		Winner		Loser	
		Anticipated Victory	Anticipated Defeat	Anticipated Victory	Anticipated Defeat
Most Favorable	6	35.5%	42.9%	32.1%	31.2%
	5	32.9	14.3	20.8	10.4
	4	15.8	28.6	20.8	16.7
	3	2.6	7.1	13.2	2.1
	2	9.3	7.1	9.4	16.7
Least Favorable	1	0.0	0.0	0.0	12.5
	0	3.9	0.0	3.7	10.4
Total (N)		100% (76)	100% (14)	100% (53)	100% (48)
		$X^2 = 3.96$, n.s.		$X^2 = 11.12$, n.s.	

Among the subsequent winners, no significant difference was discovered between those who anticipated a victory and those who anticipated a defeat in terms of their perceptions of voters' competence ($X^2 = 3.96$, n.s.). Similarly, among the subsequent losers, there was no significant difference between those who anticipated a victory and those who anticipated a defeat ($X^2 = 11.12$, n.s.). A supplementary analysis in which we compared candidates' anticipations and the direction of changes in their beliefs also revealed a negative result: the anticipations of the election outcome did not effect changes in candidates' beliefs about voters.²² Thus, one can conclude that neither the actual election outcome nor the anticipations of the outcome have significant effect upon candidates' beliefs about voters.

3. Dissonance States and Changes in Candidates' Perceptions of Voters' Competence.

Thus far, our analysis has concentrated on the

²² The results of this analysis are reported in Table B. It is clear that no significant difference obtains between those who expected to win and those who expected to lose within each group of the winners and losers. This indicates that differences in pre-election anticipations of the election outcomes have no visible impact upon the direction and magnitude of changes in candidates' perceptions regarding the competence of voters.

difference between winners and losers in their beliefs about voters, and changes in such beliefs. The overall evidence lends no clear support for the "congratulation-rationalization" hypothesis. Nevertheless, it seems yet possible to confirm the hypothesis if it is cast in somewhat different terms, terms more consonant with the theory of cognitive dissonance as originally outlined by Festinger. Although Kingdon acknowledged his debt to Festinger's theory, his formulation actually departed from the theory by placing a heavy emphasis upon the independent impact of election outcomes.²³ When Kingdon stated that winners tend to develop complimentary beliefs about voters while losers tend to develop rationalizations for their defeats by downgrading voters' competence, he was evidently assuming that dissonance is a direct consequence of the election outcome. Although a state of cognitive dissonance may result from the election outcome, it depends also upon the candidates' pre-election beliefs about

²³ Kingdon stated that: "winners develop complimentary beliefs about voters and losers develop rationalizations for the losses *simply by virtue of the outcome of the election*" [our emphasis]. See his "Politicians' Beliefs About Voters," p. 142.

Table B. Anticipation of the Outcomes and Change in Perceptions
(Percentages)

Change in Perception	Winner		Loser	
	Anticipated Victory	Anticipated Defeat	Anticipated Victory	Anticipated Defeat
Favorable Change (Increase)	26.6%	33.3%	34.3%	35.0%
No Change	36.7	33.4	15.8	40.0
Unfavorable Change (Decrease)	36.7	33.3	49.9	25.0
Total (N)	100% (60)	100% (12)	100% (38)	100% (40)

voters. What precipitates the "congratulation-rationalization effect" may be a state of dissonance induced by the incongruence between a candidate's beliefs and the election outcome, rather than the winning or losing in itself.

Restated in direct dissonance terms, the hypothesis asserts that within each group of winners and losers it is only those experiencing a high degree of dissonance who will change in their beliefs about voters. The dissonance, rather than the election outcome, is the critical variable in this formulation. High and low dissonance states were established on the basis of (1) the pre-election scale scores for candidates' perceptions of voters' competence, and (2) the election outcomes.²⁴ A candidate who entered the campaign with favorable beliefs about voters and subsequently emerged as a winner may experience less dissonance than other successful candidates who had unfavorable beliefs from the outset. Similarly, a candidate who initially had favorable beliefs but was later defeated may feel greater dissonance than other losers who had unfavorable beliefs from the outset.

In Table 5, the direction and the magnitude of changes in candidates' perceptions of voters' competence are compared by different dissonance states within each group of winners and losers. Looking at the winners first, one finds that as compared to the "low dissonance" group, the "high dissonance" group—the candidates who had unfavorable beliefs about voters at the outset—tended to change their beliefs in a more favorable direction. The rank-order correlation between dissonance states and changes in candidates' perceptions was quite high ($\tau c = .48$). This suggests that winners whose initial beliefs about voters were incongruent with the election outcomes tended to upgrade their beliefs. Thus, there is evidence to conclude that the "congratulation effect," a part of Kingdon's hypothesis, is operative, insofar as winners' initial beliefs about voters and the subsequent election outcomes are not congruent.

With respect to the "rationalization effect," another part of the hypothesis, a strong correlation

was found between dissonance states and negative changes in candidates' perceptions ($\tau c = -.41$). Among the losers, the "high dissonance" group consists of the candidates who entered the election campaign with highly favorable beliefs about voters. These candidates believed at the outset that the voters were well informed on key issues, were deeply interested in the campaign, and were capable enough to make their choices on the basis of issues rather than on the basis of a candidate's party label or his personal characteristics. Despite the highly complimentary beliefs that these candidates held, the voters did not reciprocate with electoral support: they were subsequently defeated. Thus, these candidates were likely to perceive a dissonance between the election outcome and their beliefs about voters. The data indicate that the "high dissonance" group of the losers more than the "low dissonance" group tended to change their beliefs in the direction deprecating to voters. Evidently, the "rationalization effect" is also operative among the losers, insofar as the losers' initial beliefs about voters and the subsequent election outcome are not congruent.²⁵

What emerges in this analysis is this: The "congratulation-rationalization" hypothesis can be confirmed, but only if cast in terms of "dissonance states" rather than "election outcomes." In his original formulation, probably because he lacked pre-election data, Kingdon emphasized the independent effect of the election outcomes—winning and losing. Consequently, he hypothesized that winners tend to develop complimentary beliefs about voters while losers tend to relinquish such beliefs. Clearly, this formulation obfuscates the critical importance of dissonance states by implicitly assuming that changes in candidates' beliefs are direct effects of the election outcomes. Our analysis has shown, however, that the election outcomes do not in themselves effect changes in candidates' beliefs. What causes change in such beliefs is dissonance induced by the *discrep-*

²⁴ The "high" and "low" dissonance groups among the winners and losers were established in the following manner:

Election Outcomes	Scale Scores for the Pre-Election Perceptions						
	0	1	2	3	4	5	6
Won	High Dissonance Group (N=26)			Low Dissonance Group (N=48)			
Lost	Low Dissonance Group (N=56)			High Dissonance Group (N=29)			

²⁵ The data reported in Table 5 also show considerable cognitive changes for the low dissonance groups. For instance, 54 per cent of the low dissonance winners changed their perceptions in both favorable and unfavorable directions. Among the low dissonance losers, almost 72 per cent either increased or decreased in their evaluations of voters. Thus, it seems evident that factors other than dissonance states also act upon changes in the candidates' perceptions. These unidentified factors notwithstanding, the overall evidence clearly indicates that cognitive dissonance explains a large part of the variation in changes of candidates' perceptions. The problems of validating dissonance theory are succinctly discussed in Natalia P. Chapanis and Alphonse Chapanis, "Cognitive Dissonance: Five Years Later," *Psychological Bulletin*, 61 (January, 1964), 1-22.

Table 5. Relation Between Candidates' Dissonance States and Perceptions of Voters' Competence
(Percentages)

Change in Perception*	Winner		Loser	
	High Dissonance	Low Dissonance	High Dissonance	Low Dissonance
Large increase	—	—	—	3.5
Medium increase	11.6	—	—	20.7
Small increase	53.8	10.4	25.1	27.6
No change	19.2	45.8	32.1	27.6
Small decrease	11.6	37.5	28.5	17.1
Medium decrease	3.8	6.3	10.8	3.5
Large decrease	—	—	3.5	—
Total (N)	100% (26)	100% (48)	100% (56)	100% (29)
	Kendall's $\tau c = .48$		$\tau c = -.41$	

* These seven categories were established from the scale scores for the direction and the magnitude of changes in perceptions of voters' competence: Large increase (+6, +5), Medium increase (+4, +3), Small increase (+2, +1), No change (0), Small decrease (-1, -2), Medium decrease (-3, -4), and Large decrease (-5, -6)

ancy between a candidate's beliefs and the election outcome. Therefore, whether or not winners will develop complimentary beliefs and whether or not losers will develop deprecating beliefs depends upon the states of dissonance that such candidates confront after an election, rather than on the election *per se*.

Conclusions

The analysis has disclosed no clear evidence to support the hypothesis that winners tend to develop complimentary beliefs about voters while losers develop deprecating beliefs. Although nearly two-thirds of the candidates were found to have changed in their perceptions of the competence of voters, no significant difference in direction and magnitude of such changes was discovered between the winners and losers. The evidence was again inconclusive when we focused on the shifts that took place in the candidates' rankings of the relative importance of three factors influencing voters' choices: party labels, issues, and candidate characteristics. The winners did not change their rankings of the three factors in a direction visibly more complimentary to voters than the losers, nor did the losers change their rankings in a direction more deprecating to voters than the winners. Therefore, it seems evident that winners do not necessarily develop more complimentary beliefs than losers. Conversely, losers do not necessarily develop more deprecating beliefs.

The "congratulation-rationalization" hypothesis, however, was strongly supported in the analysis when it was refined. In this new formulation, the state of dissonance rather than the election outcome was made the major in-

dependent variable. The results of analysis show that among the winners, the "high dissonance" group more than the "low dissonance" group tended to upgrade their beliefs concerning the competence of voters. By contrast, among the losers, the "high dissonance" group more than the "low dissonance" group tended to downgrade their beliefs about voters. Thus, the evidence suggests that the "congratulation-rationalization effect" is operative to the extent that candidates' beliefs about voters and the election outcomes are incongruent.

Finally, one may ask whether the congratulation-rationalization hypothesis formulated by Kingdon offers a sound explanation of the divergent beliefs held by winning and losing candidates. The findings reported in this article suggest that the hypothesis does not provide an efficient explanation for the phenomenon that winners manifest markedly more favorable beliefs than losers do. It seems clear that winners more than losers have favorable beliefs, but not because winners change their beliefs in a favorable direction as a result of their election victories nor because losers alter their beliefs in an unfavorable direction. Alternative explanations must be sought for the divergent beliefs that winning and losing candidates manifest.²⁸ Possibly such explanations might be found in such factors as the incumbent status of candidates, the degree of competitiveness in their districts, their career socialization, and the level of their political ambition.

²⁸ Kingdon suggested alternative explanations based on the incumbent status of candidates and the marginality of the district. We plan to test some of these explanations in future research. See Kingdon, "Politicians' Beliefs About Voters," pp. 142-43.

Residential Location and Electoral Cohesion: The Pattern of Urban Political Conflict*

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Urban scholars, whatever their discipline, have shared a concern for identifying the structural and environmental factors which shape local politics. The literature has become too extensive for a thorough discussion of the many variables which influence local politics, but one possibly important aspect that has largely been ignored by political scientists is the ecological structure of cities.¹

In an effort, therefore, to remedy an apparent oversight in urban analysis, this paper will focus upon the theoretical and empirical relationships between the residential location of social-class groups in selected American cities and the electoral cohesion displayed by such population groups. Attention will also be directed at studying the consequences of this intragroup cohesion upon intergroup conflict as it is manifested in electoral competition. This analysis of residential location and electoral cohesion may provide an opportunity to examine existing propositions concerning the effects of location upon urban political conflict.

Residential location reflects a variety of class-based considerations. Studies have indicated that occupational and educational groups tend to cluster spatially in their selection of residential areas.² Even with low rates of interaction in most neighborhoods, neighbors in homogeneous areas are not only of similar status, but also tend to share the same values and lifestyles.³ Residential location may also be a strat-

egy for minimizing the social and spatial distance between the individual and the population he desires to emulate. Conversely, location also serves to maximize the distance from groups with which the individual does not wish to interact.⁴ Additionally, the creation and erection of homogeneous and exclusive residential areas may also serve to increase the cohesiveness and solidarity of the inhabitants and lead to the development of distinctive group standards.

Several urban scholars have been concerned with the social implications of such group cohesion.⁵ Apparent disagreement about the effect of residential segregation upon group cohesion exists, however, which suggests that we need further empirical study of the relationship between intragroup cohesion and intergroup disagreement. The disagreement centers on whether residential segregation increases or decreases the frictions between social-class groups. The solidarity of segregated classes may also be influenced by the intensity of the electoral disputes between these same groups. The more intense the disputes between class groups, the more may be their intragroup cohesion.⁶

One method that may prove useful in the examination of intragroup cohesion and intergroup disagreement is the analysis of the voting choices of urban residents. The policy choices of various social-class groups reflected in referendum voting on such local concerns as education, civic improvements, taxation, and similar items, can provide an index of both the cohesiveness and distinctiveness of community groups. The analysis of social-class support and opposition to local ballot issues facilitates the identification of segments of the urban popula-

* This article follows from the author's doctoral dissertation, "Ecological and Structural Determinants of Urban Electoral Conflict" (University of California, Riverside, 1971). Acknowledgments are due Charles R. Adrian and Ronald O. Loveridge for comments on the dissertation. I owe special thanks to Harlan Hahn who made these data available for analysis and who read and evaluated both the dissertation and manuscript.

¹ For one discussion of some ecological factors and intercity cooperation, read Oliver P. Williams, Harold Herman, Charles S. Liebman, and Thomas R. Dye, *Suburban Differences and Metropolitan Policies, A Philadelphia Story* (Philadelphia: University of Pennsylvania Press, 1965).

² Otis Dudley Duncan and Beverly Duncan, "Residential Distribution and Occupational Stratification," *American Journal of Sociology*, 60 (March, 1965), 493-503; Suzanne I. Keller, *The Urban Neighborhood* (New York: Random House, 1968), pp. 90-92.

³ William H. Whyte, Jr., *The Organization Man* (Garden City: Doubleday and Company, Inc., 1957), pp. 310-344.

⁴ Oliver P. Williams, *Metropolitan Political Analysis, A Social Access Approach* (New York: The Free Press, 1971), p. 28.

⁵ Among the major works in this area are James Beshers, *Urban Social Structure* (New York: The Free Press, 1962); William H. Michelson, *Man and His Urban Environment* (Reading, Mass.: Addison-Wesley Publishing Company, 1970); John Sirjamaki, *The Sociology of Cities* (New York: Random House, 1964).

⁶ James S. Coleman, *Community Conflict* (New York: The Free Press of Glencoe, 1957); Lewis A. Coser, *The Functions of Social Conflict* (New York: The Free Press of Glencoe, 1956).

tion that are politically opposed to one another. By focusing upon local referenda voting, we can identify the nature and intensity of political conflicts that exist between electoral groups within many communities.

In summary, this paper is concerned with a pattern of urban conflict. This theme suggests that conflict is spatially patterned and thereby associated with residential location. The following hypotheses will be examined:

(1) Residential location will significantly influence the development of electoral cohesion *within* social-class groups. Social-class groups that are spatially segregated (located in socially homogeneous sectors) in a city will exhibit greater intragroup electoral cohesion in local referenda than spatially integrated social-class groups (located in socially heterogeneous sectors).

(2) Residential location will significantly influence the amount of electoral disagreement *between* different social-class groups. The residential segregation of class groups will promote the development of interclass disagreements, while spatial integration will hinder the development of these disagreements.

(3) The degree of interclass electoral disagreement will influence the degree of intraclass electoral cohesion. This relationship will be influenced, however, by the spatial distribution of class groups within each city. Cities with segregated classes will display high intraclass electoral cohesiveness, irrespective of the degree of interclass disagreement. On the other hand, cities with integrated classes will manifest low electoral cohesion independent of the degree of class disagreements.

Sources of Data and Methods of Measurement

Eighteen cities from a U.S. Public Health Service study of local voting behavior were selected for this study. The two criteria used for the selection of cities were size of city and number of referenda issues for which voting records and precinct maps were complete.⁷ Despite the small base, these eighteen cities represent nearly all regions of the U.S. and offer a good basis for comparison. The two sources of information for this paper are city block demographic characteristics from the 1960 United

States Census of Housing, and precinct-voting results from five types of referenda.⁸

The examination of the distribution and cohesion of population groups is not intended to be a study of the properties of the individuals within the city, instead, it is an examination of the aggregate properties of population groups residing within specified geographical sections of each community. Census statistics give valuable information about the environmental characteristics of an area, while precinct returns provide information about the political texture of an area. Aggregate data, therefore, appropriately provide the basis for the operationalization of the key concepts discussed in this paper. The next task is to elaborate on the methods used in defining these concepts operationally.

A social-class group is comprised of all of the precincts within a city that manifest similar social characteristics. A useful measure of social class presented in the Census of Housing is mean house value, and since housing values appear to be valid indicators of the social status of homeowners, they may be employed as surrogates for such typically used variables as education, occupation, and income. To determine the social-class rank of an individual precinct, the mean house value of each city block within each precinct is aggregated, and the mean house value of the precinct is computed.⁹

Social-class electoral cohesion is the amount of variance between precincts of the *same* social-class group in voting on referenda.¹⁰ A group with low electoral cohesion manifests higher intensity of conflict than a class that is electorally cohesive. If precincts within a social class exhibit a relatively high amount of variance in their voting behavior, there is an electoral division within that class. This division of

⁸ A good discussion on the use of these specific data is provided by Harlan Hahn, "Ecological Data and Structural Characteristics: Some Notes on the Homogeneity of Precincts and Census Tracts (paper presented at the annual meeting of the Western Political Science Association, Albuquerque, N.M., April 8-10, 1971).

⁹ Social-class groups, computed on the basis of mean house values, were the following: Rank 1: mean values less than \$7,320; Rank 2: mean values from \$7,321 to \$11,880; Rank 3: mean values from \$11,881 to \$18,120; Rank 4: mean values from \$18,121 to \$22,680; Rank 5: mean values above \$22,681.

¹⁰ Each city was ranked according to its mean social-class variance in voting for each referendum. The ranks were split at the median into two groups. Two-way analysis of variance statistics also indicated that splitting the ranks at the median resulted in the best homogeneous groupings possible. For fluoridation referenda the cutting-point was 73.6; for education, 70.6; for parks, 70.4; for civic improvements, 76.8; for public works, 82.2.

⁷ Peoria: 125, 736; St. Joseph: 71, 996; Medford: 29, 750; Rockford: 144, 707; Greensboro: 140, 672; Dayton: 239, 591; Fremont: 100, 739; Yakima: 45, 060; Flint: 193, 371; Lima: 53, 373; Berkeley: 113, 165; Allentown: 108, 926; Knoxville: 169, 766; Trenton: 102, 211; Lincoln: 148, 092; Eugene: 77, 284; Beaumont: 115, 716; Sioux Falls: 72, 557.

support for the referenda issues is a demonstration of intragroup electoral conflict.

Interclass electoral disagreement is the degree of variation in voting on referenda between different social-class groups within a city. High variation in voting indicates high disagreement between classes. High disagreement is indicative of electoral conflict within the community.¹¹

It is possible to specify the amount of spatial segregation or integration that a social-class group displays by examining the social dissimilarity scores of each of the precincts within a class group. This dissimilarity score is the amount of class difference between a precinct and all spatially contiguous precincts. Differences are translated into ordinal values varying between zero and one. Precincts with index scores near zero are in areas of very few class differences (socially homogeneous), whereas precincts with index scores approximating one are located in areas of great class distinctions (socially heterogeneous). The aggregation of the dissimilarity scores for all precincts within each class of precincts allows the comparison of the classes in a city on the basis of residential location, i.e., segregated or integrated.¹²

The empirical determination of the spatial contiguity of precincts is made by calculating the geographical distance between all possible pairs of precincts within each city. The calculation of the distance between all pairs of precincts is based upon the designation of the ex-

act geographic location of each precinct. In this manner, the distance between any specific precinct and all other precincts can be examined, and those precincts that are nearest to the precinct in question can be identified according to social-class characteristics.

In addition, each precinct is given a value which reflects the average geographical distance between that precinct and all other precincts of the same class. Another statistic that has been calculated for each precinct is a measure of electoral disparity—the mean difference (percentage of the vote in favor of the referendum) between the precinct and all others within the same social class. Social dissimilarity and electoral disparity are similar to the measures of residential location and electoral cohesion except that dissimilarity and disparity are calculated on a precinct level rather than a social-class level.

The referenda under study include voting on the addition of fluorides into local water supplies, school bond and tax override elections, parks and recreation bonds and tax levies, bonds for the construction and improvement of civic buildings, and bonds for the construction of public works projects, such as streets, sewers, and city water works. This analysis encompasses 58 municipal elections held in these 18 U.S. cities since 1955. Fluoridation referenda account for 12 of the elections, education for 14, parks and recreation for 8, civic improvements for 12, and public works for 12 elections. This rich variety of ballot issues allows cross-community comparison of the pattern of political conflict.

In brief, this paper will test three major hypotheses: (1) An increase in the spatial segregation of social-class groups will be associated with an increase in the electoral cohesion displayed within each group; (2) an increase in the spatial segregation of social-class groups will be associated with an increase in the electoral disagreements between class groups; (3) an increase in interclass electoral disagreements will be associated with an increase in the electoral cohesion within each group but this relationship will be modified by the spatial distribution of social classes within each city.

Residential Location and Electoral Conflict

This investigation is principally concerned with examining the empirical associations between specified ecological variables and patterns of electoral behavior. Previous studies have indicated the need for research identifying the linkages between contextual factors and political processes. To clarify these linkages,

¹¹ Essentially the same technique was used to determine high and low disagreement among classes. For fluoridation referenda, the dichotomy was 136.8; for education, 49.2; for parks, 44.2; for civic improvements, 68.8; for public works, 121.4.

¹² Social class dissimilarity is computed in this way:

$$D_i = \frac{\sum_{i=1}^N \left(\frac{X_{i1} - X_i}{d_{i1}} \right)^2}{\sum_{j=1}^M \left(\frac{M}{d_{ij2}} \right)^2},$$

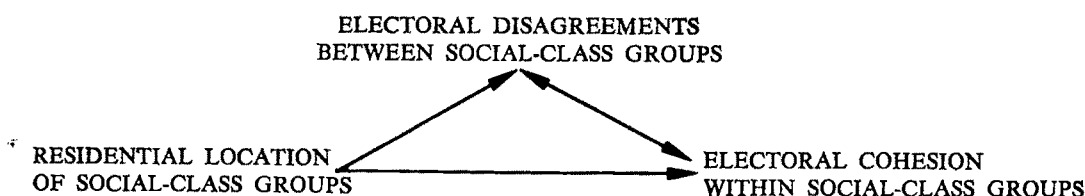
where D is the amount of dissimilarity between precincts in the i th case, and all surrounding precincts, X_i is the social class of the i th precinct, and X_{i1} is the rank of the i th precinct which surrounds the i th precinct. N_i is the number of precincts with data that are spatially proximate to the i th precinct. M equals maximum ($N_i - X_i, X_i - 1$), where N_i is the number of class groups ($N_c = 5$).

The numerator of the above ratio sums the rank dissimilarities between a precinct and its nearest surrounding precincts. This sum is weighted by the inverse of the square of the geographical distances (d_{i1}^2). (My thanks to W. Bruce Shepard, Oregon State University, for his assistance in developing this formula.) Social-class dissimilarity scores ranged from 5.497 to 22.730. For example, cities above 8.918 were categorized as high in dissimilarity, i.e., residentially integrated.

this paper suggests a model in which electoral cohesion within social-class groups and electoral disagreements between these groups are associated with the residential location of class groups in cities. In addition, the model implies that the association between interclass electoral disagreement and intraclass electoral cohesion is mutual in that each variable affects the other. The following indicates the direction of the hypothesized associations. As the model suggests, the associations between location and cohesion, and location and disagreement, are hypothesized to be direct and statistically significant, and the relationship between disagreement and cohesion is also assumed to be direct and significant.

data are not amenable to χ^2 tests of significance.¹³ The relationships were examined for each category of referendum to determine how much patterns of municipal conflict vary according to the policy content of local ballot issues. The associations between residential location and social-class electoral cohesion are presented in this first table.

Perhaps the most striking finding in Table 1 is the consistency in the association between location and cohesion. In each referendum the direction and strength of the statistical associations remain essentially unchanged. Those cities with a high degree of spatial segregation (whose class groups are ecologically isolated) tend to have classes with high electoral cohe-



Associations between the residential location of social-class groups and social-class electoral cohesion, between residential location and electoral disagreement, and between electoral disagreement and electoral cohesion were tested for statistical significance utilizing Fisher's Exact Test. This statistic is useful in the instance of bivariate tables where N is small and the

sion. Conversely, the spatial integration of class groups is significantly associated with the development of low electoral cohesion within classes. Although examination of Table 1 reveals variation by referendum type in the pro-

¹³ Sidney Siegel, *Nonparametric Statistics* (New York: McGraw-Hill Book Company, 1956), p. 96-104.

Table 1. Residential Location of Social-Class Groups and Electoral Cohesiveness
(Percentages)

Local Referendum Issue	Residential Location of Social-Class Groups	N**	Electoral Cohesion Within Class Groups	
			Low	High
<i>Fluoridation*</i>	Spatially Segregated	7	14	86
	Spatially Integrated	7	80	20
<i>Education*</i>	Spatially Segregated	10	10	90
	Spatially Integrated	4	75	25
<i>Parks*</i>	Spatially Segregated	5	—	100
	Spatially Integrated	3	100	—
<i>Civic Improvements*</i>	Spatially Segregated	7	—	100
	Spatially Integrated	5	60	40
<i>Public Works*</i>	Spatially Segregated	5	20	80
	Spatially Integrated	7	86	14

* Significant at 5 per cent level, Fisher's Exact Test.

** Number of cities.

portion of cities that display high electoral cohesion, this cohesion may be explained primarily by the spatial segregation of the class groups within those communities.

The findings in this table appear to be consistent with previous appraisals of the effects of residential segregation upon group cohesion. In general, segregated residential areas are thought to provide the physical contiguity for group cohesion and loyalty.¹⁴ Studies indicate that if citizens move into neighborhoods inhabited by people of similar class, this demographic homogeneity is influential in the development of psychosocial homogeneity.¹⁵ Similarly, other studies suggest that the class groups that are residentially located in homogeneous and segregated neighborhoods display distinctive group standards.¹⁶ Distinctive group standards may also affect the electoral cohesion of classes in voting in local referenda. The distributions in this first table confirm that residential segregation is associated with the presence of electoral cohesiveness and solidarity.

Since residential location may also affect the development of intergroup disagreements and friction, the residential location of class groups was statistically related to the degree of electoral disagreement between class groups. The associations are presented in Table 2.

¹⁴ Beshers, *Urban Social Structure*, p. 122.

¹⁵ Robert C. Tryon, *The Identification of Social Areas By Cluster Analysis* (Berkeley: University of California Press, 1955), p. 8.

¹⁶ Beshers, p. 111.

The patterns evident in Table 2 confirm that residential location significantly influences the amount of electoral disagreement between classes. Communities with segregated class groups display more electoral friction between classes than do integrated communities. Interestingly, the significant association was not found to fluctuate by referendum—a finding that is consistent with the analysis in the first table. The strong positive association between segregation and intergroup disagreement supports the inferences of the model, and in addition, may provide evidence to resolve some of the apparent contradictions of previous studies.

Several scholars have argued that intergroup conflict is a product of the spatial integration of incompatible groups. Physical proximity, they claim, may increase the probability of interaction which normally results in hostility and conflict between groups with distinctive standards.¹⁷ Additional studies have noted that spatial integration brings about increased accessibility between unlike groups, which produces intergroup frictions.¹⁸ Others speculate, however, that conflict between groups is influenced by residential segregation, leading to a decrease in interaction and communication, which further isolates groups.¹⁹ The speculation linking spatial integration to conflict and intergroup

¹⁷ D. W. G. Timms, *The Urban Mosaic* (Cambridge: Cambridge University Press, 1971), p. 2.

¹⁸ Michelson, *Man and His Urban Environment*, pp. 119–125.

¹⁹ Sirjamaki, *Sociology of Cities*, p. 270.

Table 2. Residential Location of Social-Class Groups and Electoral Disagreement
(Percentages)

Residential Location of Social-Class Groups		N**	Electoral Disagreement Between Social-Class Groups	
			Low	High
Fluoridation*	Spatially Segregated	7	—	100
	Spatially Integrated	5	80	20
Education*	Spatially Segregated	10	20	80
	Spatially Integrated	4	100	—
Parks*	Spatially Segregated	5	—	100
	Spatially Integrated	3	100	—
Civic Improvements*	Spatially Segregated	7	28	72
	Spatially Integrated	5	100	—
Public Works*	Spatially Segregated	5	20	80
	Spatially Integrated	7	86	14

* Significant at 5 per cent level, Fisher's Exact Test.

** Number of Cities

friction does not seem to be supported by the findings in this table, and therefore, may need qualification. Residential segregation appears to actually increase interclass disagreement rather than lower disagreement. Apparently, the association posited in the model between residential location and interclass electoral disagreement is confirmed by the data in Table 2.

The association between disagreement across class groups and electoral cohesion within them is imputed to be mutual and symmetrical. Analysis reveals that for all referenda, most of the cities that manifest high electoral disagreement between classes also display high electoral cohesion within class groups. This disagreement is not posited to cause cohesion because each variable is presumed to occur simultaneously. The more interclass disagreement exhibited in a city, the higher is the electoral cohesiveness, but if cohesion is taken as the independent variable, the higher the cohesion, the higher the disagreement becomes.

Although disagreement and cohesion are symmetrically related, it is easier to focus upon the influence of disagreement upon cohesion. The association has been found to be moder-

ately, but not statistically, significant. To ascertain whether or not the relationship may be real and not spurious, Table 3 presents the associations between disagreement and cohesion controlling for residential location. If the association is indeed spurious, i.e., if the two variables depend upon residential location for their value, rather than upon each other, then the moderate positive relationship between high interclass disagreement and high social-class electoral cohesion will be invalidated.

Analysis of Table 3 indicates that the association may, in fact, be spurious, because whatever the level of class disagreement, the residential location of class groups exerts strong influence upon cohesiveness. In each referendum, cities with spatially segregated class groups display high electoral cohesiveness irrespective of the degree of interclass electoral disagreement. It is also evident that spatially integrated communities generally possess low electoral cohesiveness uninfluenced by amount of electoral disagreement.

These conclusions suggest that the original model needs reformulation. Reliance upon Fisher's Exact Test indicates that the associa-

Table 3. Electoral Disagreement Between Classes and Cohesion
Within Classes Controlling for Residential Location
(Percentages)

Electoral Disagreement	Residential Location of Social-Class Groups Within Cities					
	<i>N</i>	Spatially Segregated Electoral Cohesiveness		<i>N</i>	Spatially Integrated Electoral Cohesiveness	
FLUORIDATION REFERENDA						
		Low	High		Low	High
Low	2	—	100	2	100	—
High	5	20	80	3	67	33
EDUCATION REFERENDA						
Low	3	—	100	3	67	33
High	7	14	86	1	100	—
PARKS REFERENDA						
Low	0	—	—	3	67	33
High	5	20	80	0	—	—
CIVIC IMPROVEMENT REFERENDA						
Low	4	25	75	3	67	33
High	3	—	100	2	50	50
PUBLIC WORKS REFERENDA						
Low	3	—	100	4	100	—
High	2	—	100	3	100	—

tions between location and cohesion, and location and disagreement are direct and statistically significant. As Table 3 reveals, the relationship between disagreement and cohesion may be spurious. Since residential segregation has been shown to affect cohesion and electoral disagreement, any association between cohesion and disagreement is significantly affected by the residential distribution of class groups.

These conclusions have been drawn from an analysis of social-class groupings within cities. Social-class segments constitute a useful and appropriate unit of analysis when attempting to draw inferences about the distribution and co-

areas of a city will display fewer electoral differences between themselves than do precincts of the same class when they are located in integrated areas. Precincts in these highly dissimilar, or integrated, areas will manifest a high degree of electoral disparity because the electoral cohesiveness associated with segregated areas is not developed. As the spatial distance increases between similar classranked precincts, the likelihood that these precincts will be located in residentially segregated sectors of a city will also tend to decrease, thereby raising the level of electoral disparity. The following relationship may be inferred from these hypotheses.

GEOGRAPHICAL DISTANCE BETWEEN PRECINCTS

SOCIAL-CLASS DISSIMILARITY
BETWEEN PRECINCTS

ELECTORAL DISPARITY
BETWEEN PRECINCTS

hesion of class groups. The utilization of class groups within cities as the unit of analysis, however, limits the number of observations under investigation and may increase the probability of error. A more specific level of analysis may provide the opportunity to examine additional hypotheses related to location and urban electoral behavior.

The study of precinct voting patterns furnishes the occasion for investigating spatial location and electoral cohesion on a more precise scale, and with an increased number of observations. It is the purpose of this inquiry to determine if the patterns evident in the prior model are supported by investigation at a different level.

Two hypotheses are presented for scrutiny: (1) Precincts displaying high social-class dissimilarity will tend to display high electoral disparity. Dissimilarity is a measure of the amount of class difference between a given precinct and all of its contiguous precincts. Disparity refers to the degree of electoral difference (defined as the percentage of the vote in favor of the referendum) between precincts of the same class rank, irrespective of their location within a city. (2) Precincts revealing high distance scores will tend to exhibit high electoral disparity. A distance score is a measure of the average geographical distance between a precinct and all other precincts of the same class. It is an indicator of the geographical dispersion of similarly ranked precincts.

The logic of these hypotheses is that precincts of the same class arranged in segregated

Social class dissimilarity and geographical distance occur simultaneously, but there is no theoretical ground for assuming any causal relationship, even though a weak relationship is evident upon examination of correlation matrices. Class differences (dissimilarity) do not logically produce a situation of increased geographical distance between precincts of the same rank, nor does the reverse pattern occur. This model posits that both dissimilarity and distance independently produce disparity. Table 4 presents the Kendall coefficients of correlation (τ) between these variables. Kendall coefficients are used because dissimilarity is an ordinal measure, and assumptions of normality cannot be inferred.²⁰

The finding most evident in Table 4 is that the coefficients between social-class dissimilarity and electoral disparity are generally of a high magnitude. As precincts increase in the amount of social-class dissimilarity they possess, they also display more electoral disparity between themselves and other precincts of the same class-rank. Interestingly, the coefficients between distance and disparity are also significant, but of a slightly lower magnitude than the associations between dissimilarity and disparity. Precincts revealing high distances between themselves and other similarly class-ranked precincts tend to manifest high levels of electoral disparity (voting difference between similar precincts). The data seem to indicate that the hypothesized relationships do exist, but that the strengths of relationships vary.

²⁰ Siegel, *Nonparametric Statistics*, pp. 213-223.

Table 4. Kendall Rank-Order Coefficients of Correlation (tau) Between
Dissimilarity and Electoral Disparity on Local Referenda

		Fluoridation	Education	Parks	Civic Improvements	Public Works
	N*					
Knoxville, Tenn	25	.69**	.41**	.32***		
St. Joseph, Mo.	69	.73**		.49**	.53**	.69**
Greensboro, N.C.	24	.75**				.57**
Lincoln, Neb.	97	.59**	.55**		.51**	.64**
Rockford, Ill.	148		.56**		.56**	.47**
Fremont, Cal.	65		.25**	.20**		
Lima, Ohio	67		.29**		.40**	.54**
Eugene, Ore.	75	.39**			.45**	.28**
Dayton, Ohio	180	.28**	.24**	.21**	.26**	.25**
Peoria, Ill.	113		.16**		.36**	
Yakima, Wash.	53	.41**	.33**	.53**	.24**	.33**
Berkeley, Cal.	124		.53**	.38**		
Trenton, N.J.	88	.28**	.49**			
Beaumont, Tex.	25		.27***	.19	.36**	.28***
Medford, Ore.	38	.88**	.65**		.42**	
Flint, Mich.	121	.75**	.64**			.52**
Allentown, Pa.	55	.58**	.52**	.38**	.52**	.59**
Sioux Falls, S.D.	29	.75**			.70**	.67**

Kendall Rank-Order Coefficients of Correlation (tau) Between
Distance and Electoral Disparity

		Fluoridation	Education	Parks	Civic Improvements	Public Works
	N*					
Knoxville, Tenn.	25	.47**	.21	.26***		
St. Joseph, Mo.	69	.51**		.31**	.46**	.48**
Greensboro, N.C.	24	.55**				.48**
Lincoln, Neb.	97	.38**	.34**		.46**	.50**
Rockford, Ill.	148		.32**		.48**	.39**
Fremont, Cal.	65		.18***	.11		
Lima, Ohio	67		.16***		.22**	.28**
Eugene, Ore.	75	.27**			.34**	.19**
Dayton, Ohio	180	.22**	.11***	.13**	.10***	.08
Peoria, Ill.	113		.14***		.18**	
Yakima, Wash.	53	.34**	.28**	.40**	.13	.17***
Berkeley, Cal.	124		.41**	.31**		
Trenton, N.J.	88	.20**	.36**			
Beaumont, Tex.	25		.13	.09	.21	.19
Medford, Ore.	38	.61**	.49**		.24***	
Flint, Mich.	121	.50**	.43**			.43**
Allentown, Pa.	55	.39**	.39**	.23**	.44**	.48**
Sioux Falls, S.D.	29	.48**			.42**	.36**

* No. of Precincts.

** Significant at .01 per cent.

*** Significant at .05 per cent.

To determine if the relationship between dissimilarity and disparity is spurious, and if it is largely through the geographical distance between precincts that dissimilarity influences high disparity, partial rank-order coefficients of correlation (tau) between dissimilarity and disparity are calculated controlling for distance.

The results of this partialling are presented in Table 5. When dissimilarity and disparity are correlated controlling for distance, the strong association is somewhat reduced and modified but not eliminated. The conclusion drawn from this finding is that the relationship is not spurious.

Table 5. Kendall Rank-Order Partial Coefficients of Correlation (τ)
Between Dissimilarity and Electoral Disparity*

		Fluoridation	Education	Parks	Civic Improvements	Public Works
	<i>N</i>					
Knoxville, Tenn.	25	.26	.36	.18		
St. Joseph, Mo.	69	.40		.37	.33	.25
Greensboro, N.C.	24	.51				.23
Lincoln, Neb.	97	.45	.41		.31	.47
Rockford, Ill.	148		.48		.28	.21
Fremont, Cal.	65		.19			
Lima, Ohio	67		.21	.10	.33	.38
Eugene, Ore.	75	.21			.37	.10
Dayton, Ohio	180	.13	.18	.14	.08	.19
Peoria, Ill.	113		.05		.17	
Yakima, Wash.	53	.11	.17	.35	.14	.07
Berkeley, Cal.	124		.41	.19		
Trenton, N.J.	88	.12	.21			
Beaumont, Tex.	25		.18	.16	.20	.24
Medford, Ore.	38	.60	.32		.19	
Flint, Mich.	121	.49	.36			.22
Allentown, Pa.	55	.36	.46	.44	.49	.29
Sioux Falls, S.D.	29	.42			.67	.59

* Controlling for Distance.

Kendall Rank-Order Partial Coefficients of Correlation (τ)
Between Distance and Electoral Disparity*

		Fluoridation	Education	Parks	Civic Improvements	Public Works
	<i>N</i>					
Knoxville, Tenn.	25	.08	.13	.11		
St. Joseph, Mo.	69	.23		.14	.09	.00
Greensboro, N.C.	24	.10				.13
Lincoln, Neb.	97	.05	.04		.00	.09
Rockford, Ill.	148		.14		.17	.13
Fremont, Cal.	65		.02	.07		
Lima, Ohio	67		.00		.04	.01
Eugene, Ore.	75	.13			.07	.05
Dayton, Ohio	180	.01	.03	.00	.09	.01
Peoria, Ill.	113		.10		.08	
Yakima, Wash.	53	.17	.12	.14	.02	.01
Berkeley, Cal.	124		.18	.15		
Trenton, N.J.	88	.06	.11			
Beaumont, Tex.	25		.02	.00	.12	.06
Medford, Ore.	38	.32	.25		.16	
Flint, Mich.	121	.26	.21			.18
Allentown, Pa.	55	.18	.19	.10	.23	.21
Sioux Falls, S.D.	29	.18			.15	.13

* Controlling for Dissimilarity.

The association of distance and disparity is also tested for spuriousness in Table 5 to ascertain the effect of dissimilarity upon the relationship. Examination of Table 5 indicates that the partial coefficient of correlation between distance and disparity is significantly reduced and almost eliminated. Usually, if the introduction

of a third variable into a partial correlation eliminates or reduces the original correlation, it may be inferred that the original relationship was spurious if the third variable, i.e., dissimilarity, occurred prior in time to either of the other variables. In this instance, however, distance and dissimilarity occur simultaneously;

therefore, the conclusion may be that the manner in which distance modifies disparity has been traced.²¹

In summary, distance alone does not cause disparity to vary, but rather, high dissimilarity provides the conditions under which the increasing distance between precincts produces high disparity. These findings generally support the conclusions previously developed that social-class electoral cohesion is associated with the residential location of social-class groups. The strength of the association between the social-class dissimilarity of precincts and the electoral disparity between precincts confirms the previous findings. Moreover, the hypothesized association between residential arrangements of class groups and class electoral cohesion has been supported by two levels of empirical investigation.

Summary and Conclusions

Ecologists have theorized that there is a geographical basis upon which human behavior depends and some have studied how the spatial relationships of humans have been affected by a variety of forces in the urban environment.²² To determine if the geographical distribution of social-class groups in a city has influenced the intensity of political conflict between classes, this paper has examined class voting responses to five types of referenda.

Those communities in this study that contained spatially segregated social-class groups also exhibited high social-class electoral cohesion and solidarity. Those groups located in areas of the city with few class differences appeared to have a commonality of voting behavior. Class groups not located in homogeneous areas did not exhibit this solidarity. In those cities, however, that had geographically fragmented classes with few segregated areas, the electoral cohesion of each class was low. A social-class group located in an area of wide class dissimilarity was likely not to vote in concert

with other groups of the same social class located elsewhere in the city. The geographical separation of precincts of the same class would appear to produce voting behavior that is not uniform or consistent within that class group.

The segregation of population groups may tend to fuse neighborhood sentiment together. The adherence to common values and attitudes within each group is mirrored in the cohesiveness in voting by the groups within the area. Apparently certain areas in a city attract groups that come to possess similar political viewpoints. That this process of mutual attraction occurs seems to be indicated by the low variation in voting within residentially segregated areas of cities.

Interestingly, the cohesiveness of class groups was not affected by the type of issue voted upon by the residents of the city. Presumably distinct referendum issues, which appear to exhibit different policy contents, would elicit varying responses from the electorate. The analysis indicated, however, that each referendum type was associated with similar patterns of class cohesion.

Although location influenced intraclass cohesion, the spatial patterning of classes in each city also affected the level of disagreement between class groups. Communities with segregated classes exhibited a high degree of social-class cohesion and solidarity. Conflict theorists have contended that such a situation may create intense intergroup conflict. The findings of this paper, however, did not fully confirm those assertions. Instead, interclass disagreements appeared to be associated more with the geographical distribution of class groups than with class solidarity. Those cities that displayed segregated social-class groups also manifested higher levels of interclass disagreements than those social class groups in spatially integrated communities.

In summary, ecologists have contended that location is important in determining the behavior of population groups; the conclusions of this paper seem to support these contentions. Although this study is somewhat limited in scope, the findings indicate that the theoretical contributions of human ecology may be helpful in explaining certain patterns of local politics and the conflicts that shape them.

²¹ Claire Selltitz, Marie Jahoda, Morton Deutsch, and Stuart W. Cook, *Research Methods in Social Relations* (New York: Holt, Rinehart and Winston, 1959), p. 429.

²² For a thorough discussion of these concepts, see Robert Park, Ernest Burgess, and Roderick McKenzie, *The City* (Chicago: The University of Chicago Press, 1925).

The Politics of Redistribution: A Reformulation*

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Fry and Winters introduced a new dimension of public policy output analysis in a recent article published in this *Review*.¹ In many ways their effort is a truly groundbreaking endeavor. It is imaginative, innovative and raises several important questions. As we shall suggest in the following analysis, however, the techniques Fry and Winters employed, while appropriate, were misused in several ways. After reformulating their analysis, we have found that the calculated values of their redistribution index as well as the nature of their findings, change substantially.

Fry and Winters's study is concerned with two broad questions: 1) Does politics make a difference in public policy outcomes, and 2) How are certain factors (socioeconomic and political variables) specifically related to policy outcomes?

Fry and Winters contend that previous studies addressed to these questions were formulated to bias the results against political variables. They suggest that:

... prior findings have been the result of the examination of a measure of public policy in which the influence of the political system is likely to be negligible, that is levels of public revenues and expenditures. To examine this proposition empirically, our study shifts attention to the allocation of the burdens and benefits of state revenue and expenditure policies across income classes. In redirecting analysis to allocations rather than levels

of state revenues and expenditures, we focus on a province we believe to be more predictably political.²

Their alternative is to derive a new measure of public policy output:

We have taken as our dependent variable the net redistributive impact of revenues and expenditures as represented by the ratio of expenditure benefits to revenue burdens for the three lowest income classes in each state. The major hypothesis of our study is that, in regard to the allocation of the burdens and benefits of state government revenues and expenditures, political variables will have a stronger influence on policy outcomes than will socioeconomic variables.³

They raised the question of the impact of politics on public policy outcomes because most recent expenditure studies have suggested that political variables have little or no impact on policy outcomes.⁴ In particular, the results have shown that socioeconomic variables seem to be more strongly related to state revenue and expenditure levels than are political variables, although the results have varied by function.⁵ Given these findings, Fry and Winters state:

... The political analyst may have to look beyond levels of taxes and expenditures to find politics having an independent or dominant influence on policy outcomes in the states. ... As Key originally

² Fry and Winters, p. 508.

³ Fry and Winters, p. 508.

⁴ This literature starts with Richard E. Dawson and James A. Robinson, "Inter-Party Competition, Economic Variables and Welfare Policies in the American States," *The Journal of Politics*, 25 (May, 1963), 265-289. Fry and Winters provide a concise review of the literature. For a critique of the literature in this field see, Joan B. Booms and Bernard H. Booms, "A Critique of Recent Public Policy Output Studies," unpublished paper, The Pennsylvania State University, 1972.

⁵ Fry and Winters, "The Politics of Redistribution," p. 510. Others have also pointed to the interesting fact that public welfare is the one function for which outcomes are least well accounted. See, for example, Peter S. Albin and Bruno Stein, "The Determinants of Relief Policy at the Sub-Federal Level," *Southern Economic Journal*, 37 (April, 1971), 446. Since public welfare is mainly a redistributive program, this finding suggests that redistributive elements of public policy are not well accounted for in previous studies.

* While this paper was being written, Mr. Halldorson was a Masters Degree candidate in the Department of Economics, The Pennsylvania State University. Michael R. King and Robert S. Friedman of the Political Science Department offered their valuable comments and criticisms on earlier versions of this paper. The authors gratefully acknowledge their help. Special thanks are also due Joan B. Booms, and our colleagues in the Economics Department, Teh-wei Hu, D. Lynne Kalreider, and James D. Smith for their interest and help. Stuart Scantlebury provided essential computation assistance. Special mention needs to be made of Richard F. Winters's open willingness to discuss the techniques he and Brian R. Fry used in their study and to provide the authors with some of the data used in their study.

¹ Brian R. Fry and Richard F. Winters, "The Politics of Redistribution," *American Political Science Review*, 64 (June, 1970), 508-522.

hypothesized, a fruitful area of search for such influence may be the redistributive policies of state governments.⁶

Public policy studies to date have been concerned with the allocation dimension of public policy, i.e., *how much* is provided for what program or function. In other words, research has emphasized the *levels* of resources allocated for alternative functions, implying that most public programs can be considered primarily allocative in nature. This view seems somewhat unrealistic. Obviously, public assistance is aimed purposely at redistribution within society, but most other public functions are not specifically designed for redistribution, but rather for the provision (allocation) of services that either would not be produced by the private sector because of total market breakdown, or would be underproduced because of partial market failure.⁷

Few, if any, of these allocation programs, however, are neutral in distribution impact. "Even programs that apparently benefit most of the population—such as education and highway construction—have a variable incidence of benefits."⁸ So virtually all government programs contain an element of redistribution. Fry and Winters suggest that a look at the redistributive aspects of allocation should constitute a new focus for research because it is here that politics may play a central role.

We add that it is important to study redistribution effects because they are an important determinant of the quality of life for many people in our society. The allocation of public burdens and benefits is a major point of conflict in our society. It is obviously advantageous to know something about the factors influencing the distribution of public outcomes.

Incidental to this concern of knowledge for policy purposes is the question of whether political, socioeconomic, historical, or geographical variables are more helpful in studying this question. *A priori* Fry and Winters's contention that political variables should be most important in determining the distribution of output seems reasonable. While the *level of funds*

spent seems more likely to be a function of ability-to-pay factors such as income, *who pays* and *who benefits* from a *given* level of operation may lie more in the realm of politics. It might be hypothesized that political forces design the program and influence its administration.⁹

Fry and Winters's Method

Fry and Winters sought to measure the "net redistributive impact of revenues and expenditures" in each state expressed as a ratio of expenditure benefits to tax burdens for the lowest three income classes.¹⁰ The process by which these ratios were derived was adapted from a Tax Foundation Study (1967).¹¹ The purpose of that study was

to provide broad estimates of (1) the total tax burden—Federal and state-local—on families and unattached individuals by income class and major type of tax, and (2) the benefits of government expenditures by major groups of programs.¹²

Bases of allocation were assumed for each major tax and expenditure category in order to explain the incidence of burdens and benefits to families in each income class. The statistical bases of allocation used by Fry and Winters were taken from the Bureau of Labor Statistics (BLS), *Survey of Consumer Expenditures 1960–1961*¹³ which reported families' average dollar expenditures and average dollar income receipts for specific expenditure and income categories by income class. In this study (BLS) expenditures and income were reported for the entire nation for both urban and rural families. Fry and Winters adapted these data to determine the percentage distribution of tax burdens and expenditure benefits for each income class by state.¹⁴

⁹ John P. Crecine presents some evidence which tends to support these statements. His results seem to indicate that level of expenditures and revenues is, in the short run, relatively immune to political pressure. He concludes that political pressure and influence are more likely to affect whose street gets repaired, and how much is spent per pupil at a school, rather than the level of the total street repair or school district expenditures. See John P. Crecine, *Government Problem Solving: A Computer Simulation of Municipal Budgeting* (Chicago: Rand McNally, 1969).

¹⁰ Fry and Winters, "The Politics of Redistribution," p. 513.

¹¹ Tax Foundation, Inc., *Tax Burdens and Benefits of Government Expenditures By Income Class, 1961 and 1965* (New York: Tax Foundation, Inc., 1967).

¹² Tax Foundation, p. 7.

¹³ U.S. Department of Labor, Bureau of Labor Statistics, *Survey of Consumer Expenditures, 1960–1961*, Report No. 237–93, February 1965, and supplements.

¹⁴ For example, it was assumed that expenditures for automobile operations served as the basis of allocation

⁶ Fry and Winters, p. 510.

⁷ See Richard A. Musgrave, *The Theory of Public Finance* (New York: McGraw-Hill, 1959), for a discussion of the distinction between the allocative and distributive functions of government, and a definition of public outcomes based on the performance of the market.

⁸ Herbert Jacob and Michael Lipsky, "Outputs, Structure, and Power: An Assessment of Changes in the Study of State and Local Politics," *Journal of Politics* (May, 1968), 515, cited in Fry and Winters, "The Politics of Redistribution," p. 510.

The Tax Foundation considered each revenue and expenditure category for the federal and state-local levels of government. The bases of allocation were then applied to the aggregate total amounts collected and expended by each level of government to determine the expenditure benefits and tax burdens for each income class for the entire nation. It is important to note that the statistical bases of allocation were adapted from national averages for expenditures and income, and referred to a *national distribution of families by income classes*.

Fry and Winters adapted directly the method used in the Tax Foundation study to determine the distribution of benefits and burdens for states' expenditures and revenues by income class for each state. To do this Fry and Winters assumed those bases of allocation from the Tax Foundation study that were applicable to state government expenditures and revenues (see Tables 1 and 2). They also adapted the same statistical bases of allocation used by the Tax Foundation and converted them into percentage amounts to explain the percentage distribution of burdens to benefits by income class for each expenditure and revenue category. Because they assumed the statistical bases of allocation from the Tax Foundation study, they used one set of percentages for the distribution of revenue burdens and one set for expenditure benefits. These two sets of percentages were then applied to each state's expenditures and revenues to determine the actual amount of tax burdens and expenditure benefits by each income class for each state. They then summed the amount received as expenditure benefits,

and the amounts spent as tax burdens for each state for the lowest three income classes, i.e. those with an income under \$4,000. Finally, they expressed these amounts as a ratio of expenditures to revenues in each state.

Critique of Fry and Winters's Methods

This technique, which should yield a meaningful approximation of the distribution and benefits from the public sector, is not problem-free. Fry and Winters acknowledge this, citing some of the "problems"—the necessity of selecting somewhat arbitrarily the allocation bases from which they constructed their indices, and the assumption that the consumption pattern of a given income group does not vary from state to state.¹⁵ This latter assumption results from the lack of Bureau of Labor Statistics data on a state-by-state basis. Fry and Winters thus necessarily assume that people in the \$3,000 to \$3,999 group (regardless of the state in which they reside) spend the same percentage of their income on the allocation bases commodities (e.g., cigarettes, automobile operation). While this is clearly not true for all goods, the items used here for assigning benefits and burdens to an income class are universal American consumption items. Thus this assumption seems realistic.

The above-cited problems do not invalidate their calculations and results; however, Fry and Winters make some errors in using the technique which *do* seem to destroy much of the value of their work and cast doubt on their results. We will discuss these errors, describe our reformulation and then compare the Fry and Winters findings with our own.

The *major* error committed by Fry and Winters in deriving their redistribution index is their application of the Tax Foundation's calculated distributions of expenditures and revenues (which refers to the U.S. as a whole) to each state. The Tax Foundation used expenditures by each income class as a percentage of the total; in doing this, they validly based their work on national figures for the number of families in each income class as reported by the Bureau of Labor Statistics. *But* this procedure should not be followed when developing separate state-by-state estimates of redistribution, since the number of families in each income class varies widely across the states.

Fry and Winters adapt the bases of allocation used by the Tax Foundation in order to explain the distribution of tax burdens and expenditure benefits by income class. The Tax

for motor vehicle taxes. The average amount spent for automobile operations in each income class was multiplied by the number of families in that income class to determine the total amount spent by each income class for automobile operations. This amount was summed across income classes to determine the amount spent by all families in all income classes. The amount spent by each specific income class was then taken as a percentage of that total. If the families in the income class of \$15,000 and over accounted for 10 per cent of all automobile operation expenditures, they were allocated 10 per cent of the burden for Motor Vehicle taxes. Similarly, on the expenditure side, it was assumed that benefits of government highway expenditures were allocated to each income class according to one-half its expenditures for automobile operation and one-half its expenditures for total current consumption. The same process was repeated to determine the percentage of the total amount spent for automobile operation and total current consumption by each income class. If the income class of \$15,000 and over accounted for 12 per cent of expenditures for total current consumption and 8 per cent of expenditures for automobile operation, they would then be allocated 10 per cent of the benefits for government highway expenditures.

¹⁵ Fry and Winters, pp. 514–515.

Table 1. Bases of Allocation for State Government Expenditures

Expenditure Category	Basis of Allocation
<i>Direct Expenditures</i>	
Education	
Elementary and Secondary	Number of children under 18.
Higher and Other	Higher education expenditures.
Highways	Half automobile operation and half total current consumption.
Public Welfare	Income from public social assistance and private relief.
Agriculture	Farm money income before taxes.
Insurance Trust	Public unemployment and social security benefits.
Interest	Interest income.
Other—General	Half money income before taxes and half number of families and unrelated individuals.
<i>Intergovernmental</i>	
Education	Half number of children under 18 years of age and half higher education expenditures.
Highways	Same as Direct Highway Expenditures.
Welfare	Same as Direct Welfare Expenditures.
General	Same as Direct General Expenditures.

Foundation took the average amount spent or received in each category as reported by the Bureau of Labor Statistics and multiplied it by the number of families in each income class and summed these amounts across classes to determine the amount spent or received by all income classes for each category for the country as a whole. Fry and Winters took this amount spent by each income class, and expressed it as a percentage of the total amount spent by all income classes. These figures were assumed to express the proportion of burdens and benefits allocated to each class. The single set of percentages, derived as it was from national distributions, was then applied by Fry and Winters to each state's revenues and expenditures to determine the amounts spent and received by the income classes in each state.

This method of index construction is inappropriate because it is derived from percentages based on a *national* distribution of families by income class (from the Bureau of Labor Statistics' study). The proportion of families in each income class obviously varies from state to state.¹⁶ Consequently, the application of these percentages to each state's revenues and expenditures erroneously assigns burdens and benefits to income classes. For example, if on the na-

¹⁶ An examination of 1960 Census data proves that the distribution of families by income class does vary significantly from state to state. As an illustration, the national percentage of families and unattached individuals in the income class of \$3,000 to \$3,999 is 11.0 per cent as reported by the BLS. According to 1960 Census data, the percentage of families who earn from \$3,000 to \$3,999 varies from 6.2 per cent in Connecticut to 14.3 per cent in Maine.

Table 2. Bases of Allocation for State Government Revenues

Revenue Category	Basis of Allocation
<i>Taxes</i>	
Sales and Gross Receipts	
Alcoholic Beverage	Alcoholic beverage expenditures.
Tobacco	Tobacco expenditures.
Motor Vehicle	Automobile operation expenditures.
All Other	Total current consumption.
Individual Income	Personal taxes.
Corporation	Half total current consumption and half dividend income.
Property Tax	Half total current consumption and half housing expenditures.
Death and Gift	Completely to the \$15,000 and over income class.
All Other	Total current consumption.
Social Insurance Contributions	Combination of social security, railroad, and government retirement contributions and total current consumption.
Charges and Miscellaneous	Total current consumption.
Intergovernmental	Distribution of federal taxes.

tional level, the income class of \$15,000 or over spends 10 per cent of the total spent for automobile operation. Fry and Winters assign 10 per cent of the burden for Motor Vehicle taxes to this income class in each state. But in some states, the proportion of families who make \$15,000 or more is, for example, less than the national average, and thus in those states this income group accounts for less than 10 per cent of the total spent on automobile operation expenditures. By Fry and Winters's method this income class would be allocated 10 per cent of the burden of motor vehicle taxes in each state no matter what the distribution of families who earn \$15,000 or more is in that state.

A less serious error is Fry and Winters's inclusion, as part of revenues to be allocated, of those connected with the sale and consumption of alcoholic beverages. In 16 states liquor retailing is limited by law to state owned monopoly liquor stores. The revenue from these stores is correctly included by Fry and Winters as an item of burden to be allocated. However, Fry and Winters also include purchases by state liquor stores for resale as an item of benefit to allocate and thus as a factor in calculating their output measure. There seems to be no clear rationale for doing this. Not since prohibition have federal or state governments decreed the amount of liquor *purchases*. Purchase amounts are not the subject of public policy. Also, since the official figures on liquor store expenditures reflect total price, i.e., both price of item and tax, and since only figures for states with government monopoly on sales are collected, an unknown bias is introduced into the calculation of the redistributive index. The extent of this bias must be investigated, or the calculations corrected, before the redistribution measure can be accepted as actually measuring what it is purported to measure. In reformulating a redistribution index, we correct for Fry and Winters' handling of revenues and benefits allocated on the basis of the sales of alcoholic beverages, by excluding liquor store expenditures from consideration.

The Method of Reformulation

To provide a measurement of distribution of the tax burdens and expenditure benefits on a state-by-state basis, it would be ideal to have data on consumer expenditures and income by income class for each state. Unfortunately, the extensive study done by the BLS is the only one that provides sufficient figures of families' expenditures and income by income-class. As mentioned above, due to the sampling

method that study does not provide data on a state-by-state basis.

It is the purpose of the reformulation to adapt the national figures provided by the BLS for a state-by-state analysis. Instead of using one set of national averages for the number of families and single consumers in each income class, as Fry and Winters did, the reformulation accounts for the distribution of families and single consumers by income class in each state.

The reformulation employs seven equations that determine the distribution of burdens (benefits) for each category of revenues (expenditures) to each income class, as well as the redistribution of income. The reformulation determines burdens and benefits, along with the redistribution ratios, for each category of revenues and expenditures, for each income class, in each state. (See Appendix 1 for equations.)

The first equation considers the BLS categories of expenditures and income that serve as the bases of allocation for the corresponding categories of state expenditures and revenue. (See Tables 1 and 2 for bases of allocation.) The average amount spent (received) by each income class for each BLS category is divided by the average money income before taxes for each income class. The average amount spent (received) for each BLS category is presented as a percentage of the average money income before taxes for all nine income classes.

The second equation derives the aggregate total money income before taxes for all families and unattached individuals in each income class for each state. The *distribution* of families and unattached individuals, by income class in each state is accounted for here, thus overcoming the major error of the Fry and Winters study.

The third equation derives the average dollar amounts of expenditures and income for each category that serves as a basis of allocation to the corresponding categories of state revenues and expenditures.

The fourth equation sums the average dollar amounts of expenditures (income) across all nine income classes. Each individual income class is then taken as a percentage of this total.

The fifth equation derives the actual dollar amount of burdens for state government revenues and benefits of state government expenditures for each income class in each state.

The sixth equation sums the dollar amount of benefits for all twelve categories of expenditures, for each income class, as well as the burdens for all twelve categories of revenues for each income class in each state.

The final equation derives the ratio of redis-

tribution for the lowest four income classes, i.e., those earning less than \$4,000 by dividing total expenditure benefits received for the lowest four income classes by the revenue burdens paid out for the same income classes.

A Comparison of Results

We now turn to the relationship among our index of redistribution, that of Fry and Winters, and facets of the social, economic, and political structure of the states. Our results are presented in the same manner as Fry and Winters's in order to facilitate comparisons.¹⁷

Table 3 gives the redistribution ratios for the subject states. A rank ordering of the states according to each version of the index is also provided. It is obvious the absolute values of the two versions of the index differ greatly, and the rank ordering shifts significantly. (The simple correlation between the two indices is .63.) These discrepancies seem to indicate that our revision is an important change. The errors we discussed above appear to have more than just academic import.

To check for further differences between the two approaches, a state-by-state comparison would be ideal. Unfortunately, such a direct comparison is not possible since Fry and Winters do not list the figures on the amount of burdens and benefits for each state. One possible means of comparison, however is to examine the single set of percentage distributions presented by Fry and Winters and the percentage distributions for *each* state obtained in the reformulation, but not presented here because of space limitations. Such a comparison bears out the thesis of the reformulation. For example, the percentage allocation for the burden of motor vehicle taxes for the income class of \$3,000 to \$3,999 is 8.2 per cent according to Fry and Winters's methods, while the percentages yielded by the reformulation ranged from 1.7 per cent in Idaho to 12.6 per cent in Arkansas for the same category and the same income class. Similar striking differences were found for most categories and income classes.

After constructing their index of redistribution, Fry and Winters proceeded to test several basic hypotheses regarding the factors associated with redistribution. The results of these tests are found in Table 6, Summary of Findings, in their article. For purposes of comparison and illustration only, we carried out identi-

Table 3. A Comparison of Ratios

Reformulation		State	Fry & Winters*	
Rank	Ratio		Rank	Ratio
1	6.004	Massachusetts	1	3.320
2	5.904	Illinois	10	2.376
3	5.804	California	12	2.322
4	5.385	Colorado	7	2.464
5	4.700	Connecticut	5	2.486
6	4.680	Washington	25	2.093
7	4.348	New York	3	2.644
8	4.342	Ohio	16	2.242
9	3.900	New Jersey	21	2.135
10	3.694	Missouri	2	2.712
11	3.679	Idaho	18	2.205
12	3.549	Michigan	34	1.920
13	3.501	Kansas	30	1.998
14	3.375	Rhode Island	6	2.482
15	3.372	Maryland	33	1.923
16	3.348	Indiana	42	1.793
17	2.995	Pennsylvania	23	2.107
18	2.958	Oregon	8	2.446
19	2.785	Minnesota	24	2.098
20	2.774	Nevada	39	1.826
21	2.653	Oklahoma	4	2.567
22	2.590	West Virginia	28	2.011
23	2.559	Utah	32	1.954
24	2.522	Wisconsin	11	2.340
25	2.522	Georgia	22	2.127
26	2.482	Iowa	29	2.001
27	2.435	Arizona	46	1.694
28	2.398	Kentucky	9	2.428
29	2.304	Alabama	14	2.267
30	2.216	Tennessee	27	2.031
31	2.211	Maine	26	2.060
32	2.198	Mississippi	13	2.274
33	2.184	Wyoming	47	1.660
34	2.166	Delaware	20	2.190
35	2.128	Nebraska	40	1.813
36	2.126	North Carolina	35	1.900
37	2.126	Florida	36	1.850
38	2.123	Louisiana	15	2.252
39	2.036	Arkansas	17	2.212
40	1.971	New Mexico	44	1.720
41	1.910	Montana	31	1.962
42	1.908	Vermont	19	2.199
43	1.845	New Hampshire	38	1.830
44	1.740	South Carolina	43	1.775
45	1.728	North Dakota	37	1.845
46	1.661	Virginia	48	1.620
47	1.574	Texas	41	1.800
48	1.537	South Dakota	45	1.715

* Source: Fry and Winters, p. 515.

¹⁷ The error of assuming a uniform income distribution among states made it possible to use only two tables, one for burdens, the other for benefits. Our technique yields two such summary tables for *each* state so it is impossible to include these summary tables.

cal tests using the reformulated version of the dependent variable.

It should be pointed out there are some statistical problems with the method Fry and Winters used to test their hypotheses. The most ob-

vicious problem is one of multicollinearity.¹⁸ The existence of heavy multicollinearity still yields unbiased estimates for the regression coefficients but inflates their standard errors. This, in effect, lowers the chance of a variable's being

¹⁸ See comment by James J. Noell and reply by Fry and Winters. *American Political Science Review*, 64 (December, 1970), 1249-1251. An unknown referee pointed out a problem that neither Noell nor we mention, viz., that caution should be used in choosing which income variables to include as independent variables, since the dependent variable is in part income-defined. Obviously it would be meaningless to regress a variable on its own constituent parts.

statistically significant and thus may lead to improper assumptions concerning the absence of correlation between the dependent variable and specific independent variables. In essence, the danger is not of erring in favor of finding a statistically significant correlation when one does not exist, but, rather, of missing true correlation linkages.

Noell states without specific evidence that multicollinearity exists in the Fry and Winters regressions. Our efforts provide evidence to prove this allegation. A test of orthogonality on the determinate of the correlation matrix of in-

Table 4. Summary of Findings
48 States

	Hypothesized Relation	Zero-Order	Partial	Regression Coefficient	Beta Coefficient
I. Variables					
<i>Socioeconomic Variables</i>					
<i>Ability to Pay</i>					
Median Income	+	.65 (.18)	.42 (-.27)	13.33**	1.11
Industrialization	+	.43 (.29)	.06 (-.02)	7.66	.05
Urbanization	+	.67 (.34)	.30 (.15)	26.21*	.33
Education	+	.39 (-.01)	.45 (.17)	510.33**	.48
<i>Need (Demand)</i>					
Gini Index	+	-.43 (.00)	-.18 (.22)	-82.68	-.21
% under \$3,000	+	-.56 (-.14)	.60 (-.07)	243.58**	2.07
<i>Political Variables</i>					
<i>Mass Political Behavior</i>					
Political Partic.	+	.34 (.14)	.40 (.37)	25.79**	.41
Democratic Vote	+	-.30 (-.06)	.06 (.11)	5.10	.06
Interparty Comp.	+	.24 (-.21)	.14 (-.14)	18.92	.08
Leg. Inducements	+	.37 (.03)	-.10 (.07)	-38.95	-.07
<i>Government Insts.</i>					
Apportionment	-	.07 (.04)	-.17 (.01)	-4.65	-.09
Leg. Party Cohesion	+	.50 (.24)	.20 (-.05)	1240.88	.11
Governor Power	+	.52 (.26)	-.10 (.12)	-259.89	-.07
Governor Tenure	+	.47 (.17)	.36 (.12)	2343.20**	.24
<i>Elite Behavior</i>					
I. G. Strength	-	-.23 (-.04)	-.25 (-.17)	-2408.95	-.19
Civ. Ser. Coverage	+	.44 (.33)	.20 (.34)	5.46	.14
Leg. Professionalism	+	.56 (.51)	.20 (.28)	2.34	.20
Innovation Index	+	.70 (.46)	.41 (.07)	60.96	.44
Constant				-182305.72	
II. Multiple Coefficients of Determination					
			<i>R</i> ²		
All Variables			.64 (.55)		
Socioeconomic Variables			.56 (.17)		
Political Variables			.72 (.38)		
III. Multiple-Partial Coefficients of Determination*					
Political Variables Controlled for Socioeconomic Variables			.64 (.46)		
Socioeconomic Variables Controlled for Political Variables			.42 (.27)		

* Indicates significant at 10% level for a two-tailed *t* test.

** Indicates significant at 5% level for a two-tailed *t* test. Two tailed tests were used in order to test both the sign and the magnitude of the coefficients.

Numbers in parentheses are from Fry and Winters, p. 520.

* For a discussion of this statistic see Hubert M. Blalock, *Social Statistics* (N.Y.: McGraw-Hill, 1960), pp. 350-351.

Table 5. Summary of Findings

36 Non-South States

	Hypothesized Relation	Zero-Order	Partial	Regression Coefficient	Beta Coefficient
I. Variables					
<i>Socioeconomic Variables</i>					
<i>Ability to Pay</i>					
Median Income	+	.64 (.23)	.45 (–.40)	13.71*	.79
Industrialization	+	.49 (.33)	.04 (–.05)	4.97	.03
Urbanization	+	.69 (.42)	.47 (.23)	44.35**	.52
Education	+	.20 (–.08)	.54 (.19)	644.08**	.43
<i>Need (Demand)</i>					
Gini Index	+	–.20 (.13)	–.23 (.47)	–107.70	–.18
% under \$3,000	+	–.54 (–.21)	.63 (–.23)	294.08**	1.44
<i>Political Variables</i>					
<i>Mass Political Behavior</i>					
Political Partic.	+	–.007 (.06)	.51 (.36)	39.05**	.31
Democratic Vote	+	.15 (.11)	–.08 (.17)	–13.90	–.05
Interparty Comp.	+	.16 (–.16)	.25 (–.19)	30.12	.14
Leg. Inducements	+	.21 (.11)	–.24 (.33)	–105.98	–.13
<i>Government Insts.</i>					
Apportionment	–	.001 (.10)	–.20 (.09)	–5.64	–.11
Leg. Party Cohesion	+	.41 (.25)	.18 (.00)	1141.11	.09
Governor Power	+	.53 (.24)	–.24 (–.04)	–734.74	–.18
Governor Tenure	+	.45 (.12)	.45 (.17)	2958.10*	.33
<i>Elite Behavior</i>					
I.G. Strength	–	–.08 (–.01)	–.17 (–.07)	–1630.16	–.13
Civ. Ser. Coverage	+	.47 (.35)	.40 (.48)	10.90*	.27
Leg. Professionalism	+	.64 (.56)	.21 (.14)	2.83	.26
Innovation Index	+	.68 (.50)	.34 (.17)	54.06	.39
Constant				–196966.05	
II. Multiple Coefficients of Determination					
			<i>R</i> ²		
All Variables			.87 (.67)		
Socioeconomic Variables			.53 (.28)		
Political Variables			.72 (.46)		
III. Multiple-Partial Coefficients of Determination^a					
Political Variables Controlled for Socioeconomic Variables			.72 (.54)		
Socioeconomic Variables Controlled for Political Variables			.53 (.39)		

* Indicates significant at 10% level for a two-tailed *t* test.** Indicates significant at 5% level for a two-tailed *t* test. Two tailed tests were used in order to test both the sign and the magnitude of the coefficients.

Numbers in parentheses are from Fry and Winters, p. 520.

^a For a discussion of this statistic see, Hubert M. Blalock, *Social Statistics* (N.Y.: McGraw-Hill, 1960), pp. 350–351.

dependent variables showed values of .0000000069 and .0000001525.¹⁹ Since a value of unity means no multicollinearity, and a value of zero indicates the matrix is singular, or perfect multicollinearity exists, problems of multicollinearity are obviously present.

There are also some difficulties in interpreting Fry and Winters's presentation of their find-

ings. No measure is provided of the impact that a change in each variable has on the dependent variable (other things constant). Regression coefficients, which provide a quantitative measure of these impacts, are useful for judging the potential impact of alternative policy changes or outcomes. Regression coefficient values as well as beta coefficient values are presented in Table 4. Beta coefficients can be used to measure the importance of individual explanatory variables for the calculated value of the dependent variable, just as partial correlation coeffi-

¹⁹ For a description of this test, see Donald E. Farrar and Robert R. Glauber, "Multicollinearity in Regression Analysis: The Problem Revisited," *Review of Economics and Statistics*, 49 (February, 1967), 92–107.

cients are used for this purpose.²⁰ This measure provides an alternative method for comparing the relative importance of variables in "explaining" the dependent variable. Note that the regression coefficient is inappropriate for this purpose, since the size of the coefficient can be changed by changing the units of measurement for the variable. Regression coefficients show the degree of impact, not degree of correlation. For these reasons, we have computed regression and beta coefficients in addition to the various correlation techniques used by Fry and Winters. The results are presented in Table 4. In interpreting the table, one should bear in mind that while the zero-order and partial correlation coefficients allow one to make statements about the degree of association among variables, the regression coefficient gives a quantitative measure of the impact of changes in the independent variable on the dependent variable. As was indicated above, this statistic allows for judgements regarding proposed policy changes. It also points to what might be altered if significant changes in redistribution are desired.²¹

Table 4 shows major differences between our results and those of Fry and Winters. Several are particularly interesting. Note that by reformulating the redistribution index, we have raised considerably the relative explanatory power of the socioeconomic variables (from .17 to .56 for the multiple coefficient of determination, and from .39 to .53 for the multiple-partial coefficient of determination). More significantly, we find that the amount of explained variance between all 48 states and the non-South states (Table 5) is very nearly identical (.84 and .87), contrary to Fry and Winters's results (.55 and .67). This finding tends to make Fry and Winters's lengthy explanation of South vs. Non-South differences less meaningful.

Also interesting is that the relationship between interparty competition and redistribution changes direction. Contrary to Fry and Winters's own findings, when our index is related to

competition, the data support the original Fry and Winters hypothesis; namely, that competition leads to greater redistribution.

The primary purpose of this paper was to reformulate a redistribution index for the impact of state government activities on the incomes of individuals and to show how such a reformulation was important because it yielded results significantly different from those originally obtained by Fry and Winters. The above regression analysis was carried out for comparison purposes only. In addition to the few specific comparisons made here, others could be made and important hypotheses tested; this work however awaits more appropriate analytical techniques.

APPENDIX I

Methodological Appendix

Presented below are the seven equations that were used to determine the distribution of burden (benefits) for each category of revenues (expenditures) to each income class as well as the redistribution of income. (See pp. 928 and 929 in text for a discussion of these equations.)

Equation 1:

$$P_{ih} = \frac{(\bar{C}_{ih})}{\bar{Y}_i}$$

P_{ih} = Percentage of money income before taxes spent (received) for BLS categories of expenditures and income that serve as bases of allocation

h = Bases of allocation that correspond to state expenditure and revenue categories.
 h from 1 to 24

i = Income classes. i from 1 to 9

\bar{C}_{ih} = Average dollar amounts of expenditures and income for bases of allocation

\bar{Y}_i = Average money income before taxes

In some cases the BLS categories had to be combined. Therefore,

$$(\bar{C}_h), \left[\bar{C}_h = \sum_{i=1}^9 \bar{C}_{ih} \right]$$

represents the BLS categories or combination of categories that serve as the bases of allocation for the corresponding categories of state revenues and expenditures. The values of both (\bar{C}_h) and (\bar{Y}_i) were taken from the same BLS study used by the Tax Foundation and Fry and Winters.²²

²⁰ See Arthur S. Goldberger, *Econometric Theory*, John Wiley, New York, 1964, pp. 197-198. A note of caution is in order here. The fact that beta coefficients are themselves statistics is often overlooked by researchers. Before any meaning can be attributed to a beta coefficient, one must test whether it is significantly different from zero. Likewise, in comparing the magnitudes of beta coefficients, one should test for a statistical significance difference between the two beta coefficients. See Teh-wei Hu, "Beta Coefficients Used in Statistical Inference," unpublished paper (The Pennsylvania State University, 1971).

²¹ Note also that indications of statistical significances are given for the regression coefficients, making possible an evaluation of the reliability of these estimates.

²² The values of (C_h) for the category of elementary and secondary education were not included in this formula. The basis of allocation for this category is the

Equation 2:

$$T_{ij} = (M_{ij}) (\bar{Y}_{ij})$$

T_{ij} = Total aggregate money income before taxes for all families and unattached individuals

j = State, j from 1 to 48

M_{ij} = Number of families and unattached individuals

\bar{Y}_{ij} = Average money income before taxes

The values of (M_{ij}) were taken from the 1960 census.²³

Equation 3:

$$A_{ijh} = (P_{ih}) (T_{ij})$$

A_{ijh} = Average dollar amount of expenditures (income)

P_{ih} = Percentage of money income before taxes spent (received)

T_{ij} = Total aggregate money income before taxes for all families and unattached individuals

This step recalculates the values of (C_{ih}) for each individual state. The values of (A_{ijh}) take into account the difference in the distribution of families and unattached individuals, by income class, from state to state.

Equation 4:

$$a_{ijh} = \frac{A_{ijh}}{\sum_{i=1}^9 A_{ijh}}$$

a_{ijh} = Percentage of expenditures (income) spent (received) by each income class in relation to the total amount spent by all nine income classes

A_{ijh} = Average dollar amount of expenditures (income)

number of children under 18 years of age for each family. The values of (P_{ih}) for this category were taken directly from the BLS study.

²³ U.S. Bureau of the Census, *U.S. Census of the Population: 1960, Vol. 1, Characteristics of the Population* (Washington, D.C.: U.S. Government Printing Office, 1963), pts. 2-9, 11-52. A problem arises here since the income classes used by the BLS and those used by the Bureau of the Census do not coincide in all cases. To solve this problem, those income classes between \$6,000 and \$9,999 were lumped into one class. To combine the BLS data, weighted averages were taken (according to the number of families and single consumers in the income class to be combined) to determine the average amounts of income and expenditures for the income class of \$6,000 to \$9,999. For the Census data the classes were simply added together.

The values of (a_{ijh}) represent the percentage burden (benefit) of state government revenues (expenditures) allocated to each income class.

Equation 5:

$$BE_{ijk} = (a_{ijh}) (SE_{jk})$$

$$BU_{ijl} = (a_{ijh}) (SR_{jl})$$

BE_{ijk} = Dollar amount of benefits from state expenditure program

BU_{ijl} = Dollar amount of burden from state revenue program

k = Category of state government expenditure program

l = Category of state government revenue program

SE_{jk} = Dollar amount of state government expenditures

SR_{jl} = Dollar amount of state government revenues

The dollar amounts of state expenditures (BE_{jk}) and state revenues (BU_{jl}) were taken from the *Compendium of State Government Finances in 1961*.²⁴

Equation 6:

$$be_{ij} = \sum_{k=1}^{12} BE_{ijk}$$

$$bu_{ij} = \sum_{l=1}^{12} BU_{ijl}$$

be_{ij} = Total dollar amount of benefits for all categories of state expenditures

bu_{ij} = Total dollar amount of burdens for all categories of state revenues

BE_{ijk} = Dollar amount of benefits from state expenditure program

BU_{ijl} = Dollar amount of burden from state revenue program

Equation 7:

$$r_j = \frac{\sum_{i=1}^4 be_{ij}}{\sum_{i=1}^4 bu_{ij}}$$

r_j = The ratio of redistribution for the lowest four income classes

be_{ij} = Total dollar amount of benefits for all categories of state expenditures

bu_{ij} = Total dollar amount of burdens for all categories of state revenues

²⁴ U.S. Bureau of the Census (Washington, D.C.: U.S. Government Printing Office, 1962).

Voting Systems, Honest Preferences and Pareto Optimality*

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"In a capitalist democracy there are essentially two methods by which social choices can be made: voting, typically used to make 'political' decisions, and the market mechanism, typically used to make 'economic' decisions."¹ To economists, the freely functioning competitive market mechanism has alluring structural properties.² If each individual follows his own self-interest in making his market decisions, an efficient outcome is achieved.

What of the possibility of constructing a voting mechanism that would achieve a collective political outcome that would have desirable properties that parallel those of the competitive market outcome? Can a voting system be impersonal and decentralized, allow each individual to act on his own behalf, and guarantee that a Pareto-optimal outcome will be achieved?³

* Kenneth Arrow, Robert Klitgaard, Pamela Memishian, Howard Raiffa, Thomas Schelling, Michael Spence, Milton Weinstein, and a referee for the *American Political Science Review* provided me with helpful comments. This research was supported by NSF grants GS-28626x and Gr-58.

¹ Kenneth Arrow, *Social Choice and Individual Values* (New York: John Wiley and Sons, Inc., 1964), p. 1.

² It is with some discomfort that I leave aside the problem of distribution in this paper. The issue is attacked directly in a later effort, "Risk Spreading and Distribution," Kennedy School of Government, Discussion Paper No. 10 (August, 1972). There I argue that a profound belief in the efficacy of the outcome of a perfect market gives one insights into distributional questions. One can consider the problem of drawing the social contract equivalent to that of a group of future citizens starting in some initial position of equal potential (they face the same lottery on future possibilities). They draw up a contingent claims agreement, the magnitudes of payment to depend on their future fortune in securing endowments of goods and capabilities (that is the states of the world). In retrospect, we may decide that it is our duty to redistribute in the manner that would be prescribed by the hypothetical agreement that antedated our present position. For a philosophically compatible, but perhaps policy inconsistent view, see John Rawls, *The Theory of Justice* (Cambridge, Mass., Harvard University Press, 1971).

³ Thomas Schelling has commented at great length, interpreting and questioning the significance of Pareto optimality as a criterion for social choices. He states, "the significance of failure to achieve Pareto-optimality merely means that the situation is improvable . . . it is the degree of improvability that determines how important the nonoptimality is. Pareto optimality is not like virginity or justice. . . . Small departures are of small interest, large departures are of large interest." Personal communication, July 12, 1972.

Economists have long recognized that Pareto optimality by itself is a hopelessly incomplete guide to normative and descriptive considerations of social choice. An indication of this author's response is given by the conclusion to this paper.

A Desirable Voting Scheme

In the market, an individual indicates his preferences through his purchases and sales. In general, these tracings are insufficient to define an individual's complete preference mapping, but from the standpoint of efficiency this creates no difficulties. With merely the limited amounts of information fed to it, the market leads to a Pareto-optimal outcome. (It should be noted that no individual has an incentive to disguise his preferences.)

A voting system in theory could operate in the same decentralized fashion. Each individual would feed in some indication of his preferences, and the processing mechanism would employ this information to yield an outcome. To see whether such a procedure should be established as an ideal, we must determine how it can perform along a number of significant dimensions.

Major Characteristics of Social Decision Procedures

The flow diagram below provides the context for the major topics discussed in this paper. (At a later juncture, it will serve to summarize its principal results.) These topics are indexed A through F.

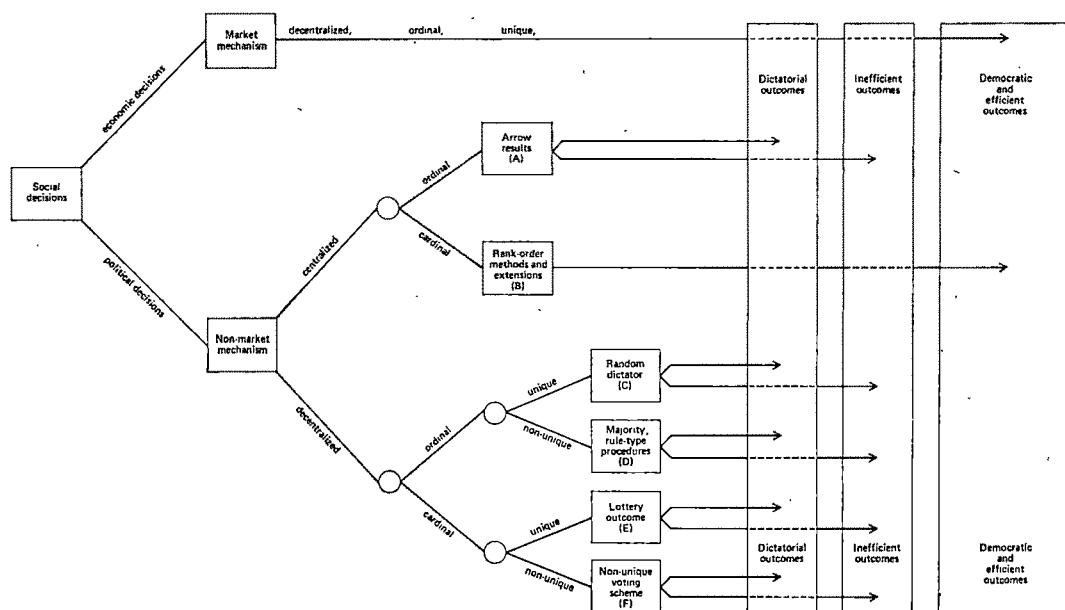
Social decision problems are first classed as economic or political. Economic decisions are handled by the market mechanism. Under the appropriate competitive conditions, the market outcome will meet some preassigned standards of acceptability. The principal standard to be met is efficiency. The measuring rod for this standard is Pareto optimality.

There are myriad situations in which the very restrictive competitive assumptions are not met. For these situations, much the subject of current economics debate, the unhindered play of the market will not lead to an attractive outcome. Perhaps the establishment of new areas of property rights, or innovative additions to present market mechanisms will set things right. Whatever, this is not material to the present discussion.

These situations would be of import for this paper if they were treated as political decisions. They then would be carried down the lower branch of the flow diagram, where they could serve as further exemplars for the analysis of that branch.

Political Decisions and the Individualist Approach

In evaluating outcomes in the economic arena,



Context and Summary Diagram

we rely on consumer sovereignty. Each individual is entitled to judge what is best for himself, and these individual judgments form the basis for assessments of social welfare. This paper carries over this individualist ethic to the evaluation of all social choices, including those that are conventionally considered to be of a political nature. Thus individuals' preferences form the basis for the social decisions considered here. In social decisions more than one individual is involved. Together, social decision and consumer sovereignty exclude from consideration any form of paternalistic rule.

The ground rules then are set. We are searching for a procedure that enables us to proceed from information on individuals' preferences to a social outcome that meets some conditions of acceptability. The ultimate objective is to discover a decentralized procedure, one which consciously or automatically takes the (perhaps incomplete) information provided by individuals' ballots and yields an acceptable social outcome.

Centralized Decision Procedures

For purposes of comparison, I shall begin with a look at the success or nonsuccess of centralized procedures. With centralized decision, a single decision authority with accurate knowledge of individuals' preferences selects an outcome.

Centralized Decision, Ordinal Preferences (A)

At first glance it would seem that centralized decision procedures hold out promise of great

success. Yet Kenneth Arrow, in a much celebrated work, has demonstrated that there is no general procedure to pass from individuals' preferences to social orderings that meets certain standards of attractiveness.⁴ The power of the Arrow result derives from the fact that the standards he imposed were both innocuous in appearance and intuitively appealing.

Arrow sets forth four standards. In unrigorous language, they are: (1) The procedure must include all logically possible combinations of individuals' orderings. (2) It must lead to Pareto-optimal outcomes. (3) The choice between any two alternatives cannot be influenced by the presence or nonpresence of a third alternative. (4) No individual can always secure his choice regardless of the preferences of others. A later expositor has provided a mnemonic for these properties:⁵ unrestricted domain, U; Pareto principle, P; independence of irrelevant alternatives, I; nondictatorship, D. Dealing with the simplest case of interest (at least two individuals and at least three alternatives), the famous Arrow result is that there is no social welfare function that satisfies U, P, I, and D.

Let us stop for a moment to get our bearings in the flow chart. We should like all of our social decisions to apply over an unrestricted domain. Condition U is imposed at all nodes in the flow diagram and throughout this paper. Next observe

⁴ For the conditions, see K. Arrow, *Social Choice*, pp. 22-33.

⁵ Amartya K. Sen, *Collective Choice and Social Welfare* (Holden-Day, Inc., San Francisco), pp. 41-42.

that the "Arrow result" box lies on the ordinal branch. The independence of irrelevant alternatives condition, I, requires that the centralized decision procedure rely on no more than individuals' ordinal preferences. The impossibility of simultaneously satisfying all four Arrow conditions is indicated by the paths of the arrows emanating from the box. They are stopped either by inefficiency (nonsatisfaction of P) or dictatorship (nonsatisfaction of D).

Centralized Decision, Cardinal Preferences (B)

Somewhat different formulations of the centralized social choice problem will lead to success stories. A number of analysts, some of whom substantially predate Arrow, have proposed that acceptable centralized decision procedures can relax one or another of the conditions he imposed. Perhaps the most frequently cited suggestion is that the independence of irrelevant alternatives condition, I, be softened or eliminated. The objective underlying this relaxation is to secure some cardinal measurement on individuals' intensities of preferences.

By way of example, more than two centuries ago, Borda proposed the rank-order method of election, a method now frequently employed to establish national rankings of college athletic teams. Under this system, preassigned weights are given to voters' first, second, third choices, etc. Sum each candidate's scores over all voters: highest aggregate score wins. The procedure is neither dictatorial nor inefficient.⁶ Reference to the flow diagram shows that its emanating arrow penetrates to the rightmost rectangle.

Are rank-order systems ethically appealing? Neither economic theory nor any other axiomatic system can give us any insight into this question. Each analyst of social choice must decide for himself whether in making the choice between A and B, it is of concern how each fares with the voters not only with respect to the other, but also with respect to C.

We need dwell no further on the appropriate constraints for centralized procedures; decentralized procedures represent the principal focus of

this paper. Thus far we have been merely setting the stage and filling in the complementary boxes.

Decentralized Decision Procedures

The remainder of this paper will follow out the implications of and possible standards of performance for a variety of decentralized decision procedures. It will index them, as it did the centralized procedures, by the capital letters in the flow diagram.

The analysis that follows is largely nonmathematical and thus almost by necessity, unrigorous. Other theorists, no doubt, will be able to reconstitute these results in more formal, perhaps stronger contexts. The purpose of this paper is to sketch some new methods and results, and the framework in which they apply.

With centralized decision, each voter's preferences are assumed to be known by the social choice mechanism. With decentralized decision, knowledge of preferences is gained only indirectly. Under decentralized procedures each voter fills out a ballot; he is assumed to follow as best he can his own self-interest. These ballots are processed by some mechanism to produce a social choice. Depending upon the particular choice mechanism that is in operation, and the situation in question, a voter's ballot may or may not be a complete and truthful indicator of his preferences. This observation provides a hint about where we are going. A whole new class of problems arises when we move from centralized to decentralized decision procedures. Essentially they revolve around getting individuals to reveal their preferences. The unfortunate result, as we shall see later, is that the very structure of social choice schemes that elicit true preferences on ballots leads to inefficiencies in outcomes.

An abstract representation of a decentralized choice procedure will facilitate discussion. There are

m voting individuals indexed over i , and
 n alternatives indexed over j .

An individual, i , fills out x_i as the ballot indicating his preferences. The voting system processes the received ballots, $X = (x_1, \dots, x_i, \dots, x_m)$ to arrive at a social choice. A social choice is represented by the probability vector $P(X) = (p_1(X), \dots, p_j(X), \dots, p_n(X))$ whose elements indicate the probability that an alternative is selected. In the language of mathematics, a voting mechanism maps X into P .

Most of the familiar voting procedures are deterministic. Given X , a single alternative is selected with certainty; one element of P is unity, the rest zero.

Later sections of this paper discuss voting pro-

⁶ Following Pareto optimality, efficiency requires that a switch away from the selected candidate would lower the welfare of at least one voting individual. If the selected candidate had the highest total score, then there could be no other candidate who received an equally high individual score from each voter. A switch from the man with the highest total would therefore involve a loss for at least one voter.

⁷ Special difficulties arise if lotteries on alternatives are permitted; and rank-order schemes need not lead to Pareto-optimal outcomes. See Richard Zeckhauser, "Majority Rule with Lotteries on Alternatives," *The Quarterly Journal of Economics*, 83 (November, 1969), 696-703.

cedures that may lead to randomized outcomes. These outcomes are analyzed and evaluated before the final results are known, before the lottery is conducted. Randomized outcomes may lead to gains in efficiency. More important for the purposes of this paper, they may help insure that individuals mark their ballots with their true preferences.

To evaluate probabilistic outcomes, a voter must be able to evaluate outcomes in a non-deterministic world. Even if the world is deterministic, there may not be full information, in which case the voter may be making a decision under uncertainty. Fortunately, there is a well-developed body of literature on prescriptive decision making under uncertainty. We shall follow the major track of that literature.⁷

A rational decision maker under uncertainty is assumed to assign a utility value to each possible outcome. The paradigm decision problem requires that he select one among alternative lotteries where each lottery is described by the probability vector it offers for the different outcomes. In choosing among lotteries, the rational decision maker selects the one that offers him the highest expected utility value.

Notice that judgments of welfare are made here on an ex ante basis, before lotteries are resolved. Thus certain social choices will be ruled non-Pareto optimal, even though they will eventually lead to situations that considered in isolation are Pareto optimal. An example displays the properties of this before-the-fact evaluation procedure. Two risk-averse individuals each have \$10,000 in the bank. They are given the opportunity to gamble their total savings on the flip of a coin. Both would decline as they both prefer their present situation to participation in the lottery. If they were forced to participate in the lottery, it would be a non-Pareto-optimal situation. This despite the fact that if the lottery were conducted and one man ended up with \$20,000 and the other with nothing the outcome would be Pareto-optimal.⁸

⁷ For a good introduction to this subject, see Howard Raiffa, *Decision Analysis* (Reading, Mass.: Addison-Wesley, 1968).

⁸ The ex-ante expected utility approach can be employed to make evaluations of social welfare. See Richard Zeckhauser, "Determining the Qualities of a Public Good—A Paradigm on Town Park Location," *Western Economic Journal*, 11 (March, 1973), 39–60. For an application of this principle in the social decision context, see Richard Zeckhauser, "Majority Rule With Lotteries on Alternatives." Peter Fishburn formalizes this argument in "Lotteries and Social Choices," *Journal of Economic Theory*, 5 (October, 1972), 189–207. Peter C. Ordeshook employs the principle to define candidates' mixed strategies as lotteries on social states should they get elected. See "Pareto Optimality in Electoral Competition," *The American Political Science Review*, 65 (December, 1971), 1141–1145.

To conduct an analysis of situations of this sort requires that individuals define what are called von Neumann-Morgenstern utilities for each possible outcome.⁹ The utility values for individual i are indicated by the v_i^j 's; they are normalized with a value of 1 assigned to the most preferred outcome, and 0 to the least.

The voting mechanism, however, may seek to elicit something much less than the individual's full cardinal preference structure. Majority rule procedures, for example, merely ask the individual which is his most preferred alternative.

It is useful to classify decentralized decision procedures on a couple of dimensions. Decentralized procedures, like those that are centralized, can rely on cardinal or merely ordinal indications of individual's preferences. The two alternatives are indicated in the first branching in the flow diagram.

Unique Voting Schemes

The second branching reflects a consideration that does not arise with centralized procedures; for with them, individuals' preferences are assumed to be known. With decentralized procedures, however, each individual ballots in his own self-interest. This raises the possibility that with some of these procedures and some situations an individual working in his own behalf will find it desirable to disguise his preferences. This may be unfortunate. It may be important for the apparatus that is processing the votes to be able to determine the exact preferences of an individual from his markings on the ballot.

If an individual will mark his ballot the same way for two or more preference structures, then the ballot is not unique.

Definition: A voting system is called *unique* if a voting individual will mark his ballot in a different manner for every possible configuration of his preferences.

Decentralized Decision, Ordinal Preferences, Unique (C)

If merely ordinal preferences are required, it would seem the task of constructing a unique scheme would be somewhat simpler. In many real world situations things are simpler still. Individuals are asked to indicate only a small portion of their ordinal preference structure, their first-place choice. This is the customary procedure in the United States when we are selecting a single individual for an office: the presidency, a mayorship, the local dog catcher.

⁹ Appropriate historical attribution would give much of the credit for this utility concept to Frank P. Ramsey.

A traditional majority-rule decision procedure will lead to honest revelation of preferences if there are but two alternatives. But when the competitors number three or more such honesty can no longer be assured. For example, if in a three-man race your preferred candidate *A* is likely to receive little support, you may choose to vote for your second choice rather than waste your vote on a lost cause. If your ordering is *A-B-C*, you will vote for *B*. You would vote the same way if your ordering were *B-A-C* or *B-C-A*. The voting procedure is not unique, but this should have been expected. There are six possible orderings and only three possible ballot markings. There is no way to have a unique marking for each ordering. What is disturbing is that the system is not unique with respect to the first-place preference, and it is information on that preference that the system was attempting to solicit. Considerations such as these lead us to a modified concept of uniqueness, one that applies in a more restricted context.

Definition: A voting system is called *unique with respect to a property of a preference ordering* if a voting individual will mark his ballot in a different manner for every possible configuration of that property.

For example, a voting system is unique with respect to first-place preferences if a voting individual will mark his ballot differently depending upon which candidate he likes best.¹⁰ Majority rule does not qualify.

Does a voting system exist that is unique with respect to first-place preferences? Yes, there is a type of system that meets this requirement. It is most easily understood with the aid of a mechanical example that has the required intrinsic properties.

The Random Dictator System: A Voting System That is Unique with Respect to First-Place Preferences

Each individual writes the name of a candidate on a ballot. The voters' ballots are collected and placed in a revolving drum. After shuffling, a ballot is chosen at random. The name on the chosen ballot is the elected candidate. In effect, the voter whose ballot is chosen dictates the outcome, hence the name of the system. (Matters would be just the same if a voter were first selected at random, and then the selected voter were asked to choose the elected candidate.)

The random dictator system is not determinis-

tic. If an alternative *j* is marked as first choice on *k* ballots, p_j will equal k/m . That is, a candidate will have a probability of election that is proportional to the number of first place votes he receives. The p_j 's will take on fractional values (except in the special case where there is a unanimous first choice); the system will not be deterministic.

We shall see here, and again later with cardinal preferences, that the consideration of probabilistic outcomes is essential if a voting scheme is to be unique. Consider the voter's behavior in the random dictator model. He assigns utility values v_A , v_B , and v_C to the three candidates. Why should he vote for his favorite, *A*? Assume that instead he had voted for *B*. This would reduce the probability that *A* gets elected by $1/m$; it would increase the probability that *B* got elected by $1/m$. The net change in the expected utility to the voter would be $(1/m)(v_B - v_A)$. By assumption, $v_B < v_A$; the voter would lose by switching; he also would lose if he decided to choose *C*. The key element here is that when the individual switches his vote the probability gain for the switched-up candidate is just equal to the probability loss for the switched-down candidate. This implies that the probability of election of the third candidate remains constant when a vote is switched between the other two. This absence of third-candidate effect is essential. Otherwise, the decision on whether to vote for *A* or *B* might depend on how the vote affects the probability that *C* gets elected.¹¹ A formal generalization of this argument requires a new concept.

Definition: A voting system will be said to be *probabilistically linear* if it is of the following form. Let *S* be the matrix of votes, where element $s_{ij} = 1$ if voter *i* submitted his ballot for *j*, and 0 otherwise. The vector *P* that gives the probability that each candidate is elected is calculated $P = wS + e$. Here *e* is a row vector giving exogenous contributions to the election probabilities, and *w* is a row vector of non-negative weights summing to $1 - \sum_j e_j$.

Theorem I: If a decision procedure that solicits individuals' first place preferences is to be unique, it must be probabilistically linear.

Note that probabilistic linearity is equivalent to the following property: For any voter *i* and for any pair of candidates *D* and *E*, switching his vote from *D* to *E* must increase the probability that candidate *E* gets elected by the amount w_i , de-

¹⁰ If we extend this concept to the market setting, we discover that market procedures are unique with respect to the information required for efficiency: individuals' marginal rates of substitution and producers' marginal rates of transformation among valued goods.

¹¹ Say v_A is only slightly greater than v_B , but much greater than v_C . If *C* had less probability of being elected if the individual voted for *B* rather than *A*, it might be reasonable for him to vote contrary to his true preferences.

crease the probability that D gets elected by the amount w_i , and leave all other election probabilities unchanged.

The proof of the theorem can be demonstrated with a three-man example. For notation, let $\Delta_{D \text{ to } E}^F$ represent the change in the probability that F is elected if voter i switches his vote from D to E . Consider the previous example where the present vote is for A . Necessary conditions for probabilistic linearity are that

$$\Delta_{A \text{ to } B}^A = -\Delta_{A \text{ to } B}^B = \Delta_{A \text{ to } C}^A = -\Delta_{A \text{ to } C}^C$$

and that

$$\Delta_{A \text{ to } B}^C = \Delta_{A \text{ to } C}^B = 0.$$

Given that the sum of the mutually exclusive probabilities of election must add to 1 as they did when the vote went to A , there are the conservation equations

$$\Delta_{A \text{ to } B}^A + \Delta_{A \text{ to } B}^B + \Delta_{A \text{ to } B}^C = 0$$

and

$$\Delta_{A \text{ to } C}^A + \Delta_{A \text{ to } C}^B + \Delta_{A \text{ to } C}^C = 0.$$

To see that $\Delta_{A \text{ to } B}^C$ must equal 0, assume it were negative. The voting system must handle any set of preferences. Thus, let the voter strongly dislike C , say $v_A - v_C = k$, where k is a large number, and let him slightly prefer A to B , $v_A - v_B = \epsilon$. Despite this preference, the voter will switch his ballot to B , as his net utility gain will be $-\Delta_{A \text{ to } B}^C(k - \epsilon)$, a positive number. Keep the same ordinal preferences, but let the gap between v_A and v_B be k , with the gap between B and C being the minimal ϵ . Then the individual will choose A over B , the utility gain once again being $-\Delta_{A \text{ to } B}^C(k - \epsilon)$. Thus, unless $\Delta_{A \text{ to } B}^C = 0$, the voter may ballot in two different ways, despite the fact that his ordinal preferences are the same in both instances. This contradicts the assumption that the voting system is unique. Therefore $\Delta_{A \text{ to } B}^C = 0$ as claimed.

An identical argument shows that $\Delta_{A \text{ to } C}^B = 0$. These results together with the conservation equations show that $\Delta_{A \text{ to } B}^A = -\Delta_{A \text{ to } B}^B$ and $\Delta_{A \text{ to } C}^A = -\Delta_{A \text{ to } C}^C$. To show that $\Delta_{A \text{ to } B}^A$ must equal $\Delta_{A \text{ to } C}^A$, merely apply the argument above to show that whether you ballot for B or C cannot influence the probability of election for A if a voting system is to be unique with respect to ordinal preferences. The conditions of the theorem are thus demonstrated.

This theorem and the random dictator system can be extended to deal with further places in voters' ordinal rankings. Provide each voter q ballots for his first choice, r for his second, s for his

third, etc., with $q > r > s$. The selection procedure is random as before.

Thus we find that only variants of the random dictator system will elicit ballots unique with respect to individuals' (portions of individuals') ordinal preferences. How well does the random dictator system perform? An example tells the sad news.

With the values shown in the box, Voter 1 will mark his ballot with A and Voter 2 will inscribe a C .

Utility Valuations in Two Voter World

	v_A	v_B	v_C
Voter 1	1	.90	0
Voter 2	0	.90	1

The resulting social choice when the random dictator system is employed will have the probability vector $P = (.5, 0, .5)$. The expected utility values for each of the voters will be .50. But this outcome is not Pareto optimal. Both voters would have preferred the social choice $P = (0, 1, 0)$.

The difficulty is apparent. The random dictator system does not take into account intensities of preference. (Indeed, no information is gathered on second and further preferences, but even if it were, this would not cover the intensities problem. With the same ordinal patterns, both voters could have assigned a value of .10 to v_B . In that case the social choice of the random dictator device would be Pareto-optimal, as would be any lottery on A and C .)¹²

To sum up, it is interesting to observe that there exists a class of unique ordinal voting schemes.¹³

¹² For further discussion of this matter, see R. Zeckhauser, "Majority Rule with Lotteries on Alternatives."

¹³ This paper is addressed to situations where a single outcome is selected, and all individuals vote to help determine that outcome. The model does not apply, for example, to the selection of a legislature. (With a stretch of the imagination, we might construe voters to be choosing among all possible legislatures, but usually an individual voter ballots to help determine only a small part of that body.) Strict proportional representation is the closest (but still imperfect) legislative equivalent to the random dictator system. The number of seats secured is proportional to the number of votes received.

Proportional representation will guarantee honest balloting if two conditions are satisfied. (1) Each voter's utility for the legislature must be an additive separable function of his utilities for each elected representative. (Otherwise, his preferred vote might depend on the candidates others were voting for.) (2) All ballots cast in the first round must count in the determination of the final legislature. This requires that representation be broken down into arbitrarily small units, and that there be no elimination of candidates. (Condition (2) can be violated if there are strict party interests, and if each voter is indifferent among the candidates of his party.) See Mark Thompson and Richard Zeckhauser, "Proportional Representation," mimeo (1971). A referee suggests that Kramer also has unpublished work on this subject.

We now know that such schemes perform terribly inefficiently. Leave aside the adverb, and this lesson could have been derived directly from the Arrow results.

Decentralized Decision, Ordinal Preferences, Nonunique (D)

This next class of social choice procedures merits discussion primarily because it is the one most frequently in use in the United States. Majority and plurality rule both fall under this category. The Arrow results tell us that these schemes cannot meet his four standards either.

One difficulty with these procedures, as is well known, is that they lead to intransitive choice patterns for certain combinations of individuals' preference orderings. Transitivity is required if we are to have a social ordering. The only way around the problem is to rule out patterns of individuals' preferences that do not satisfy the single-peakedness condition.¹⁴ To exclude in this way would be to violate Arrow's condition *U*.

Now that we are working on a decentralized basis, further problems may arise. The vote processing mechanism may not even be able to divine individuals' true preference orderings. It turns out, fortuitously, that single-peakedness is also sufficient to eliminate any incentive for a voter to disguise his preferences. Getting rid of one problem automatically eliminates the other. Whenever the structure of voters' preferences is such that majority rule can be guaranteed to work on a centralized basis, it can be guaranteed to work in a decentralized format as well.

Standards for Decentralized Schemes

One lesson we have learned thus far is that ordinal preferences do not provide sufficient information to enable social choices to meet Arrow's standards. For centralized procedures, we saw that information on cardinal preferences is sufficient to guarantee an efficient, nondictatorial outcome. The key question for the remainder of this paper is whether we can find decentralized procedures that perform satisfactorily when the balloting system elicits cardinal indications of the voters' preferences.

Satisfactoriness, like beauty, is in the eyes of the beholder. My criteria, consistent with the earlier analysis, are Pareto optimality and nondictatorship. The first concept is unambiguous; the latter is subject to a variety of interpretations. I shall provide one definition of dictatorial outcomes

that enables me to achieve my other results. The reader may come up with other definitions that serve equally well.

Dictatorial Outcomes—What They Are and Why They Should Bother Us

Most discussions of social choice are concerned with the selection of a candidate in a single election, or an outcome in a particular set of circumstances. Given the restricted domain of such a social decision, it might be perfectly acceptable to reach an outcome that catered heavily to one individual's or group's preferences and little or not at all to those of others. (Surely, this provides the normative justification for such practices as log-rolling.)

Other social decisions can be of much greater consequence. Indeed at the most general level, we can conceive of a grand social decision as selecting a single point from the entire space of social states. It is with reference to momentous social decisions of this sort that we impose distributional requirements on the outcomes of social choice processes. We should like to observe outcomes that demonstrate that the preferences of more than one individual have been taken into account.

Even if a series of social choices is made sequentially or in a variety of separate arenas, we might conceive of the combination of the voters' expressed preferences on all issues and the social choice mechanism as selecting one point in the space of social states. Thus even if a great number of social choices is to be made on a one by one basis, we should like to see the total social outcome reflect the fact that all individuals have participated in the selection process.

One indication that a group had been excluded from positive consideration in the social choice procedure would be that it selected an outcome that was most unattractive for that group. A more precise understanding of what is meant by unattractive is given by the term Pareto-terrible.

Definition: An outcome is called *Pareto-terrible* for a group of individuals if it is a strictly constrained minimum: if there exists an outcome that is preferred by all members of the group, and if there is no outcome that is worse for some member without being better for some other member.

An outcome is Pareto-terrible for a one-man group, an individual, if it is his least preferred outcome. With lotteries permitted there will always be outcomes that are not Pareto-terrible for any reference group.

I shall employ the Pareto-terrible concept as a cornerstone of my definition of dictatorial outcomes.

¹⁴ See A. Sen, *Collective Choice*, pp. 166–168 for a discussion of single-peakedness where individuals' preferences are defined over a single dimension. There is no equivalent to the single-peakedness criterion if two or more dimensions enter individuals' preferences; cyclical majorities will be the rule.

Definition: A voting scheme is *dictatorial* for a set of voting individuals preferences if it selects an outcome that is Pareto-optimal for one nonempty subset of individuals and is Pareto-terrible for the complementary nonempty subset of individuals.¹⁵

With this definition in tow, we can proceed on our search. Exposition is simplest if we step slightly out of order to the bottom of the Context and Summary Diagram and address first the question of honest balloting.

Decentralized Decision, Cardinal, Nonunique (F)

Nondictatorial, Pareto-optimal social choices cannot be made unless, in general, the voting scheme is unique with respect to individuals' cardinal preferences. In particular instances non-uniqueness may not matter. For every nonunique ballot marking for individual 1, however, there will be a set of preferences for individual 2 such that Pareto optimality cannot be assured.

A three-candidate example will reveal the difficulties. Assume that individual 1 marks his ballot x' whether his preferences are (1, .60, 0) or (1, .70, 0) for candidates A , B , and C respectively. Individual 2 has preferences (0, .35, 1). If individual 1's FIRST set of preferences hold, candidate B will have zero probability for all Pareto-optimal choices. With individual 1's SECOND preferences, all Pareto-optimal probability vectors have either A or C receiving zero probability.

tween two situations which require different outcomes to be Pareto-optimal.

The 1, 0, 0 and 0, 0, 1 vectors are ruled out because they are dictatorial. They allow one voter to get his first choice at the expense of the other who receives the outcome that for him is worst. This would be particularly objectionable if, as is quite possible, the voting scheme were selecting a single point from the space of social states, i.e., if all social decisions were being simultaneously resolved.¹⁶

To achieve Pareto optimality, any voting scheme that is nondictatorial for this set of preferences must be able to distinguish between FIRST and SECOND configurations of individual 1's preferences. An equivalent "counterexample" demonstration can be arranged for any indistinguishable pair of individual 1's preferences. The three-alternative case covers all essential complexities.

First consider the case where individual 1's indistinguishable configurations have different alternatives as the most preferred. Let individual 2 be indifferent between the two sometimes-most-preferred-by-1 alternatives, with the third alternative less attractive. Which of two alternatives is Pareto-optimal depends on which of individual 1's preference configurations applies; there is no way to tell.

Matters are a bit more complicated if individual 1's indistinguishable orderings rearrange his second and third candidates or merely change their

Pareto Optimality of Different Probability Vectors

Configuration of Individual 1's Preferences	Probability Vectors ($A+$ indicates positive probability)						
	1, 0, 0	0, 1, 0	0, 0, 1	+, +, +	+, +, 0	+, 0, +	0, +, +
FIRST	yes	no	yes	no	no	yes	no
SECOND	yes	yes	yes	no	yes	no	yes

Except for the first and third listed vectors, no probability vectors are Pareto-optimal for both of individual 1's possible preference configurations. The problem is simple. The mechanism that processes the votes is unable to distinguish be-

relative scores. To construct the counterexample first normalize the two individuals' utilities to 1 and 0. Add together 1's and 2's utilities for each of the three candidates. The candidate with the lowest total must receive zero probability in any nondictatorial Pareto-optimal outcome. (If the

¹⁵ The concluding theorem of the paper employs the dictatorship concept negatively. It states that no system can simultaneously be nondictatorial and at the same time guarantee that other desirable standards are met. The more restricted the definition of dictatorship, then, the stronger my final result. This result would still be achievable if the term "Pareto-optimal" in the above definition were replaced with "unanimously most preferred." The argument for the definition in the text is that it captures more of what we may mean by dictatorship in a general context.

It should be noted that Pareto-terrible is not a perfect counterpart to Pareto-optimal. There are situations where all outcomes are Pareto-optimal and none Pareto-terrible. The converse is not true.

¹⁶ On a less grandiose level, there are a number of ways that single social-choice decisions can be formulated to deal with a large number of issues. Issues can be compounded. The issues at hand might be whether Smith or Jones should be mayor and whether or not the new high school should be built. This can be viewed as a single decision with four possible outcomes. Alternatively, we might amalgamate issues by employing a lottery. A coin will be flipped to see whether a voting mechanism will select a mayor or decide on a high school. Here too there are four possible outcomes, each composed of two contingent decisions.

two voters have opposite orderings, then the extreme candidates may tie for lowest total. Then, at least one of these candidates will be excluded from any Pareto-optimal outcome.) Given the two different utility vectors for 1, select a utility vector for 2 so that (a) his first place choice is not the same as that of 1, and (b) the candidate receiving the lowest total will differ depending on which of 1's vectors applies. Such a selection will always be possible.¹⁷

It is true, of course, that for certain patterns of preference for 2, particular instances of non-uniqueness for 1 will not matter. This raises the question whether a voting individual can monitor other individuals' preferences or ballot markings to help select his own ballot marking. Such monitoring raises no problems so long as each individual's markings are unique, conditional on the ballot markings of others. For with this information, all individuals' accurate cardinal preferences can be deduced.

A definitional problem arises with the concept of uniqueness if voters monitor each others' ballots and only employ the same marking for two different preference orderings if they discover that it will not affect the set of Pareto-optimal outcomes. Notice that this monitoring would have to be most accurate, for as long as there is any positive probability that the other individual(s) have a "counterexample" set of preferences, Pareto optimality cannot be assured. In discussing uniqueness for the remainder of this paper, I shall assume either (a) that the required accurate monitoring is not possible, or (b) if it is possible, an individual who has an incentive to mark his ballot nonuniquely will sometimes do so in cases where other individuals have counterexample-relevant sets of preferences.¹⁸

This slightly tortuous argument leads to a general result that can serve as a building block for later arguments.

Theorem II: For nondictatorial voting schemes, if the ballot processing mechanism is always to lead to Pareto-optimal outcomes, the voting individuals' ballots must be unique with respect to their cardinal preferences.

¹⁷ Another numerical example may be helpful. Assume that 1 marks his ballot the same way whether his cardinal utility values are 1, 5, 0 or 1, 0, 2. Then if individual 2 has preferences 0, 1, 9, for example, a nondictatorial Pareto-optimal outcome cannot be assured. The third candidate can never be included for individual 1's first set of preferences and must always be included for his second set of preferences.

¹⁸ I strongly believe that it is possible to substantiate the arguments in the remainder of this paper without these assumptions. But the whole area is most elusive, and I have not been able to discover a convincing proof for this assertion.

Decentralized Decision, Cardinal, Unique (E)

To continue the thrust of our major investigation, we should like to know the properties of voting schemes that are unique with respect to cardinal preferences. In essence, we are asking: What voting schemes will get individuals to mark their true cardinal preferences? Unique schemes require one-to-one correspondences between ballot markings and preferences. If the mapping process is understood, we can say that in effect individuals are marking their true preferences.¹⁹

The required structural characteristics can be easily delineated in our three-candidate example. Before marking his ballot, the individual voter will consider his possible effects on the probability outcome vector. Matters are simplest if we start with the case of one individual.

What are the structural characteristics of a decision process that gets a single individual to reveal his true cardinal preferences? The individual is assumed to understand how the scheme works. The scheme is an announced function, s , that transforms the individual's ballot into a probability vector outcome. This process can be represented as

$$P = s(x).$$

The individual, knowing the form of s , will select x to maximize his expected utility. Representing his utility vector as V , and his optimal ballot as x^* , this can be expressed

$$\max_x s(x)V^T \text{ is achieved at } x^*.$$

We wish to discover the properties of the function s that will lead

$$x^* \equiv V.$$

That is, we wish to construct a function s such that it is in the individual's best interest to vote his true cardinal preference.

First, the function must obviously assign probabilities in the same rank order as the utilities that are provided through the x vector. (For simplicity,

¹⁹ It may be possible to construct voting schemes that are not unique for each individual taken alone, but are unique for all individuals taken together. This would require that individuals have some capability to monitor each others' preferences and ballot markings. For example, individual 1 might mark his ballot the same way when he has preferences V_1' and individual 2 marks X_2' as he does when he has preferences V_1'' and individual 2 marks X_2'' . The processing system can take 2's markings into account in deciphering individual 1's preferences.

If each individual's cardinal preferences are to be revealed, it is sufficient that *given other individuals' markings* there be a one-to-one correspondence between an individual's markings and his preferences.

we consider the case without ties.) Otherwise, the individual would have an incentive to misreport which alternative he liked best and which least.

With no loss of generality, we can normalize and assign a utility value of 1 to the most preferred alternative and 0 to the least. Assign the value y to the intermediate alternative; thus $V=(1, y, 0)$.

The key question then is, what additional properties must $s(x)$ have so that

$$\max_z s(1, z, 0) \begin{pmatrix} 1 \\ y \\ 0 \end{pmatrix} \text{ is achieved at } z = y$$

for all y , $0 < y < 1$.

The sum of the probabilities of the outcomes must equal 1. Thus, the function $s(x)$ is fully described by detailing the effect of the choice of z on p_1 and p_2 . Represent these relationships

$$p_1 = f(z), \text{ and} \\ p_2 = g(z).$$

The individual's expected utility is

$$1p_1 + yp_2 + 0(1 - p_1 - p_2).$$

He will select z to maximize

$$f(z) + yg(z);$$

the third term vanishes because of the zero multiplicand.

I will deal with the situation in which the f and g functions are continuous and differentiable; my results can easily be extended to noncontinuous cases. First-order maximization procedures applied to the preceding expression reveal that

$$f'(z) = -yg'(z).$$

If it is to be in the individual's self-interest to select $z=y$, a significant property of f and g is dictated.²⁰ For all z

$$f'/g' = -z. \quad [A]$$

Additional required properties are that for all z ,

$$f''(z) + zg''(z) < 0,$$

$$g(z) < f(z),$$

and

$$g(z) > 1 - f(z) - g(z). \quad [B]$$

²⁰ The uniqueness requirement is equally well satisfied so long as there is a one-to-one mapping between z and y . Any scheme that induces such a mapping has the key structural aspects that are discussed below in relation to this scheme.

The first requirement tumbles out of the second-order conditions that guarantee that the individual is at a maximum, not a minimum. The second and third requirements insure that the probability assigned to the highest value outcome is greatest, the second is second, and the lowest is lowest.

We should also like to have

$$f(1) = g(1),$$

$$g(0) = 1 - f(0) - g(0).$$

If alternatives are ranked the same, they should receive the same probability assessment.²¹

There is an infinite number of schemes that meet these requirements. One satisfactory scheme has the functions

$$p_1 = f(z) = .6 - .1z^2/2,$$

$$p_2 = g(z) = .25 + .1z,$$

and

$$p_3 = 1 - p_1 - p_2 = .15 + .1(z^2/2 - z).$$

Here $g'(z) = .1$; and to satisfy requirements

$$f'(z) = -zg'(z) = -.1z$$

for $z=y$. For all values of z , there must be the ordering $p_1 > p_2 > p_3$. This three-alternative scheme is illustrated in the figure below.

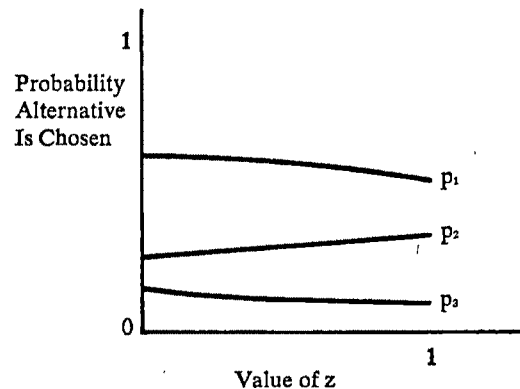


Figure 1. A scheme that leads an individual to express his true cardinal preferences.

	Value of z					
	0	.2	.4	.6	.8	1
p_1	.6	.598	.592	.582	.568	.55
p_2	.25	.27	.29	.31	.33	.35
p_3	.15	.132	.118	.107	.102	.10

²¹ I am indebted to Kenneth Arrow for this point among others.

The procedure just employed to construct a unique voting scheme for cardinal preferences can be readily extended to the many-alternative case. The computations get somewhat messy as the number of alternatives increases, but no new conceptual problems arise.²²

Pareto Optimality of the Unique with Respect to Cardinal Preferences Scheme

The scheme presented is unique with respect to the individual's cardinal preferences. Unfortunately, it is not Pareto-optimal. Pareto optimality would require that $p_1 = 1$. This misfortune, alas, is representative. With rare exceptions, schemes that are unique with respect to information required to make an efficient decision are by their very construction not Pareto-optimal.

The general difficulties that are encountered can be understood in the context of the simplest case where the difficulties matter, the case of two voters and three alternatives. (The one-voter case is not disturbing; Theorem II applies only to nondictatorial voting procedures. With one individual, dictatorship is acceptable; the cardinal preferences required by Theorem II are not needed to achieve a desirable outcome. Merely ask the individual which alternative he prefers and give it to him.)

Voting Schemes with Two Voters and Three Alternatives

With two or more individuals, as Theorem II tells us, efficient voting procedures that are democratic require cardinal preferences as inputs. The procedure must be unique with respect to individuals' cardinal preferences.

The insights we have derived from consideration of the one individual situation are readily extended. Look at the matter from the standpoint of voter 1. Assume that the vector giving voter 2's true cardinal preferences, γ , has been fed to the decision mechanism, and that voter 1 knows this γ .

The social outcome now is a function of both γ and x . Represent this relationship as

$$P = r(x, \gamma).$$

If honesty is to be insured, whenever

x^* is maximizer of $r(x, \gamma) V^T$, then

$$x^* \equiv V.$$

Paralleling our procedure from before, with z being the utility value of the in-between alternative, represent the function r fully by

²² To deal with discontinuous cases, merely replace $(f[x + \Delta x] - f[x])/\Delta x$ wherever $f'(x)$ appears, and similarly for $g'(x)$. Second derivatives can be handled in parallel fashion.

$$p_1 = j(z, \gamma),$$

and

$$p_2 = k(z, \gamma).$$

The efficiency condition that parallels [A] is for all z ,

$$j_1(z, \gamma) = -zk_1(z, \gamma).^{23} \quad [C]$$

There are no simply-stated conditions that insure the individual will write down his preferences in their true order. What is needed boils down to the tautological requirement that for all γ , the x that is the maximizer of

$$p_1 + \lambda p_2, \quad 0 \leq \lambda \leq 1$$

is of the form $(1, z, 0)$, where z too is a value between 0 and 1.²⁴

Consider a scheme that meets all requirements and is unique with respect to the individual's cardinal preferences. For such a scheme an individual with preferences $(1, y, 0)$ will select $z = y$ for all possible values of y . From condition [C], given that z is less than 1, the increase in p_2 as z increases is greater than the corresponding decrease in p_1 . The slack can only be taken up in p_3 , ($p_1 + p_2 + p_3 = 1$), which therefore must be decreasing over the whole range of z . This implies that p_3 must have positive probability (except in the case when the individual is indifferent between his first and second alternatives).²⁵ This observation suggests another general result.

Theorem III: A voting scheme that is unique with respect to individuals' cardinal preferences must lead to probability vector outcomes all of whose elements are positive.

²³ This analysis could be carried out equally well for a world where other individuals' ballot markings were uncertain parameters. A subjective probability distribution $f(\gamma)$ would be placed over that parameter. Thus, efficiency condition [C] would become

$$\int j_1(z, \gamma) f(\gamma) d\gamma = -z \int k_1(z, \gamma) f(\gamma) d\gamma.$$

Other conditions would be modified equivalently; the basic results would be the same. Where there is perfect information on other individuals' preferences and ballot markings, the case considered in the text,

$$f(\gamma) = 1 \quad \text{for } \gamma = \gamma^*, \quad \text{otherwise } 0.$$

²⁴ One scheme that elicits true preferences for this two-individual case allows either individual to allocate 50 per cent of the outcome probability. Each individual takes half his probability as given and deals with the rest as he would in Figure 1, with the vertical scale relabeled with .5 placed where there is now a 1.

²⁵ If $y = z = 1$, then p_3 can be 0. As z can no longer increase, p_3 need not further decrease in value.

Shortcomings of Schemes That Are Unique with Respect to Cardinal Preferences

Theorem III has a very unfortunate implication. No democratic voting schemes that are unique with respect to individuals' cardinal preferences will guarantee Pareto-optimal outcomes. (We saw this previously when there was but one individual.) With two individuals and three alternatives, say, all three alternatives will have positive entries in the probability outcome vector. The diagram below illustrates why, in general, such a probability vector cannot represent a Pareto-optimal outcome. In the diagram, each alternative is represented by its vector of individuals' utilities.

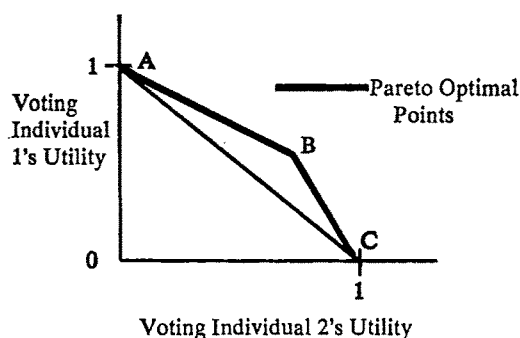


Figure 2. Pareto optimality and vectors of individuals' utilities for alternatives.

The northeast frontier represents all Pareto-optimal points. These points can be achieved through linear weighting of alternatives *A* and *B* or a linear weighting of alternatives *B* and *C*. If all three alternatives are given positive weight, a point strictly interior to the triangle *ABC* will be the outcome. Every such interior point is dominated by a point on the northeast frontier; therefore it cannot be Pareto-optimal. Only in the case where all three alternatives lie on the same straight line will it be possible for a Pareto-optimal scheme to give positive probability to all three alternatives. (In such a case, the concept of efficient choice loses much of its meaning. All possible probability outcome vectors are Pareto-optimal.)

The conclusion, therefore, is that in general when there are but two individuals, only two alternatives will be involved in Pareto-optimal outcomes. Converting the geometry to n dimensions, we find that with n individuals a maximum of n alternatives will need to be given positive probability weight in order to achieve all Pareto-optimal outcomes. Furthermore, assigning positive probability to more than n alternatives will lead to non-Pareto-optimal outcomes unless the

utility vectors of the excess alternatives are linearly dependent (lie on the same straight line in the two-individual case) on those in some set of n .

Theorem IV: In an n -individual world, assigning positive probability to more than n alternatives whose vectors of individuals' utilities are linearly independent will result in a non-Pareto-optimal outcome.

Major Theorem

The motivation at the start of this paper was to discover a voting system that leads to Pareto-optimal outcomes. Individuals were assumed to vote in their own self-interests. It was recognized that depending on the situation and the voting system, they might or might not vote honestly. The vote-processing system would then lead to an outcome. For at least some situations we should like to require that the outcome be democratic. It was hoped that the outcome would be Pareto-optimal as well. The objective was to find systems that could guarantee these properties. Unfortunately, no such system exists.

Theorem V: No voting system that relies on individuals' self-interested balloting can guarantee a nondictatorial outcome that is Pareto-optimal.

Proof

Consider the case of two individuals with strict preferences among three alternatives whose vectors of individuals' utilities are linearly independent. By Theorem II, "if the ballot processing mechanism is to lead to Pareto-optimal outcomes, the voting individuals' ballots must be unique with respect to their cardinal preferences." By Theorem III, this voting scheme "must lead to probability vector outcomes all of whose elements are positive." But by Theorem IV, assigning positive probability to all three alternatives in this two-individual world will lead to a non-Pareto-optimal outcome. The three theorems together prove that no satisfactory voting system can be found for this case. An equivalent demonstration can be provided for any number of voters.

Summary

The recent literature of social choice has provided rather pessimistic conclusions about the possibility of passing from individuals' ordinal preferences to socially desirable outcomes. Most voting mechanisms, the most common social choice procedure where political outcomes are involved, elicit no more extensive information than individuals' ordinal preferences, and those preferences may not be honestly revealed.

This paper has confronted these problems.

First, it showed that procedures are available that will elicit individuals' honest ordinal and cardinal preferences. With honest cardinal preferences as possible inputs to a decentralized voting procedure, there was a flicker of hope for finding an efficient social choice mechanism. The flicker was extinguished when it was discovered that any social choice mechanism that elicits honest preferences automatically leads to non-Pareto-optimal outcomes. Theorem V presents the disheartening conclusion.²⁶ No voting system that relies on individuals' self-interested balloting can guarantee a nondictatorial Pareto-optimal outcome.

Conclusion

No decentralized mechanism can guarantee a satisfactory social choice. This result is particularly unsettling to us economists, for we are accustomed to the paradigm of the efficient market mechanism functioning in a world of perfect competition.²⁷

Perhaps we should not ask despairingly, Where do we go from here?, but rather inquire, Have we been employing the appropriate mind-set all along?

We economists, to the extent that we are scientists, have fallen prey to the natural tendency to concentrate our attentions on those phenomena for which our theories have the greatest power. The land of perfect competition has been our homeground, and most times when we have ventured out, it has been to consider areas that differ in but one or two aspects from the place we know best. Our prescriptive theories for situations with externalities and public goods with rare exceptions have been developed as if the "aberrant" case we are considering is the sole deviation from the norm of the competitive model. By following this track we have achieved some useful results. But this success has only been achieved at a cost. We have constricted ourselves to view and evaluate new lands in the same terms that we have been able to apply profitably to familiar territory.

²⁶ New variants of Theorem IV could be developed by altering the strength of different definitions, and interchanging existential and universal modifiers. I suspect that Pareto improvements may be possible: stronger results with the same definitions, or stronger definitions leading to the same results.

²⁷ I am indebted to Thomas Schelling, who suggested that I emphasize this theme in my conclusion and provided me with helpful insights. This theme reiterates the central message of my volume, *Studies in Interdependence*.

The results of this paper contribute to a muffled but growing plea: Let us not rely on market-based standards of behavior and performance when examining those important areas of the world that in no way resemble the model of perfect competition. Rather, we should describe and evaluate those areas employing terms and theories developed to meet their specific needs.

Where might this lead us? It might force us to undertake some study of the role of preference revelation. With the competitive market outcome, it is each man for himself. No one cares about anyone else's preferences, and that is the reason that no one has an incentive to disguise or distort information about his own. In all other situations, we care very much about the structure of everyone else's preferences. Self-interested people become appropriately self-conscious about revealing their preferences, and find that they have an incentive to distort them. For example, the essence of a voting problem is a common or shared decision. I shall try to influence your vote. Not only that, the way I shall vote myself will depend very much on my assessment of the voting patterns of others. I cannot be relied upon to provide the information that any vote-aggregating mechanism will wish to elicit. Decentralized decision procedures cannot be expected to function effectively. Recognizing this, we might ask, Is a fully decentralized decision procedure what we should really seek? (Indeed, to the extent that consumer sovereignty relies on individuals' expressed preferences, we might even question its universal desirability.) In political contexts, this would come down to asking, Is voting the appropriate way to make social decisions?²⁸

This brings us full circle from the quote that opens this paper. That observation suggests invoking a geometric analogy for a concluding theme. Geometers may find that they can derive their most powerful results if they analyze and deal with circles, for circles are representations of perfect closed curves. But the more general category is the class of closed curves. It deserves attention too. Reference to the exceptional perfect case, the full circle in this instance, may be a type of analogy that is regularly overdrawn.

²⁸ Many observers of the political scene would argue that our present social choice procedures are very far from aggregative processes that rely on individuals' preferences. See Edward Banfield, "'Economic' Analysis of 'Political' Phenomena; A Political Scientist's Critique" (mimeo.), 1967, and a forthcoming joint piece by Banfield and Zeckhauser on the same subject.

A New Shape Measure for Evaluating Electoral District Patterns

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The year 1812 is famous for the birth of that favorite political monster, the gerrymander. Many facets of this creature's subsequent behavior have been studied although most scholars have overlooked its most powerful trait—the ability to hypnotize both friend and foe alike. Politicians, political commentators, and political scientists have become fascinated by the shapes of electoral districts. Politicians have proposed and sometimes enacted legislation to ensure contiguity and compactness;¹ political commentators have been using their imaginations to conjure up more political monsters;² and political scientists have recently been trying to measure some elusive shape ideas.³ In this short note I want to shake off the hypnotic influence of the gerrymander to consider briefly how the concept of shape has been approached in other disciplines, notably geography, so that we may return to the electoral districting problem with what seems to be a new shape measure that is particularly suited to this problem.

Shape is in many ways a rather peculiar empirical concept. In its most general form it can only be a classificatory concept.⁴ Thus areal shapes are often compared to the shapes of familiar objects—Italy is like a boot, etc. This nominal level of measurement is equivalent to that of the political commentators referred to above. But if we compare areal shapes to geo-

metric shapes⁵ whose basic properties are known, we can produce shape measures at a ratio level. This is one of the basic approaches of political scientists who compare electoral district properties with an ideal circle.⁶ When we do this we are in fact making our shape concept more specific, and we usually call these measurements measures of *compactness*. However, this is not necessarily the final stage in making the shape concept more specific. We can in fact envisage several types of divergence from compactness.⁷ Thus, while we would all recognize a thin rectangle as being "less compact" than a square or circle, we would also have to make a similar statement concerning a five-pointed star. Clearly we have two further concepts here, namely, *elongation* and *indentation*. Furthermore, we can add two topological divergences from compactness to our list, namely, *separation* (noncontiguity) and *puncturedness* (shapes with holes in them).⁸ The advantages of this abstract consideration of shape are that given these four divergences from compactness, we can (1) look at the shape measures we have and see what divergence(s) they measure, and we can (2) make a judgment about which concept is particularly relevant to a given problem and then concentrate on its particular measurement without incorporating less relevant aspects of the shape.

⁵ See David R. Lee and T. Thomas Sallee, "A Method of Measuring Shape," *Geographical Review*, 60 (Oct., 1970), 555–63.

⁶ Reock, p. 73. Schwartzberg, p. 448. In other subjects this approach has been used by: V. C. Miller, "A Quantitative Geomorphic Study of Drainage Basin Characteristics in the Clinch Mountain Area, Virginia and Tennessee," Office of Naval Research, Geography Branch, Project NR 389-042, Technical Report, 3 (1953); Stanley A. Schumm, "The Evolution of Drainage Basins and Slopes in Badlands at Perth Amboy, New Jersey," *Bulletin of the Geological Society of America*, 67 (May 1956), 597–646; Jack P. Gibbs, ed., *Urban Research Methods* (New York, 1961), pp. 99–106, and Lee and Sallee, p. 557. A short review can be found in Peter Haggett and Richard J. Chorley, *Network Analysis in Geography* (London: Arnold, 1969), pp. 70–78.

⁷ Peter J. Taylor, "Distances within Shapes: an Introduction to a New Family of Finite Frequency Distributions," *Geografiska Annaler*, 53B (July, 1971), 40–53.

⁸ Taylor, Figure 3.

¹ Emanuel Celler, "Congressional Apportionment—Past, Present and Future" in Symposium of 12 articles on "Legislative Reapportionment," ed. R. Kramer, *Law and Contemporary Problems*, 17 (Spring, 1952). Andrew Hacker, *Congressional Districting* (Washington: The Brookings Institution, 1964).

² Charles Roberts, "The Donkey, the Elephant and the Gerrymander," *The Reporter* (September 12th, 1952) 30–4. Robert J. Sickels, "Dragons, Bacon Strips and Dumb-bells—Who's Afraid of Reapportionment?" *Yale Law Journal*, 75 (July, 1966), 1300–8. Gus Tyler and David Wells "Camel Bites Dachshund," *New Republic*, 145 (November 11th, 1961), 9–10.

³ Ernest C. Reock, Jr., "Measuring Compactness as a Requirement of Legislative Apportionment," *Midwest Journal of Political Science*, 5 (Feb. 1961), 70–74. Joseph E. Schwartzberg, "Reapportionment, Gerrymanders, and the Notion of Compactness," *Minnesota Law Review*, 50 (January, 1966), 443–52.

⁴ See Carl C. Hempel, *Fundamentals of Concept Formation in Empirical Science* (Chicago: University of Chicago Press, 1952).

With regard to the first point, shape measures that relate a shape's area to a specified circle would seem to be most sensitive to elongation,⁹ while measures relating a shape's perimeter to a specified circle's circumference are particularly suited to measuring the degree of indentation.¹⁰ Clearly, neither of these approaches is suitable for measuring separateness or puncturedness, although these concepts can be picked up by more general measures of shape based on aggregate divergences from a center of gravity.¹¹ Since difference indices seem to measure different aspects of the general concept of shape, it becomes imperative to consider exactly what one wants to measure in a given problem. Let us consider what we want to measure in the context of political districting problems.

The introduction of shape criteria in any districting problem has traditionally sprung from both positive and negative motives. On the positive side the purpose of considering shape has not been explicit, but very often the shape of districts has been associated with attempts to produce areas within which contact is relatively easy.¹² This contact in turn may foster a sense of community and identity within the district by both party organizations and voters. Clearly noncontiguity is unacceptable on these grounds, since it means areas of a single district are separated by zones of other districts. Indentation is related to contact less directly. Although "indented" shape districts are topologically unitary, so that contact between locations can be maintained by moving solely within the shape, in practice, when any indentations exist, contact between extreme points will be through

zones of adjacent districts. The effect therefore will be similar to that of noncontiguity, and hence will possibly inhibit ease of voter participation and party organization.

The negative motive of compactness and contiguity conditions in districting has been much more explicit: It is simply an attempt to prevent unfair districting of the gerrymander variety. In building up an electoral district from smaller base areas, such as city blocks or rural counties, the drawing of any single boundary will involve making a decision every time base area boundaries join. If the decisions are largely made on partisan political grounds, the result may well be a series of the peculiarly shaped districts with which we are all familiar.

If the above discussion of the purposes of shape measures as districting criteria is accepted, it leads us to conclude that the measure we need should be sensitive to indentation divergences from compactness, while concentrating on the junctions of the base area boundaries where the basic districting decisions are carried out. The indentation index I_A can be applied in just this way.

I_A is a shape measure based on angles rather than one of the more usual distance-based indices. Any shape can be considered a closed curve made up of a finite number of straight line segments. Adjacent pairs of lines define the interior angles of the shape which may be reflexive (greater than 180°) or nonreflexive (obtuse and acute). Every indentation will be represented by a reflexive interior angle. Thus the total number of reflexive interior angles is an *absolute* measure of indentation. This simple measure, however, will partially reflect the number of straight line segments used to describe the shape. This problem is overcome by relating the number of reflexive angles to the number of nonreflexive angles and total angles. The following expression thus defines a *relative* measure of divergence from compactness due to indentation:

$$I_A = \frac{N - R}{T}$$

where N is the number of nonreflexive interior angles, R is the number of reflexive interior angles and $T = N + R$. For all compact shapes that are convex, $R = 0$, $N = T$, and therefore $I_A = 1$. In contrast a five pointed star has five acute angles at the points, five reflexive angles between stems, resulting in $I_A = 0$. These are the two limiting cases, since N cannot be greater than T and R cannot be greater than N if the curve (shape boundary) is to be closed.

⁹ For instance, the measures proposed by Reock, Schumm and Gibb.

¹⁰ For instance, the measures proposed by Schwartzberg and Miller.

¹¹ For the use of such measures in political districting problems compare: Henry F. Kaiser, "An Objective Method for Establishing Legislative Districts," *Midwest Journal of Political Science*, 10 (May, 1966), 200-23. James B. Weaver and Sidney W. Hess, "A Procedure for Non-partisan Districting: Development of Computer Techniques," *Yale Law Journal*, 73 (December, 1963), 288-308. For the use of such measures in other problems, see: Ronald B. Boyce and W. A. V. Clark, "The Concept of Shape in Geography," *Geographical Review*, 54 (Oct. 1964), 561-72; D. H. Blair and T. H. Biss, "The Measurement of Shape in Geography: An Appraisal of Methods and Techniques," *Bulletin of quantitative data for geographers*, 11 (University of Nottingham, 1967).

¹² This justification for considering shape in redistricting is more explicitly recognized in the British context where size and shape are related to problems of accessibility within a constituency; see David Butler, "The Redistribution of Seats," *Public Administration*, 33 (1955), p. 127.

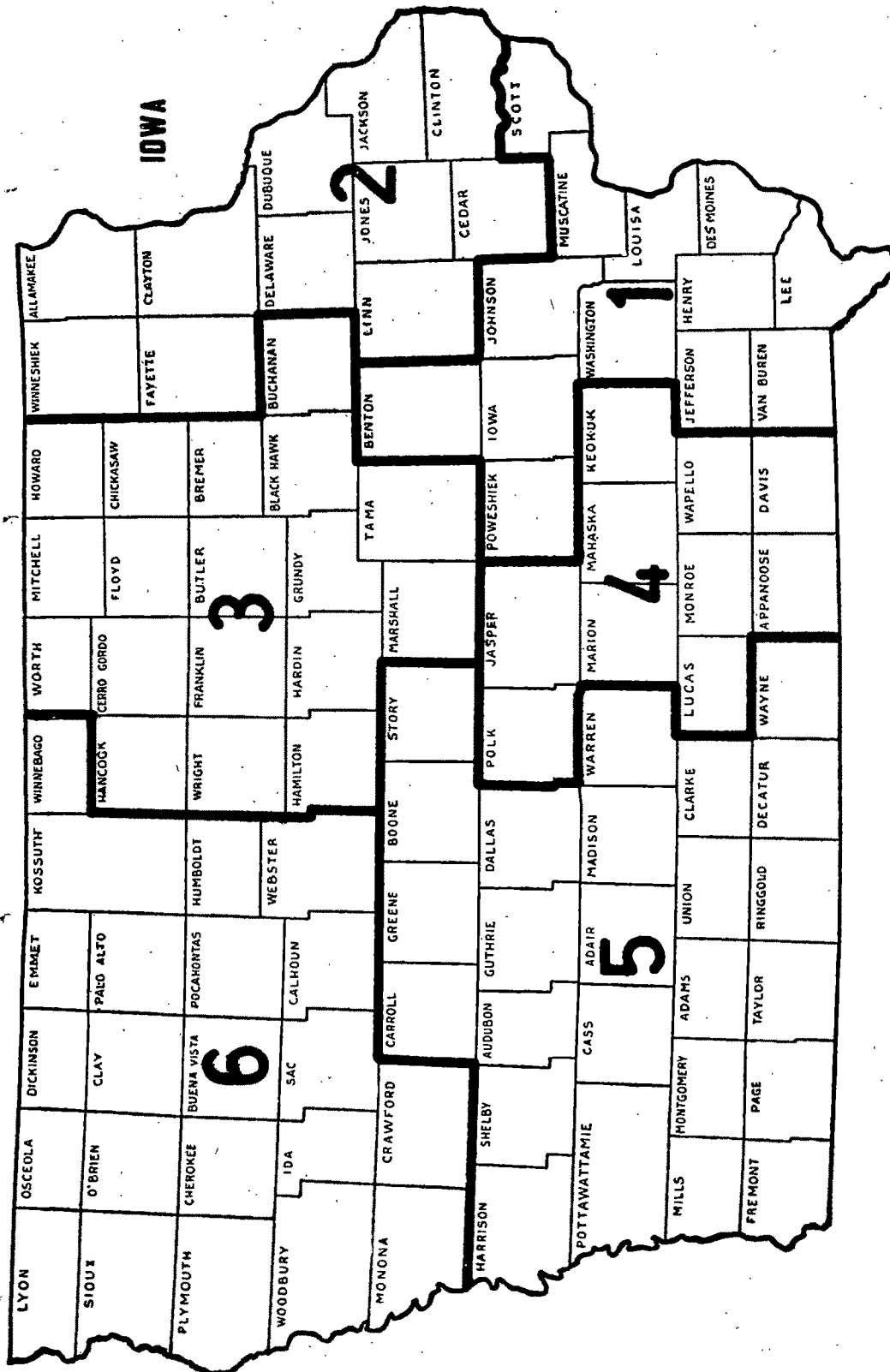


Figure 1. Proposed Congressional Districts: Iowa 1971

The application of I_A to the assessment of electoral districts is relatively simple. Any electoral district boundary may be generalized as a series of straight line segments between base area junctions.¹³ (Obviously, we do not wish to consider indentations arising from base unit boundaries [rivers, coastlines, etc.] beyond the control of the districting procedure.) With electoral districts in this form, I_A may be easily calculated, thus permitting measurement of the degree of indentation of the district in terms of the base unit junctions where the ultimate districting decisions are made.

We can briefly illustrate the application of this measure using the Iowa Congressional Districts approved by the Iowa legislature in 1971. All six proposed districts exhibit indentations, and so we can expect I_A values of less than unity. To measure the degree of indentation we initially enumerate the number of base area junctions on a district's boundary, since each of these represents a decision on the part of the districting agent. Iowa's First Congressional District, for instance, has 24 base area junctions on its border. Of these, seven produce internal reflexive angles so that $I_A = (17-7/24) = 0.42$. The I_A indices for the other five districts can be similarly calculated producing the following results: 2nd—0.47; 3rd—0.68; 4th—0.52; 5th—0.64; and 6th—0.69.

Let us now consider the uses to which this shape measure can be put. Clearly it cannot be used as an absolute criterion since other factors such as population equality—might well be violated by a system of neat convex districts. In any case, a control on district shapes in no way prevents gerrymandering, since the required pattern of districts for unfair districting will depend on the underlying distribution of voter preferences.¹⁴ Dixon concedes, however, that: "A rule

of compactness and contiguity, if used merely to force an explanation for odd-shaped districts, can have much merit."¹⁵ The I_A index seems particularly well suited to this role of evaluating proposed district patterns. By concentrating on indentations, it pinpoints divergences from convexity and thus may be used to "force an explanation." In our Iowa example it is immediately apparent that the three districts with low I_A values (1, 2, 4) include those counties (Polk, Linn, Dubuque, Johnson, Des Moines) from which the state's minority party, the Democrats, draw most support. The explanation that this suspected gerrymander forces, relates to the greater need for indented boundaries in this part of the state because of its uneven population distribution. At a more specific level, however, the two I_A values below 0.5 for the First and Second Districts do invite comment on their shapes particularly with regard to their common boundary. In effect this boundary locates Johnson County in the Republican First District thus "wasting" Democrat votes that the Democrats would much prefer to have in the more marginal Second District. No doubt this can be justified on population equality grounds although our boundary analysis does suggest a partisan effect from this neutral population criterion.

In this paper I have tried to develop a shape measure for evaluating electoral districts from an initial abstract consideration on the concept of shape. We are now in a position to return to the more exciting world of gerrymanders. Thus the most famous recent gerrymander, Tuskegee's "24 sided sea-horse"¹⁶ encloses 12 reflexive angles and so has maximum indentation scoring zero on the I_A index. We can therefore conclude that, at the very least, I_A identifies the extreme cases. Furthermore, we now have a new tool specifically designed to force explanations of odd shaped districts that is relatively easy to interpret and can be easily understood and therefore used by professional and layman alike.

¹³ Schwartzberg ("Reapportionment," p. 447) terms these points "trijunctions," since the base area boundaries meet in threes.

¹⁴ This point is particularly well developed by Robert G. Dixon, *Democratic Representation: Reapportionment in Law and Politics* (London: Oxford University Press, 1968), Chapter 18. See also Peter J. Taylor, "On Some Implications of the Spatial Organization of Elections," *Transactions, Institute of British Geographers* (forthcoming, 1973).

¹⁵ Dixon, p. 460.

¹⁶ Bernard Taper, *Gomillion versus Lightfoot: The Tuskegee Gerrymander Case* (New York: McGraw Hill, 1962).

Policy and Priority in the Budgetary Process*

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Politics and the Budgetary Process

By an impressive margin the federal government spends more money, does more things, and affects more people than it did twenty-five, or even ten, years ago. But the record of innovation and expansion has not been uniform; rather the prosperity of individual programs has varied widely during this period. Some programs have continuously prospered, grown, and evidently become permanent fixtures in the governmental system: farm commodity supports, highway construction, Social Security, and, most recently, health care. Others seem to have thrived only temporarily: aircraft nuclear propulsion, manned space flight, and the "Green Berets"; while still others have an uneven history of success and failure and are constantly involved in political turmoil: AID, Model Cities, and the Atomic Energy Commission's "Plowshare" program.

What is responsible for the success or failure of governmental programs? Who (or what) determines our national priorities? Political scientists have long regarded the budgetary process as the richest source of evidence on these matters. The assumption has been that the budgetary process, the most important "action forcing" mechanism in government, reflects the aspirations and controversies which cause some programs to be favored over others. Within the budgetary process clashes of interest and priority should be expressed in real dollar terms. Evidence concerning the goals of program directors and clients, the support of agency heads, the surveillance of the Office of Management and Budget (formerly the Bureau of the Budget), the strategic choices of the president, and the influence of Congress should all be contained in the data generated annually by the federal budget cycle.¹

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¹ The "traditional" literature on the budgetary

process is immense. The text and references in either Arthur Smithies, *The Budgetary Process in the United States* (New York: McGraw-Hill Book Company, 1955), or Jesse Burkhead, *Governmental Budgeting* (New York: John Wiley and Sons, 1956), provide convenient guides. Of particular interest are: Richard E. Neustadt, "The Presidency and Legislation: The Growth of Central Clearance," *American Political Science Review*, 48 (September, 1954), 641-671; Arthur Maass, "In Accord with the President's Program," in C. J. Friedrich and K. Galbraith, eds., *Public Policy*, 4 (Harvard University Press, Cambridge, Mass., 1954), 77-93; Fritz Morstein Marx, "The Bureau of the Budget: Its Evolution and Present Role," I and II, *American Political Science Review*, 39 (August and October, 1945), 653-684 and 869-898; and Lucius Wilmerding, Jr., *The Spending Power* (New Haven: Yale University Press, 1943). The notion of an "action forcing" mechanism is, of course, Richard E. Neustadt's.

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² Aaron Wildavsky's highly influential *The Politics of the Budgetary Process* (Boston: Little, Brown and Co., 1964) is the watershed. The ideas embodied in this analysis were more precisely formulated in Otto A. Davis, M. A. H. Dempster and Aaron Wildavsky, "A Theory of the Budgetary Process," *American Political Science Review*, 60 (September 1966), 529-547. See also, by the same authors, "On the Process of Budgeting: An Empirical Study of Congressional Appropriations," in *Papers on Non-Market Decision Making*, ed. Gordon Tullock (Charlottesville: Thomas Jefferson Center for Political Economy, 1966), pp. 63-133. This general analytic approach has been importantly extended by John P. Crecine. See his "A Computer Simulation Model of Municipal Resource Allocation" (paper delivered at the Midwest Conference of Political Science, April, 1966), as well as *Governmental Problem Solving* (Chicago, Ill.: Markham Pub. Co., 1969). See also Ira Sharkansky, *The Routines of Politics* (New York: Van Nostrand, 1969).

given period pretty much determines what will happen in subsequent periods.

There is, of course, an important element of truth in these findings. Administrative budgets are not rewritten from scratch every year; and it is a little silly to judge the consequences of policy innovations as if they were. Further, it is undoubtedly true that political historians (and public administrators) often exaggerate the importance of particular policies, choices, and competing personalities. After all, they find the business of public administration interesting precisely because it embodies great controversies between powerful forces—conflicts which seem both to be inherent in and to shed light upon the process of popular government.

Still, the gap between traditional and quantitative approaches to the budget remains. It is not enough to dismiss the conclusions of the older school out of hand, as the product of too much enthusiasm for public administration and the inevitable result of unsystematic research. Much of the traditional literature comes from men who in fact have served in government—participant observers, if you will—and their description of the central role of the budgetary process as a policy vehicle is important if only because that is what they saw happening.

The challenge of the quantitative budget studies is precisely over the question of how much public policy is embedded in the budgetary process. Their argument is that administrators rarely depart significantly from what they are already doing. They "muddle through" as best they can, making only marginal changes in the established operations of government. Consequently, agency budgets are massively stable from one year to the next.³ Bureaucratic behavior and the budgets that result from it seem to reduce to a simple set of operating principles that, at the same time, leave no room for other explanations.

The most striking fact about the equations [which describe the budgetary process] is their simplicity. This is perhaps partly because of the possibility that more complicated decision procedures are reserved for special circumstances represented by extreme values of the random variable. However, the fact that the decision rules generally fit the data very well is an indication that these simple questions have considerable explanatory power. Little of the variance is left unexplained.⁴

³ See in this regard Charles Lindblom, "The Science of 'Muddling Through,'" *Public Administration Review*, 29 (Spring, 1959), 79–88; and Charles Lindblom, *The Intelligence of Democracy: Decision Making Through Mutual Adjustment* (New York: The Free Press, 1965).

⁴ Davis, Dempster, and Wildavsky, p. 543.

In the end, this line of reasoning may yet provide a new and strong foundation for the study of public administration. However, it may be also that the relationship between public policy and the budgetary process has been obscured by the way the problem has been quantified.

The theme of this essay is that the political choices and conflicts inherent in the administration of government have been unnecessary casualties of the quantitative revolution. The problem is essentially one of theory. After reviewing the theoretical underpinnings of the quantitative budget studies we will suggest an alternative approach to the manipulation of budgetary data, developing along the way a methodology that will uncover controversies over policy and priorities.

The Incremental Model of Budgeting

The budgets of federal agencies are developed over a period which begins as much as eighteen months before appropriations requests are acted upon by the Congress. During this period agency budgets are typically formulated to meet the guidelines established by the OMB speaking for the president and his administration. The initial budgets normally undergo a sequence of successive reductions and appeals often culminating in a presidential review.

The budgetary process itself is composed of six stages, though the precise pattern that is followed may vary slightly by agency and department, particularly in the final stages of appeal and presidential review. The process begins with the budget guidelines that are sent down by the OMB. These are deliberately varied from year to year so as to communicate to the bureaucracy a sense of limited resources and to direct attention to items that happen to be of particular concern to the administration. Usually agencies will respond to the OMB by preparing "flash estimates" or "preview budgets." These budgets serve to begin the process of negotiation, and, in the weeks that follow, a series of informal estimates are established indicating how much expansion is likely to be considered reasonable in light of the administration's priorities.

On the basis of this information, the agency's comptroller begins to aggregate budget requests from the operating divisions within the department or agency. Of course, division requests regularly exceed the amount of money that, according to the OMB, will be available. Hence these estimates are worked over by the comp-

troller with an eye towards bringing them in line with administration expectations.

The comptroller's recommendations along with each division's request constitute the material from which an agency budget is produced. It is during this part of the process that competition *within* the agency (or department) reaches its greatest intensity. Each division struggles for funds against the budgetary interests of the other operating divisions, each trying to avoid the heavy hand of the comptroller by shifting the burden of budgetary cuts to some other division's programs.

The easiest way for a division to protect itself is, of course, to gain the support of the department secretary or agency chairman. But the competition for funds often extends beyond the administrative framework in which the operating divisions are located. Thus, it is not unusual to find program directors drawing in other elements of government—congressmen, other bureaus and departments, consultants, lobbyists, etc.—in order to gain some vital budgetary advantage. (Admiral Rickover, for one, was so skilled at managing his congressional support that he operated virtually outside the normal review procedures of the AEC.) The department or agency must now produce a series of budgetary settlements that compromise the demands of its working parts along with the push and pull of external political forces while, of course, trying to hold to OMB guidelines.

The budgetary outcomes at this stage of the process become formal requests to the president for funding and are sent to the OMB for review. Acting "in accordance with the President's program,"⁵ the OMB "marks up" each budget, item by item, program by program—cutting away wherever it can, accepting other requests as submitted, and occasionally, adding money to some project that the administration looks upon with special favor. For the managers of the federal bureaucracy, this is the most difficult part of the process, because it threatens to unbalance the blend of policies and priorities that has been arranged among a department's working parts. Yet the entire thrust of the OMB at this point is to attack budget requests that have produced unnecessary and expensive settlements. Budgets then are normally marked up with a heavy hand and returned to those who wrote them for reconsideration.

Here the agency or department has a choice: it can accept the OMB's actions as they stand

or it can appeal to the OMB for some measure of restoration. It is important to note here that any further budgetary action by either the president or the OMB occurs only over contested items in the budget. Those items not disputed by the OMB or subsequently returned to the OMB by appeal are regarded as a settled part of the administration's budget. Thus, the budget is now agreed upon for the most part with only a small number of controversies remaining.

The OMB reconsiders those matters that are still disputed and again leaves the agency or department with a choice. Again the decision of the OMB can be accepted, thus settling the matter. There is, however, also the alternative of appealing the OMB's final action directly to the president. It is understood that each agency chairman or department secretary can argue for restorations directly before the president, much like a knight pleading before his king for special favor. The president's decision, of course, ends the process and the results are printed up and sent along to Congress (where specific issues can be reopened). Several things ought to be noted about the presidential budgetary reviews. The first is that this stage is centered on specific budgetary items and does not usually include a general review of the agency's or department's budget. Second, it is in these last stages of appeals that the budgetary process varies most widely. These variations usually reflect the relative stature of particular branches of government. (Thus military budgets, for example, have a preferred status in the appeal process.) Also, it is at this point that the standing of the department's chief administrator is determined. It is he who must go before the president and argue for funds that are presumably vital to his department's functions. Consequently, the degree of success that he has with the president is widely regarded as an evaluation of his administrative worth and, at the same time, a measure of the importance that agency or department has in the administration.

By design, then, the budgetary process produces a climate of scarcity in which the success of those who participate is measured by the number of dollars they are able to win. Indeed, the entire process of formulating budgets within agencies plays upon the institutional interests of bureaucrats so as to produce the explicit competition between alternative "policies."⁶

⁵ See Arthur Maass, "In Accord with the President's Program," for an excellent discussion of the great subtlety and variation with which this phrase is used.

⁶ For a thorough review of one such cycle, see Irvin C. Bupp, "Priorities in Nuclear Technology: Program Prosperity and Decay in the USAEC, 1956—

This competition among alternative programs seems to have been all but lost in the work of Davis, Dempster, and Wildavsky. These scholars have argued that agency budgets are produced by relatively straightforward incremental strategies and hence are not often an especially interesting source of conflict, not to mention change and innovation.⁷ Specifically, they have examined the variations in fifty-six nondefense federal agencies from 1947 to 1963 and discovered that they can be explained very simply by a "set of simple decision rules that are linear and temporally stable."⁸ In their most sophisticated equation (decision rule), Davis, Dempster, and Wildavsky argue that the congressional appropriation of any agency is

... a fixed mean percentage of the agency's request for a certain year plus a fixed mean percentage of a dummy variable which represents that part of the agency's request for the year at issue which is not part of the appropriation or request of the previous year plus a random variable representing the part of the appropriation attributable to the special circumstances of the year.⁹

This is to say that appropriations subcommittees normally give an agency some proportion of what it asked for, adjusted by things in dispute and special considerations peculiar to a particular year.

The point that Davis, Dempster, and Wildavsky most emphasize is that their decision rules fit the data: "... the results are very good. We leave very little of the variance statistically unexplained."¹⁰ In fact, for the majority of congressional decisions (appropriations) their equations yield R's exceeding 99 per cent, and for all but two show R's above 90 per cent. Further, these decision rules seem to square neatly with other research which suggests that the budgetary process in appropriations subcommittees has become an institutionalized se-

ries of actions sanctioned by an elaborate set of norms and behaviors.¹¹

The creative insights upon which Wildavsky and others have built are contained in the writings of Herbert Simon and his colleagues.¹² The theme of this work is that people are quite limited in their ability to process new information, generate alternatives, anticipate consequences, and weigh values. The Simon-Lindblom administrator faces a highly complicated world, a world of multiple values and goals related to each other in unknown ways. He has no very reliable way of predicting the consequences of alternative courses of action and little information pertinent to assessing these consequences even if he had an acceptable theory. As Crecine has put it,

[He] is a man with limited knowledge, limited information, and limited cognitive ability, making a policy choice in an uncertain world by "drastically" simplifying the problem and making marginal adjustments in past "successful" policies to formulate current policies.¹³

The phrase that captures the essence of the administrative situation is "organized complexity."¹⁴ And nowhere are its consequences more apparent than in the making of budgets. The budgetary process is, after all, a complicated procedure in which officials are usually under the intense pressure of time and circumstance. One expects that whatever decision rules have worked in the past will be used again in the solving of current disputes. Indeed, one does not expect the level of disagreement to be very high, because individual administrators can be

¹¹ Richard Fenno, "The House Appropriations Committee as a Political System: The Problem of Integration," *American Political Science Review*, 56 (1962), 310-324. See also Richard Fenno, *The Power of the Purse: Appropriations Politics in Congress* (Boston: Little, Brown and Co., 1966).

¹² Herbert A. Simon, *Administrative Behavior*, 2d ed. (New York: Macmillan, 1957); Herbert A. Simon, *Models of Man* (New York: Wiley, 1957); Herbert A. Simon, Donald Smithburg, and Victor Thompson, *Public Administration* (New York: Knopf, 1950); David Braybrooke and Charles Lindblom, *A Strategy of Decision* (New York: The Free Press of Glencoe, 1963); Richard Cyert and James March, eds., *A Behavioral Theory of the Firm* (Englewood Cliffs: Prentice-Hall, 1963); H. A. Simon, "Theories of Decision Making in Economics and Behavioral Science," *American Economic Review*, 59 (June, 1959) 253-283; G. P. E. Clarkson and H. A. Simon, "Simulation of Individual Group Behavior," *American Economic Review*, 60 (Dec., 1960), 920-932.

¹³ Crecine, *Governmental Problem Solving*, p. 11.

¹⁴ Herbert Simon, "The Architecture of Complexity," Reprint No. 113 of the Graduate School of Industrial Administration, Carnegie Institute of Technology.

1971" (Ph.D. dissertation, Harvard University, 1971), chap. 1.

⁷ Davis, Dempster, and Wildavsky, pp. 540-543, do make explicit provision for deviant cases in their analysis. These are cases that lie beyond the normal rules of budgeting that they describe; they are also cases that presumably involve controversy, change and innovation in the budgetary process. This line of thought is further developed in their subsequent article: "On the Process of Budgeting II: an Empirical Study of Congressional Appropriations," *Studies in Budgeting*, eds. R. F. Byrne, A. Charnes, W. W. Cooper, and D. Giffords (North Holland, Amsterdam, 1971), pp. 292-375.

⁸ Davis, Dempster, and Wildavsky, p. 529.

⁹ Davis, Dempster, and Wildavsky, p. 534.

¹⁰ Davis, Dempster, and Wildavsky, p. 537.

expected to request funds for what they are already doing, give or take a little bit.

These observations are all quite true as far as they go. Across aggregations of decision-making units (federal agencies, municipal administrations or individual budget-makers), behavior at time "T" can be shown to be a robust function of comparable behavior at "T - 1." What is striking is that administrative behavior is so very similar in all sorts of diverse organizations. State governments, business organizations, labor unions, organizations of every sort seem to promote the same sort of responses from those who administer them, responses that are rationalized in budgetary processes that do not seem to vary widely in effect from organization to organization.¹⁵

The trouble is that in politics the budgetary process, while certainly responding to the requirements of administrative life, is also a battlefield for conflicting priorities and alternative policies. The categories of analysis established by Wildavsky, Crecine, and others help us to understand the behavior of the pertinent decision maker as he attempts to resolve the problems that he faces. But this line of reasoning does not shed much light upon the sort of things which over time cause some activities to become the continuing policies of government while others fade and are discarded. It is this variation in the *competitive* success of alternative programs, rather than the cognitive processes of decision makers, which is central to the politics of public administration. It is this aspect of government that the "problem solving" perspective is inherently unable to explain.

Let us stress again that we are not quarreling with descriptions of administrative behavior. Quite to the contrary, our argument is premised on the findings of Simon, Lindblom, Cyert and March, and Wildavsky. Administrators in complex organizations *behave similarly*. However, accounting for these patterns of administrative behavior—itsself vital to the science of public administration—is not the focus of our problem. We want to explain why some programs are repeatedly funded while others are considerably less successful, *given* that the behavior of their administrators is likely to be quite similar. This is to say that we are looking at a different level of public administration. Our focus is not on the behavior of administrators, *per se*,

but on administrators as they are organized in terms of policy outputs, programs as they compete for dollars.

In a sense budget data present us with a measurement problem: *How are yearly government budgetary figures reordered so that they indicate policy choices and public priorities?* This is by no means an easy question to answer. For the budget reflects simultaneously a yearly *process of administration* and decisions among *alternative political priorities*. That is, budgets are at once measures of the way government is organized and of the policy decisions that the organization implements. Obviously, these are not the same things although they are nested within the very same data.

The fact that political choices and the budgetary process are so completely intertwined accounts for much of the confusion among approaches to public administration. What has happened is that the traditional scholars have spoken of public administration without placing it within the context of an annual, stable budgetary process. Thus, they have tended to write about policy as if that were the only thing that government was producing. In discovering that there is, in fact, a budgetary process in administration, younger and more quantitatively trained researchers assumed that the process of government, in addition to producing budgets, produced settlements on questions of public policy. Here the quantitative approach was a victim of its own theoretical and statistical precision. The budgetary process models worked so well that there simply did not seem to be anything left to explain.

The reason for the great statistical success of the quantitative budget studies lies in the fact that *agencies* were taken as the unit of analysis, yearly budget cycles being nothing more than replications of the same event over time. These data yielded to Wildavsky and his associates a simple set of equations (decision rules) that, by and large, seemed to describe how public policy is formulated while reserving more complex and sophisticated political strategies for a small number of "deviant cases." What has been captured—quite accurately—by this formulation is the great stability of the administrative structure of government. But because administrative categories have formed the basis of budgetary analysis, the entire process in which public policy is produced has been obscured.¹⁶

¹⁵ Compare: Thomas Dye, *Politics, Economics, and the Public: Policy Outcomes in the American States* (Chicago: Rand McNally and Co., 1966) with Cyert and March, and William Leiserson, *American Trade Union Democracy* (New York: Columbia University Press, 1959) with Crecine.

¹⁶ In policy terms, the entire metaphor of normal and deviant cases is vastly misleading. With it, Davis, Dempster, and Wildavsky promote the idea that bureaucracy is largely inert, proceeding to do today

The first step, then, in untangling information on policy choices from data on the administration of government is to shift the level of analysis for agencies and departments to programs. This is to say that in the context of the budgetary process, programs are the operating units of public policy, that they provide "categories that are closer to being true outputs [of government] than the older categories."¹⁷

It should be noted, however, that it is difficult to make explicit comparisons *between* program budgets (although even with the data in this form a great deal can be done).¹⁸ Programs vary widely in their scope, size, and content, and these differences are unfortunately reflected in their budgets. The amount of money necessary to fund a strong cancer research program differs substantially from the funding necessary to lift the incomes of the elderly above the "poverty line." Similarly, reducing the army's manpower budget by 100 million dollars has a markedly different effect from taking the same number of dollars away from OEO. In a fundamental way, then, all budgetary dollars are not equal; and while all policy outcomes

are expressed in terms of dollars, some care must be taken to make this information commensurate across programs. This is to argue that merely shifting to programs as the basis of policy analysis is not in itself sufficient, that budgetary data organized by programs must themselves be transformed so that the relative prosperity of programs in the policy process can be measured and compared.

It should be added, finally, that programs have histories of support and opposition, and focusing explicitly on them returns this dimension to budgetary analyses. Agencies (or departments) usually do not have such histories associated with them (except in their first years of operation). They are normally accepted as part of the fixed institutional landscape, terrain that seems to change dramatically only during periods of great crisis and political turmoil. An important lesson of the Eisenhower and Nixon Administrations is that changing the party in power does not open up already established agencies to direct attack. However, the relative priorities for programs within an agency (and even the relative standing of the agency as a whole) do vary quite widely from administration to administration, even though the agency continues as an established part of government.

Thus, the salient characteristic of established agencies and departments is their stability—both organizational and budgetary. And again, it is this aspect of government that Wildavsky and others have captured so well. Yet within departmental and agency boundaries (and occasionally between them, e.g., the case of OEO) there is a constant struggle by program directors, lobbyists, congressmen, state and local politicians, and White House personnel to fund new ideas and to continue the funding of old ones. It is a competition that draws to it people and institutions with radically different ideas and purposes; and that decides, in the end, what public policies we are to have.

From this perspective the struggle to establish new public policies looks very much like the competitive ideal in nineteenth century capitalism. Entrepreneurs—"policy entrepreneurs," if you will—seek scarce resources for their programs. In their quest for funding and political authority, they use every available weapon: pressure from various constituencies and groups, aggressive selling inside government, attracting congressmen as innovators or as protectors (congressmen who in turn often lobby other congressmen), pressuring the White House as well as receiving pressure from the White House, and so on through a diverse range of opportunities and strategies. The fact

essentially what it did yesterday, all without competition and controversy. The exceptions, by this logic, are reserved for a small number of deviant cases which involve "more complicated decision procedures."

The entire thrust of our argument is that the logic of the budgetary process involves public policies in a continuous struggle for scarce resources; that, if there is any "normal state of affairs" in the policy process, it is one of intense competition between programs for public funds. Further, this logic holds equally for policies that desire nothing more than to continue to do what they were doing last year. There is nothing simple or automatic about continuing. Again we must stress that we are speaking of policies and programs here, not the administrative framework in which they are managed. Thus when Davis, Dempster, and Wildavsky (pp. 544-545) suggest that the analysis of individual programs follows from the broad outlines established in their own work, we must respond that their emphasis on normal and deviant cases has started such research off in the wrong direction.

¹⁷ Roland N. McKean and Melvin Awshen, "Limitations, Risks and Problems," in *Program Budgeting: Program Analysis and the Federal Budget*, ed. David Novick (Cambridge, Mass.: Harvard University Press, 1965) p. 286.

¹⁸ For a thoughtful discussion which complements many of these points, see Charles L. Schultze, *The Politics and Economics of Public Spending* (Washington, D.C.: The Brookings Institution, 1968). Indirect, but vivid, support for many of the same arguments can be found in Charles L. Schultze, Edward R. Fried, Alice M. Rivlin, and Nancy H. Teeters, *Setting National Priorities: The 1972 Budget* (Washington, D.C.: The Brookings Institution, 1971). And at the municipal level see John E. Jackson's perceptive essay, "Politics and the Budgetary Process," *Social Science Research*, 1 (April, 1972) 35-60.

that political strategies vary widely as a result of the peculiar combination of resources that happens to be available probably explains why "the case study method" of public administration was never able to produce a cogent set of generalizations. But this should not be taken as evidence that there is no struggle, that the process is one of slow, incremental change. Rather it suggests that the policy process in the United States is an open one where all sorts of influences are at play, and where—because the process is a long and complex one (six stages plus congressional action over more than eighteen months)—the competition between policies is reinforced and accentuated.

The Case of the AEC

To make these thoughts more concrete, let us turn to the activities of a particular federal agency: the Atomic Energy Commission. Though now a mature agency with established purposes and programs, the AEC was created and in many ways has continued to exist in an atmosphere of uncertainty and conflict about the appropriate applications of a new and poorly understood technology.¹⁹ Was nuclear technology to be applied exclusively to the development of weapons? Or were its applications ultimately to be essentially civilian and peaceful? Could peaceful applications be carried out with economy and safety? By whom, and under whose supervision? These and other questions have defined areas of sharp conflict among philosophies of government and among programs and personalities. The early history of these disputes has been compiled and is presently being published as a multivolume study of the Atomic Energy Commission.²⁰ A definitive account of the programs and controversies that developed over the years within the AEC, this material provides a rich historical context against which we can examine the AEC's activities as they are reflected in the agency's budgetary records. Perhaps more important, how-

ever, are the specific characteristics of the actual budgetary records upon which we have been able to draw. First, we have been able to identify and trace the histories in whole or in part of twenty-three distinct AEC programs during the period of 1958–1972. While these programs do not represent an exhaustive categorization of all AEC activities, they are all "real" programs as distinct from mere projects on the one hand or "accounting categories" on the other. The twenty-three identified programs are mutually exclusive but are not exhaustive of all Commission activities, comprising on the average about 90 per cent of any given year's total AEC operating budget.²¹

Second, within each of the fifteen "budget cycles," FY 1958–FY 1972, we have isolated six discrete stages beginning with the program divisions' requests to the Commission's general manager and ending with the official presidential budget for a given year. The intermediate stages represent the general manager's recommendation to the Commission; the Commission's "September" submission to the Administration; the OMB's "mark-up" of that submission; and the "appeal" of the "mark-up," either to the Office of Management and Budget or the president.²² Consider now the data reflected in Table 1.

In Table 1 we have ignored the programmatic breakdown of the fifteen AEC budgets and have simply correlated the requests for allocations for each stage across the fiscal cycles for which we have data. For these fifteen observations, the funds requested at a given stage correlate almost perfectly with that requested at a previous stage and that granted at a subsequent stage. The perfection of these correlations is powerful support for Wildavsky's basic argument about the regularities of the budgetary process, but it suggests very little about the political conflicts which we *know* were taking place during the period spanned by this data.

¹⁹ James R. Newman and Byron S. Miller, *The Control of Atomic Energy* (New York: McGraw-Hill Book Co., 1948). See also David E. Lilienthal, *The Atomic Energy Years, 1945–1950* (New York: Harper and Row, 1964).

²⁰ Richard G. Hewlett and Oscar E. Anderson, Jr., *The New World, 1939–1946, A History of the USAEC*, Vol. 1 (University Park, Pennsylvania: Pennsylvania State University Press, 1962); Richard G. Hewlett and Francis Duncan, *Atomic Shield, 1947–1953, A History of the USAEC*, Vol. 2 (University Park, Pennsylvania: Pennsylvania State University Press, 1969). A separate study of the Naval Nuclear Propulsion Program has also been prepared by Richard Hewlett and Francis Duncan and is scheduled for publication in the Fall of 1973.

²¹ One of the authors (Irvin Bupp), was for several years on the staff of the secretariat of the AEC and hence had access to the agency's internal financial records. This association led to AEC support under the auspices of the Office of the Historian for the research reported here.

It is perhaps also worth noting that the AEC is one of the very few executive agencies to have followed a generally consistent "output" format in its budgetary practices since its creation. This fortunate circumstance considerably simplified the problem of isolating and tracing distinct programs.

²² This fifth "appeal" stage is the only one not strictly comparable across all fifteen years. Records of this process were often difficult to locate, and for FY 1959 and FY 1960 there is no evidence that an "appeal" as such was allowed at all.

Table 1. Correlations Between Budgetary Stages for 23 AEC Programs Pooled Across FY 1958-FY 1962

	Division	General Manager	AEC	OMB	Appeal	Presidential
Division	1.000					
Gen'l Mgr.	.996	1.000				
AEC	.997	.999	1.000			
OMB	.995	.998	.997	1.000		
Appeal	.996	.997	.997	.998	1.000	
Final	.995	.996	.997	.998	.999	1.000

For, within this apparently stable framework, five substantial AEC programs were cancelled altogether, two major activities experienced a sharp monotonic decline in their fortunes, three grew impressively, and most others fluctuated widely. Moreover, the picture is not very much different at the program than at the agency level.

When requests and allocations are correlated at the program level there is on the whole less close association across stages but the average magnitude of the correlation coefficients is still quite high. Most are in the .80-.90 range, and out of twenty-three programs only four show associations of less than .70 between pairs of stages. So, even at places where conflict is *known* to have occurred, the budgetary process perspective merely uncovers "striking regularities." Simply changing the units of analysis, that is to say, does not uncover the underlying political competition that structures the process of policy formulation; a more profound change is required.

It is at this point that we come to manipulating budgetary data themselves, to transforming dollars so that they will better reflect the competitive struggle for growth, for continued (or renewed) prosperity. The elements that we have to work with are: the agency in which programs operate (i.e., the administrative context), the past success or failure of a program in obtaining funds, and the action that is taken on program requests as they pass through the budgetary cycle (i.e., a program budget from initial request to final presidential decision).²³

²³ Congressional action is not included in our calculations, although a good case could be made for doing so. Congress is, after all, quite active in the budgetary process through its committee and subcommittee system; see Fenno, *Power of the Purse*. We are concerned, however, with the formulation of program budgets, not with their review. Congressional review of the budget really moves on to a different level of analysis. Of course, much of congressional influence is informal. As such, it should be entered into any explanation of why some programs prosper and others fail. This is again a different analytical problem from

The raw data, of course, come in the form of absolute dollar levels of programs from year to year. And it is by transforming and recombining these data that we hope to create an "index of prosperity" which will reflect the relative political success or failure of programs as they compete for scarce resources. Such an index has to meet two different problems of comparability.²⁴

The first has to do with the variation in the total AEC operating budget between FY 1958 and FY 1972, which was, of course, by no means constant over that period. In Table 2 below, we have computed the sum at each stage of the twenty-three programs on which we have calculated data. It varies from year to year, ranging from a low of \$2006.3 million in FY 1967 to a high of \$2568.5 million in FY 1963.²⁵

It seems reasonable to argue that programs are equally prosperous only if each preserves its proportion of the total even as the total changes *both* from year to year *and* across the stages of budgetary process. A first step, then, toward operationalizing the notion of "prosperity" is to divide all program allocations at all stages by the total for that stage. This procedure, however, leads to a second problem of comparability.

There is great variation in the absolute magnitude of program budgets. Among the twenty-three AEC programs for which we have data, several have budgets in the \$5-\$10 million

developing an index of program prosperity—the results of which can then be searched for appropriate causes.

²⁴ For a somewhat different, but also highly imaginative, approach to this problem, see John Jackson, "Politics and the Budgetary Process."

²⁵ For any given year or stage, the totals reflected in Table 2 are equal to about 90 per cent of the comparable total operating budget, the remaining 10 per cent being that portion of the AEC budget which we were unable to allocate to meaningful output categories. Since we are explicitly interested in competition among programs, the aggregated totals shown have been used as the basis for the transformation described in the text.

Table 2. Total at Each Budget Stage for 23 AEC Programs
(In \$ millions)

Fiscal Year	Division Request	General Manager	Commission	OMB	Appeal	Final
1958	\$2170.9	\$2059.1	\$2199.1	\$2081.2	\$2083.2	\$2076.6
1959	2414.9	2235.4	2267.9	2253.9	2253.9	2252.3
1960	2575.7	2501.7	2510.9	2447.5	2472.7	2451.0
1961	2494.3	2409.6	2368.8	2206.6	2310.3	2296.4
1962	2538.7	2499.2	2491.1	2285.3	2306.5	2343.0
1963	2586.2	2428.7	2447.5	2389.8	2592.3	2568.5
1964	2781.2	2468.5	2586.6	2366.7	2548.3	2381.4
1965	2505.1	2428.1	2447.0	2294.5	2283.1	2239.6
1966	2323.8	2251.4	2257.4	2104.0	2195.3	2129.1
1967	2213.6	2098.1	2118.2	1986.0	2034.4	2006.3
1968	2399.9	1999.1	2110.1	2005.8	2053.6	2026.4
1969	2438.2	2272.8	2269.8	2151.7	2225.3	2180.5
1970	2376.0	2238.9	2274.4	2104.8	2169.9	2037.4
1971	2469.4	2285.1	2303.9	2024.4	2121.0	2013.7
1972	2420.7	2179.5	2150.7	1962.6	1990.2	1980.5

range, and a few have budgets that were greater than \$400 million. The obvious consequence is that each of the smaller programs is a tiny proportion of the total agency budget and is thus "lost" for analytical purposes. For example, one of the most politically controversial AEC programs is "Plowshare," a project to develop nonweapons applications for nuclear explosives. This program has never commanded more than one per cent of the total Commission budget.

We need to normalize the percentages obtained by dividing each program budget by the appropriate total so that these small programs are not made to seem insignificant out of sheer tininess. To adjust for this problem we have first calculated for each budget stage a program's *mean* proportion of the total for that stage across either (a) all the years spanned by our data, or (b) when a program began or was cancelled within that period, the years during which the program existed. We have then divided a program proportion of the total for each stage/year by this mean and multiplied the quotient by 100, converting to a percentage. The effect is to normalize these percentages so that they average 100 for the period FY 1958-FY 1972, or, when appropriate, for the years a program existed. We believe that the characteristics of this index and its behavior over time are highly suggestive of the process of competitive growth, i.e., "prosperity" in which we are interested. Eventually we shall want to use "prosperity change scores." This is the numerical difference of the prosperity scores across years for a given stage and will be

zero when a program's proportion of the stage total does not change over the years.

At this point, it is useful to state these arguments more formally. For a full array of budget observations for programs over a number of years across each budget stage, $D_{(i,j,k)}$, where

i = programs (1-23 in the case of the AEC),

j = a specified budget stage (1-6),

k = years,

T = Total agency budget for the sum of all programs

the corresponding array of prosperity scores, $P_{(i,j,k)}$ is defined as:

$$P_{(i,j,k)} = (Pr_{(i,j,k)} / \text{mean } p_{(i,j)}) * 100$$

where

$$(a) \quad Pr_{(i,j,k)} = D_{(i,j,k)} / T_{(j,k)}$$

for

$$T_{(j,k)} = \sum_{i=1}^{23} D_{(i,j,k)}',$$

and where

$$(b) \quad \text{mean } p_{(i,k)} = T p_{(i,j)} / Ic_{(i,j)}'$$

for

$$T p_{(i,j)} = \sum_{k=1}^{15} Pr_{(i,j,k)}',$$

and

$Ic_{(i,j)}$ = the number of years for which the i th program in the j th stage > 0 .

Table 3. Four AEC Program Histories Expressed as "Prosperity Scores"

Fiscal Year	Rover (Nuclear Space Rocket)	High Energy Physics	Sherwood (Controlled Thermonuclear Research)	Nuclear Weapons (Research, Develop- ment and Production)
1958	25	20	87	69
1959	19	25	95	75
1960	28	34	130	67
1961	34	36	95	68
1962	56	51	88	80
1963	128	82	86	94
1964	187	118	83	92
1965	165	107	80	109
1966	174	122	92	105
1967	173	144	103	104
1968	131	149	108	110
1969	145	147	109	122
1970	103	156	114	129
1971	94	154	123	133
1972	33	150	97	136

In Table 3 we present the scores for four programs within the AEC, all of which show striking variance in prosperity. In fact, all twenty-three programs exhibited a lively and theoretically interesting variance, but limitations of space preclude discussing them all.²⁶

We see quite vividly the growth, decay, and fluctuation of which we have spoken—all within the context of a roughly constant total budget. These variations were caused by political events, not the operation of a budgetary process. The sudden reversals in the fortunes of "Rover" were caused by various officials of three administrations having attached sharply different priorities to the undertaking. The competitive success of high energy physics resulted in part from the constant attention of the Atomic Energy Commission Chairman with a strong interest in pure research. High energy physics did not grow "accidentally" or "naturally"—indeed none of these curves represents any sort of "natural process." On the contrary, each is the record of a sequence of priority settings made within a political system. Programs do not naturally grow, decline, or remain constant, or anything else. They are caused to do all of these by politicians and administrators making often difficult choices among competing claims upon scarce resources.

This "authoritative allocation of values" is the essence of the political process, a process that is vividly portrayed in these prosperity scores. The relative stability of "Sherwood" (controlled thermonuclear research) is not an

illustration of organizational process in Lindblom's terms.²⁷ Sherwood has remained constant, has *resisted* competing claims upon its share of the Commission budget, only because it has been supported by the Commission, the Office of Management and Budget, and the president. Its constant level of prosperity is an indicator of stable political support, not of the operation of some "budgetary constant." Its support could have evaporated completely much like the nuclear rocket program's (Rover)—a program which received high priority in the early years of the Kennedy administration only to be completely abandoned.

Alternatively, consider the history of weapons. This has been a truly prosperous enterprise and one with an especially suggestive administrative history. The circumstances surrounding the 1950 presidential decision to build thermonuclear bombs left the Atomic Energy Commission with little influence over the future of this program.²⁸ The function of the Commission came to be one of merely fulfilling requirements for nuclear explosives in the establishment of which it played no role. The AEC, that is, played no role in setting the priority of weapons relative to other nuclear activities or among types of weapons. The competition for resources among the latter took place with the relative priority of weapons already set by forces external to the Commission. The weapons program record of continuous prosperity is largely the story of the consequences of the ac-

²⁶ Cf. Lindblom, "The Science of 'Muddling Through.'"

²⁸ Hewlett and Duncan, 362-409.

²⁶ For a full discussion of all twenty-three programs, see Bupp, chapters 1-3.

Table 4. Arithmetic Mean of Prosperity Change Scores, FY 1958-FY 1972, 23 AEC Programs
(Presidential Budget)

Rank	Program	Mean	Rank	Program	Mean
1	High Energy Physics	9.0	13	Sherwood	0.57
2	Nuclear Safety	8.1	14	Rover	.43
3	Prog. Dir. and Admin.	7.1	15	Euratom	.08
4	Gen. Reactor Tech.	6.1	16	Training, Edu. and Information	-2.2
5	Biol. and Med.	5.4	17	Advanced Systems	-2.8
6	Cooperative Power	5.1	18	Spec. Nuc. Mats.	-3.5
7	Weapons	4.5	19	Pluto	-4.0
8	Naval Reactors	4.5	20	Army Reactors	-7.0
9	Civilian Power	4.3	21	Merchant Ship	-7.7
10	SNAP	4.1	22	Raw Materials	-13.6
11	Plowshare	3.5	23	Aircraft Nuclear Propulsion	-25.6
12	Isotopes	3.3			

tions taken in 1950. The program emerged from the "great thermonuclear debate" with a very real competitive advantage over other AEC programs. Weapons was a "stronger" program.²⁹

These measures of prosperity permit us to summarize AEC program activities since 1956. We could, of course, just subtract a given program's prosperity in FY 1958 from its score in the FY 1972 and rank the program according to the numerical value of the result. The "net prosperity change" is not, however, an especially helpful number. Now, we can recover exactly the same rank ordering by working instead with "prosperity change scores." For any program, the mean of the distribution of these change scores across all fifteen yearly intervals is also an indicator of how that program has prospered during the period for which we have data. The numerical value of these indicators, moreover, has an instantly appealing interpretation. A glance at Table 4 shows that since FY 1958 fifteen programs have prospered and eight have not.

What does all this have to tell us about how priorities are established within government? There is growing interest in examining federal budgets to determine how the nation is allocating its resources and exercising its spending priorities. The authors of one of the best and most recent of such studies correctly point out that in order to understand what the federal budget for any one year says about the national priorities, one must pay attention to past trends.

In determining priorities and formulating the federal budget, the President and his advisors do not start with a clean slate, deciding *de novo* how . . . the expenditures for fiscal 1972 should be allocated

in meeting national goals. Recent history, prior commitments, current political realities, relations with Congress, economic and social events beyond the control of budget makers—all play a major role in limiting their ability to change radically the current shape of the budget. What they consider desirable must be tempered by what they consider feasible. . . . The margin of truly free choice is surprisingly small.³⁰

Still, patterns of obligation and expenditure do change even at the aggregated federal level. Schultze and his colleagues, too, have found impressive variation in the objects of government expenditures over time. They proceed to infer changing priorities from changing absolute outlays.³¹ But we also know that programs are in explicit competition with each other as they move jointly through the budget formulation process. A measure of relative priority levels must take account of this competition.

The prosperity index developed in the preceding pages does so; it extracts from each individual budgetary observation a *meaning determined by its context*. The essential idea here is that the individual budgetary figures must be seen as the product of two quite different processes. One is the *budgetary process*, the public accounting that organizations give for the dollars that they receive. The stress here is on organization, the continuing complex hierarchy of government—bureaus, divisions, agencies, and departments. And the key to understanding administrative behavior in these terms is in the incremental nature of the process. The other is the *policy process*. Here emphasis is on the competition among programs for scarce resources which are needed to expand, or to continue, or even to begin. The central problem in

²⁹ For a more careful definition of "program strength," see Bupp, chapter 2.

³⁰ Schultze et al. (1971), *Setting National Priorities*.

³¹ Schultze et al., *Setting National Priorities*, 19-21.

Table 5. Product-Moment Correlations Prosperity Change Scores, Presidential Budgets
FY 1958-FY 1972 with Weapons Program

Raw Materials	-0.47	SNAP	-0.26
Special Nuclear Materials	-.25	General Reactor Technology	-.44
Civilian Power Reactors	-.13	Advanced Systems	-.10
Cooperative Power Reactors	-.32	High Energy Physics	-.29
Euratom	-.19	Sherwood	-.37
Merchant Ship Reactors	-.28	Biology and Medicine	-.24
Army Reactors	ns*	Training, Education and Information	ns
Naval Reactors	ns	Isotopes	-.17
Aircraft Nuclear Propulsion	ns	Plowshare	-.25
Pluto	ns	Program Direction	-.23
Rover	ns		

* ns = not significant

analyzing political competition between programs is to make the information about each comparable. Dollars are the operational unit of administration. Political competition is something else again. The great difficulty in untangling these two aspects of the budgetary process is that they occur simultaneously.

The methodological problem this poses can be handled by a series of transformations which in general affect different observations in different numerical ways but whose result is a set of numbers which now may be compared freely among themselves. Inspection of these numbers confirms the fundamental proposition of this preliminary analysis: in the context of the "massively stable" processes cited by other scholars, there is considerable variation in the fortunes of federal activities *at the program level*. We believe that the explanation of this variance constitutes the real challenge for further empirical analysis of budgetary data.

Analyzing Federal Budgets

What has been the nature of the competition among programs? Has it been a "war of all against all" or are certain activities capable of causing other less strong programs to "pay" for their prosperity either by exercising first claim upon newly available resources or by actually growing at the expense of lower priority activities?

Professor Russett has suggested that we may be able to recover some evidence about the extent of such "benefits" or "payments" by examining the patterns of covariation among yearly changes in program levels.³² Since we now have strong *a priori* reason to believe the Atomic Energy Commission's weapons program to

have been independent of other Commission activities for the past twenty years, the inter-correlation of changes in weapons prosperity with those in the other twenty-two programs seems especially pertinent. They are reported in Table 5.

It is clear that everyone pays for defense. With the exception of the Commission's various military reactor enterprises (the largest group among those with insignificant correlations) all AEC programs change scores are negatively associated with changes in the weapons program. As anticipated, the relationship between prosperity changes in raw materials and weapons is high and negative. Decay in the prosperity of the former has been strongly associated with the growth of weapons. But the latter has evidently not only benefited from the decline of this program, it has also in Russett's sense been able to make other programs, notably general reactor technology and controlled thermonuclear research, "pay" for its prosperity.

The difficulty with such patterns of covariation is that, though fascinating, they may also be *wholly spurious*. Certainly they do not in themselves illuminate the real causal processes which produce variation in the prosperity of programs. At most, the *sign* of the associational measure, particularly when it is negative, may provide some clue about the right questions to be asked. Thus in the case of the AEC weapons program one could plausibly accept the overall pattern of inverse association as empirical confirmation of what we suspect on independent grounds. But positive correlations among prosperity change scores have no obvious causal interpretation and the meaning of differences in magnitude between any pair of negative coefficients is totally obscure. More important, the covariation patterns cannot tell us why some programs are stronger than others. Assuming the competitive struggle for limited funds is not exactly a war of all against all, what is it that

³² Bruce Russett, "Who Pays for Defense?" *American Political Science Review*, 63 (June, 1969), 412-426. See also, Bruce Russett, *What Price Vigilance: The Burdens of National Defense* (New Haven: Yale University Press, 1970), especially chapters 5 and 6.

makes some more equal than others? Is it strong directors who make for strong programs by "running a tight ship" and by "effectively generating support at the agency and congressional levels?"³³ Directors generating such support are able to neutralize opposition by the most potent natural enemy, the Office of Management and Budget. Programs which are supported at the Commission, the Office of Management and Budget, presidential and congressional levels are prosperous; programs which are not supported soon fall victim to the exigencies of the budgetary process.³⁴

This widely accepted professional wisdom, then, contains a clear theory of the governmental process. National priorities are not set by administrators with national constituencies; they are set at the operating levels of federal bureaus—by program directors sensitive to their own clienteles. National priorities are established by bureaucratic entrepreneurship in a process which settles priorities without anyone being aware of them. In this regard, Davis, Dempster, and Wildavsky's stochastic models perpetuate a fundamental error about the way government operates. The whole metaphor of an inert bureaucratic machine doing the same thing this year that it did last year misses the basic point. Priority setting in the federal bureaucracy resembles nineteenth century capitalism: Priorities are established by aggressive entrepreneurs at the operating levels of government. Programs prosper because energetic division directors successfully build political support to withstand continuous attacks upon a program's resource base by competing claims. As a consequence, the only matters which reach the president are those already in dispute. At the presidential level the administrative process is less one of "policy-making" than "dispute-settling." It is at this level that things are accepted simply because they have been accepted, that the desirable has to be adjusted to the feasible. On the whole, the differences which count, the actions which produce the patterns of relative prosperity and strength we

have discussed occur at lower levels of the administrative process.

All of this is missed by taking the Wildavsky and Lindblom perspective. By concentrating on the underlying regularities of the administrative process, these scholars are obliged to argue (in effect) that except for learning adjustments, no changes of any significance occur. We have seen that real change does occur within this "massive stability," reflecting real conflicts over purpose and priority. The more telling point about the process is that the program director, the operating-level bureaucrat, is a central figure in the determination of public policy. The history of the Atomic Energy Commission's weapons program is far more typical of the way public values are allocated than is the dramatic termination of the aircraft nuclear propulsion program.³⁵

This is a strong theory, and we must be careful to stress that our discussion is limited to the single agency on which we have data. But if this fact makes us somewhat cautious, we also feel that the concepts we have specified and the theory which underlies them are probably more—not less—applicable to other areas of government. The Atomic Energy Commission has been a fairly stable agency both in administrative organization and actual spending levels for the last ten years. That we find such great variation in the relative prosperity of programs here seems to suggest that we can expect greater fluctuations elsewhere, particularly in the controversial areas of social policy.³⁶ Do program directors occupy the same crucial position throughout the government? Is there more or less presidential leadership in other policy areas? For what programs is congressional intervention truly a significant factor?

These are the type of questions that need to be asked. By translating budgetary data into actual operating units of government programs, students of public administration should be able to provide more than speculative answers. Prosperity scores are themselves an important step in this direction, for they establish a measure of program prosperity and decay.

³³ These observations are based on a series of interviews with officials in AEC and the Office of Management and Budget.

³⁴ Russett's substantive conclusions have recently been challenged. See Jerry Hollenhorst and Gary Ault, "An Alternative Answer to: Who Pays for Defense," *American Political Science Review*, 65 (Sept., 1971), 760-764.

By respecifying Russett's regression model to include several dummy variables, Hollenhorst and Ault claim to detect important "sub-period effects." This more methodologically sophisticated approach, however, seems to us merely to compound the original theoretical error. The posited relationships are spurious.

³⁵ See, W. Henry Lambright, "Shooting Down the Nuclear Plane," Inter-University Case Program, ICP Case Series No. 104 (Indianapolis: Bobbs-Merrill, 1967).

³⁶ Congress appears to be much more active in the area of social policy. An excellent account of administrative policy in this area refers again and again to congressional intervention in specific programs. See Gilbert Y. Steiner, *The State of Welfare* (Washington, D.C.: The Brookings Institution, 1971). Or in a different area, Arthur Maass, *Muddy Waters: The Army Engineers and the Nation's Rivers* (Cambridge, Mass.: Harvard University Press, 1951).

COMMUNICATIONS

TO THE EDITOR:

In his recent article "Positivism, Historicism, and Political Inquiry," *APSR*, 66 (September, 1972), 796-817, Professor Eugene F. Miller discusses on pp. 812-14 my views on the relevance of existential phenomenology to the philosophical foundation of political inquiry and comes to the conclusion that "there is much to be learned from Jung's reexamination of the philosophical foundations of political inquiry, but he does not provide us with a genuine alternative to an historical relativism" (p. 814). Because of lack of space, I shall limit my present comments to a clarification of a few crucial points and postpone what I hope will be a fuller dialogue to another occasion.

Since I take Miller to be essentially a Straussian or belonging to the school of contemporary political thought whose spiritual guru is Leo Strauss, I ought to mention a critique of Strauss's "essentialism" I wrote several years ago.¹ Then I agreed with Strauss that positivism created a moral vacuum in contemporary political thought. Yet I asserted that "while, as Strauss agrees, positivism takes the naïve life-world for granted and, for this very reason, may be charged with negligence, his essentialism is guilty of self-contradiction" ("Leo Strauss's Conception of Political Philosophy: A Critique," p. 515). Strauss tends to reject all modern thought, the thought which does not conform to the classical standards as he interprets them: thus, for him, the history of modern thought is a history of nonphilosophy. His essentialism must be understood in terms of his ontological supposition that "to be" is "always to be," that is, there is *physis* which is unchanging, permanent, and universal. Without *physis* there can be no ethics, metaphysics, and epistemology. For Miller, too, without *physis* there is no valid ground for both "evaluation" and "explanation": to use the language of Stanley Rosen, who is critical of both Heidegger and Wittgenstein and whom Miller often cites with approval, there is only "nihilism." It must be pointed out, however, that Miller, unlike Strauss and Rosen, has not reduced positivism to a form of historicism, nor has he considered historicism as worse than positivism.

The word *historicism* has a controversial his-

tory in contemporary thought—from Karl Popper to Strauss, whose respective conceptions of it are diametrically opposed. Is existential phenomenology a form of historicism or historical relativism? Miller seems to think that it is. The belief that existential phenomenology is a historical relativism stems from a misunderstanding of the notion of the life-world (*Lebenswelt*). Inasmuch as the phenomenology of the life-world is a hermeneutics of social existence, so is Miller's epistemology a hermeneutics of *physis*. Take the example of Heidegger, whose philosophy I learned from the late John Wild (along with Edmund Husserl's and Maurice Merleau-Ponty's phenomenology, William James' radical empiricism, and John Macmurray's philosophy of action). For Heidegger, the "world" is the most inclusive horizon of meaning which is radically historical (or temporal). To acknowledge man's historicity (*Geschichtlichkeit*) is not to commit one to historicism (*Historismus*). To say that man is temporal is, for example, simply to affirm him as a finite being—man "dies," and only human beings can "die." Upon a close examination, Heidegger's thought does not reject *physis* as *logos* or *aletheia*. Whereas Miller, like Strauss, wishes to defend the notion of *physis* understood by post-Socratics, Heidegger goes back to the primeval notion of *physis* in pre-Socratics. Further, Miller seems to be suggesting that phenomenological thought is merely an extension of the *Lebensphilosophie* of Wilhelm Dilthey. It is well-known that in defending the notion of philosophy as a "rigorous science" (*strenge Wissenschaft*) Husserl was critical of Dilthey's historicism.

Then the real question is not simply an affirmation or a denial of "human nature" as such—the latter being called "historicism." Rather, Miller is interested in defending that version of human nature which is unchanging and universal without which, according to him, there is no possibility of knowledge or intelligibility. Even if there is a constant and universal human nature, the question of how conflicting theories about the permanent structure or content of human nature can be resolved remains to be answered. Miller's version of human nature is the one which is claimed to have a set of predetermined characteristics, whereas for the existentialist view, the essence of man is open and indeterminate. Temporally speaking, the former is a fidelity to the past, whereas the latter is an affirmation of the open future in the passage

¹"Leo Strauss's Conception of Political Philosophy: A Critique," *The Review of Politics*, 29 (October, 1967), 492-517. Miller's Straussian position is clearer in his "Political Philosophy and Human Nature," *The Personalist*, 53 (Summer, 1972), 209-21.

of time. "For human reality," Simone de Beauvoir writes in *The Coming of Age*, "existing means existing in time: in the present we look towards the future by means of plans that go beyond our past, in which our activities fall lifeless, frozen and loaded with passive demands" (p. 361).

Like Strauss, Miller holds that the world of common-sense understanding (or, as Strauss calls it, the "natural world" of man) is the presupposed foundation of the scientific or philosophical conceptualization of political things. However, essentialism, Miller's notwithstanding, reduces the world of lived experience (existence) to the world of pre-fixed essence (*physis*). It is tantamount to a claim for the primacy of *theoria* over *praxis*. According to Miller, "To know that a man is presupposes already some knowledge of what man is" (Miller, "Political Philosophy and Human Nature," p. 209): "existence" presupposes "essence." For him, therefore, without *physis* or in historicism "there is no possibility of obtaining knowledge that is objective, final, or absolute" (ibid., p. 218). In contrast to essentialism, in phenomenology the relevance of the life-world does not lie in its status as knowledge but instead lies in its status as the meaningful ground for human action, that is, as the original source of knowledge. So the geography of conceptual knowledge presupposes the natural landscape of preconceptual understanding. By exalting *theoria* at the expense of *praxis*, man himself is reduced to "a little abstract effigy" (Gabriel Marcel, *Being and Having*, p. 169). By "opticing" thought (Martin Buber, *Eclipse of God*, p. 40) or conceptual knowledge, essentialism loses the sight and importance of the life-world. It is then the quest for epistemological certainty or absoluteness that constitutes the central concern for Miller as "the distinguishing characteristic of human nature is man's desire and capacity to know" ("Political Philosophy and Human Nature," p. 219). The same temperament is shared, though strangely enough, by scientific objectivism whose slogans are, among others, conceptual rigor and predicative certainty.² By intending to elevate the intelligible, essentialism regards conceptual thinking as the activity of a disembodied and worldless mind. Whereas Husserl speaks of theoretical *praxis*, essentialism treats philosophy as if it is a lifeless activity.

² For a phenomenological critique of behaviorist epistemology and ontology, see the author's forthcoming article "A Critique of the Behavioral Persuasion in Politics: A Phenomenological View," in *Phenomenology and the Social Sciences*, ed. Maurice Natanson, 2 vols. (Evanston, Ill.: Northwestern University Press, 1973).

Human dignity, which both Miller and I cherish, is not served or preserved by postulating a pre-fixed human nature. On the contrary, it is elevated by affirming freedom, the denial of a pre-fixed human nature, in a world whose meaning is not fixed but must be constantly won. As John Wild said somewhere, freedom is not a natural but an existential norm. For a predetermined human nature (and a predetermined history, too, for that matter) is the death of human freedom and dignity or, as B. F. Skinner has it, the triumph of the reinforced man "beyond freedom and dignity," (or more appropriately, "against freedom and dignity"). The existentialist thesis that the meaning of existence is not fixed but is constantly won is not an acknowledgement of nihilism in which everything is permitted. According to Simone de Beauvoir, existential philosophy not only permits an elaboration of ethics but also is the only philosophy in which ethics has its place. It makes possible a genuine ethical theory precisely because it focuses on *how* man *acts* rather than *what* he is. This is the reason why I called the essentialism of Strauss "an ontological determinism." The structure of good and evil is not of God-given nature but is a human gift which has to be worked by *us all* in coexistence or transaction. As Martin Buber puts it, only where there is mutual sharing, is there reality or responsibility—including the reality or responsibility of thinking. Man ought to be distinguished from God, as time is from eternity. In conclusion: it may be said that the differences between Miller's views and mine stem from two fundamentally different styles of philosophy and ultimately views on life itself. Perhaps one is "priestly" and the other "jesterly." Be that as it may, if I were to call a stand-off between our styles of views and to refuse to acknowledge the superiority of mine over his, I would be a "historicist," and that would make my contention superfluous.

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TO THE EDITOR:

W. Phillips Shively's recent article "Party Identification, Party Choice, and Voting Stability: The Weimar Case," *APSR*, 66 (December, 1972), 1203-1225 is to be commended for its critical questioning of the utility of the American concept of party identification in the Weimar setting. The identification of *tendence* or sociopolitical bloc as an alternative does provide a better fit for the overall evolution of electoral patterns in many European systems.

Yet I cannot help but quarrel with Professor Shively's all too general conclusion that

"there is evidence that party identification in the sense of learned and enduring attachments to parties was not a factor in Weimar elections. Accordingly, the party identification theory of electoral stability apparently cannot help us explain Weimar voting" (p. 1220). This dichotomous present/absent approach to the question of lasting attachments to political parties cannot, in my opinion, be spread evenly across the board in the Weimar case without tending to distort our understanding of the phenomenal rise of the NSDAP as an electoral vehicle from 1928 to 1932. It is necessary to differentiate strongly, as scholars such as Lipset and Bracher do, between the qualitatively higher levels of voter stability, even through the worst depression years, of the SPD, KPD, and Zentrum, and the nearly complete electoral annihilation of all non-Catholic bourgeois parties (with partial exception of Hugenberg's anti-system Nationalists).

A more satisfactory approach might utilize party identification not as a dichotomous system-wide variable but rather as a party-specific variable within the *tendence* or bloc model. This, it seems to me, would more adequately take into account the organizational continuity of several parties and mass organizations, particularly the SPD and the Catholic parties, from the Kaiserreich to Weimar, as opposed to the hastily built Protestant bourgeois parties founded only after the Revolution of 1918.

The above revisions in the *tendence* approach might also be useful in comparing the development from Kaiserreich to Weimar to Bundesrepublik of party identification within each bloc. The mild recession of 1966-67 in West Germany and the accompanying quick rise (and equally quick decline) of the NPD, with many similarities to the NSDAP in ideology and voter appeal (see my *The National Democratic Party: Right Radicalism in the Federal Republic of Germany* [Berkeley: University of California Press, 1970]), indicates that this line of inquiry is still of substantive as well as theoretical concern in the area of German politics.

JOHN D. NAGLE

Syracuse University

TO THE EDITOR:

I believe that Professor Nagle has essentially restated here one of the arguments which I myself tried to make in my article. I distinguished there between party identification and loyalty to a socioeconomic bloc. Thus, I interpreted the collapse of the non-Catholic bourgeois parties as resulting from the fact that they represented

the same bloc as the Nazi Party, and that in the near-absence of party identification, there was nothing to prevent their voters from moving to the Nazis. By contrast, the KPD, SPD, and Zentrum represented different blocs, and bloc loyalty prevented their voters from moving to the Nazis.

Professor Nagle, as I read his letter, would prefer to treat bloc loyalty as a type of party identification. I do not think this would be fruitful since, as I suggest in my article, there are theoretically interesting differences between the two kinds of loyalty, both in the way they are acquired and in the way in which they manifest themselves in the electorate.

Two of Professor Nagle's suggestions are particularly useful:

(1) At some points in my article, for convenience in writing, I referred to the "absence" of party identification among the Weimar electorate. What my interpretation of Weimar voting suggests, however, and all that is necessarily supported by my findings, is that the level of party identification in Weimar was very low. A dichotomy between the "presence" and "absence" of party identification is unnecessary and untenable.

(2) Professor Nagle's emphasis on the organizational history of the parties is well-placed. This could help to explain, for example, why the inroads of the Nazi Party on other parties of its bloc were so much more complete than the inroads of the Communist Party on its rival, the Social Democratic Party.

W. PHILLIPS SHIVELY

University of Minnesota

TO THE EDITOR:

Read the Mendelson piece in the latest, December 1972, *APSR*, "From Warren To Burger," 1226-1233, and came across at least one grievous error/boo-boo. (So it seems to me because I have just finished a soon-to-be-published volume on Justice Black and Due Process, etc.) Mendelson, on page 1228, claims that "there was no dissent" in the important case of *Boddie v. Connecticut*. There was, indeed, a dissenting opinion, a very forceful dissenting opinion written by a Justice of the Court since 1937 who had, since his elevation to the Court, fought against judicial intervention and oversight functioning regarding the social and economic activities of state legislatures: Hugo Lafayette Black.

In the *Boddie* case, as in countless hundreds of opinions before, Black criticized the employment of the Frankfurter/Harlan et al. "fundamental fairness"/"shock the conscience" standard to evaluate the reasonableness and consti-

tutionality of state legislation. "Such a test," Black wrote in dissent, "willfully throws away the certainty and security that lies in a written Constitution, one that does not alter with a judge's health, belief, or his politics. I believe the only way to steer this country toward its great destiny is to follow what our constitution says, not what the judges think it should have said."

Knowing that Black would not like the thought of being associated with the subjective "shock the conscience"/"fundamental fairness" doctrine he so thoroughly criticized during his tenure as Associate Justice of the United States Supreme Court, I do hope you print this correction.

HOWARD BALL

Hofstra University

TO THE EDITOR:

I am very disturbed by the political bias that Professor Yang used in his review of my book, *The Politics of Formosan Nationalism*, APSR, 66 (June, 1972), 654-655. It might well have been written in Peking or Taipei because both regimes have blacklisted me for years and jailed hundreds of persons advocating self-determination for Taiwan.

My main objection is that he ignored the key purpose of the book, which was to present the views of native Taiwanese at home and abroad toward their postwar condition. My personal views were not the focus, as Professor Yang implies, nor do I feel today as I did three years ago when the book appeared. The changes in world politics since 1970 affect Taiwan especially, and have turned many who used to support the Taiwanese cause toward Peking or the KMT. Today the right of all 15 million on Taiwan to decide their future is paramount.

Professor Yang's criticism of my aim does not stand up, but I prefer to allow readers to judge for themselves the difficulty of studying native opinions in Taiwan, Rhodesia, Southwest Africa, or any Communist nation. The book explains fully why I could not use quantitative surveys as I have often done in Japan (the USIA told me it has never been able to ask sensitive political questions in Taiwan polls). To suggest that I should have surveyed overseas Chinese opinions ignores the fact that most are not Taiwanese but mainlanders descendants with little interest in or knowledge of Taiwan. I used all available secondary sources, including official Nationalist books and media, but never accepted official "facts" as automatically valid or relevant to the perception of the 13 million Taiwanese whose attitudes were my concern.

Finally, to claim that my book "performs a disservice to the betterment of Sino-American friendship" is to apply a standard which should never be used in academic reviews: I was not writing to please any government or further official policies. That would be the sole yardstick used by the two Chinese regimes in deciding whether or not to allow publication of a book. I am glad it is not used to censor American newspapers, magazines, or academic work.

DOUGLAS H. MENDEL, JR.

University of Wisconsin, Milwaukee

TO THE EDITOR:

I regret very much that Professor Mendel was "disturbed" by the "political bias" in my review of his book, *The Politics of Formosan Nationalism*. First, the "key purpose" of the book was recognized rather than "ignored," if one noted my remarks throughout the review. My major reservation was that the author passionately represented the views of an elite minority as being the voice of all 15 million people in Taiwan.

Professor Mendel wants his readers to believe that both the difficulty of scientific research inside Taiwan and the inadvisability of interviewing overseas Chinese would justify "those limitations" in the measurement devices of his behaviorist methodology. Consequently, his readers are expected to accept his conclusions that only the views of certain elite minority groups can be presented. Surprisingly, Professor Mendel mentioned nothing about the statistical lacunae and factual discrepancies I had cited, even though "all secondary sources available, and all KMT official data" were consulted. Besides, debunkers would not be impressed with the omission of a well-balanced bibliography.

Inasmuch as Professor Mendel stated in his book that he could fulfill his duty as a political scientist "to make his research available to the public as well as to governments and other scholars," one wonders whether Professor Mendel would still disclaim any credit should his advice have been adopted by the American Government in 1969 and after. Since that time, changes in world politics and the Nixon Administration's new China policy have not only crippled the movement of Formosan independence but also disconfirmed the predictions of *The Politics of Formosan Nationalism*. Obviously, the counsel of Professor Mendel's book did not serve the best interests of the governments concerned. Finally, Professor Mendel admitted in his letter that his own views were not the focus nor does he feel today as he did in 1969 when he read the final proofs before

publication, I am therefore puzzled why all his complaints became so imperative.

RICHARD HSIUH YANG

Washington University, St. Louis

TO THE EDITOR:

This is the first time that I have replied to a review of any of my books, and I hope it will be the last. Although he has penned an essay distinguished by his customary felicity of phrase and wit, Samuel Krislov also has indulged himself in various ad hominem speculations that detract from what (as I infer) he intended to be a generally favorable review of my *The Constitutional Polity*, *APSR*, 66 (September, 1972), 1043-44. Two points in particular demand correction: the first evidently may be comprehended as a slur on my present departmental colleagues, and the other as disparagement of my familial antecedents.

Krislov suggests that I attempted to discuss the possible impact upon the Supreme Court of radical ideology, because of the (evidently, undue) influence of my departmental colleagues at the University of Hawaii, rather than because of any serious interest I might have in the political ideology of Supreme Court justices. My Bacon Lectures were delivered in February, 1969, and they were published as the book which Krislov criticizes in 1970; I joined the faculty of the University of Hawaii in August 1971. The colleague influences to which I was exposed at the time I wrote the lectures were those of the Osgoode Hall Law Faculty of York University in Toronto, Canada.

Krislov states that "there are several class-origin digs by which Schubert seems to be asserting his own proletarian bona fides at the expense of middle-class colleagues." Hold that Tiger, Sam! What you evidently misunderstand is a remark in which I contrast our present phony affluence with the world of "our childhood, . . . a time when not just the urban ghettos and rural slums housed the relatively poor, but rather a time when almost everyone was absolutely poor for awhile." By "our" I intended to refer to Americans of my age group, and the statement was meant to be a description of social rather than personal history, as I thought and think the context (p. 133) makes abundantly clear. I have never previously thought it relevant to mention the matter, but my father was a graduate of Cornell, Phi Beta Kappa, Class of '14.

While I have the floor, I'd like to deal also with what Professor Krislov himself characterizes as his "greatest quibble," and in particular

with the first two of his three questions—the third would take us out of judicial behavior and into the very different subject of the professional role behavior of sociologists, and I am content to let that matter rest on the differences of opinion that Krislov and I evidently entertain. All three of these points are necessarily questions, because he introduces them with the caveat that his difficulty may—as it does—stem from his lack of familiarity with the relevant facts. So I hasten to supply these facts.

(1) He "find[s] it hard to imagine the empirical base on which th[e] conclusion is reached" that John Clarke was more liberal than Cardozo. It doesn't take very much imagination; I scaled the Taft and Hughes Courts, as my book details; and my data show that Clarke was considerably and consistently more liberal than Brandeis, while Cardozo was not significantly so.

(2) On the Steel Seizure decision, Krislov's sententious clincher ("All historical events occur when they do") rather begs the question *why* they occur when they do, and really, I insist, leaves open the only question of other than pedantic interest—which is a specification of the conditions under which it is possible for particular types of events to occur. I said that the Vinson Court would not have decided the Youngstown Case as and when it did if Harry Truman were not a lame duck; and I stand by that remark, drawing some comfort from the recollection that my position is supported not only by Corwin himself ("A Judicial Brick without Straw") but also by such an independent and nonbehavioral public lawman as John Roche—both Roche and I have articles, contemporary with the decision, in *Western Political Quarterly*, and only a couple of years later Roche published a more general theoretical statement in the *APSR* ("Judicial Self-Restraint," vol. 49, pp. 762-72) while I ventured a more general and historically-oriented empirical statement in Chapter 8 of my *The Presidency in the Courts*. It is true that Corwin, Roche, and I may all be wrong; but without Krislov's having first taken our arguments into consideration, I find it hard to accept as responsible criticism his own cavalier statement that "It is almost meaningless or simply wrong" for us all to have reached the considered judgments that, quite independently, we did reach about the Steel Seizure Case. Krislov may feel that this is an instance of "inadequate footnoting"; but he will discover, I trust, that in annotating lectures for publication (as distinguished from writing a textbook) one must assume that the professional literature of a generation ago

eventually passes into the public intellectual domain.

GLENDON SCHUBERT

University of Hawaii

TO THE EDITOR:

Since I regard Professor Schubert's description of my review as "not unfavorable" as a considerable understatement, I am both surprised and genuinely sorry that offense has been taken where none was intended. The cited two feeble attempts at playfulness arose out of a desire to satisfy puzzlement, but were in no event slurs at nor disparagements of anyone. On these personal matters, I am happy to accept Glendon Schubert's factual corrections. Although I could cavil at his rendering of his rather complex relationship with Hawaii, my point would have been the same had the sentence read "colleagues at Osgoode Hall" or indeed "in the profession." That—the serious point—is, I suppose, conceded in avoidance; the conceptual category of "radical justices" is not obviously applicable to anyone on or likely to be on the Supreme Court, and is therefore curiously contentless and not parallel to the other categories developed in the work.

As to the substantive matters Schubert deals with after rising to personal privilege, I find my original positions at least arguable. My "greatest quibble" was explicitly not with the specific comparison of the liberalism of Clarke and Cardozo, but with such trans-historical comparisons without explanation of how the conclusion was reached. I am not sure scalar distance from Brandeis, based upon cases in the 1910s and '20s in one instance, and the '30s in the other, warrants the conclusion discussed; I am positive such conclusionary statements should have been buttressed in the book by their evidentiary base, however strong or fragile.

As to the *Youngstown* issue, my comment was on the sentence actually printed, not what Professor Schubert tells us he meant, let alone his incorporation-by-reference of the authority of Roche and Corwin. That delphic sentence—some might find it sententious, even—reads:

Evidently, it could not have come at any other time than when it did, during the Vinson period and with an outgoing presidency at the nadir of its authority. (page 41)

This—the total discussion of the issues raised in his letter—not only fails to convey in any precise way what he meant, but also is destructive of the type of analytic thought he is arguing for in the book and the letter. By confounding sufficient and necessary conditions, it represents, in Hegelian garb, an unwarranted overgeneralization (Schubert now suggests only, at most, that the decision would not have occurred with a non-lame duck, and/or popular president).

In a book which pleads for and largely succeeds in developing systematic relationships, such assertions as the above two seemed out of place, whereas they might have been overlooked or accepted in a more routine work. It would, indeed, not be unreasonable for Professor Schubert to regard these criticisms as a compliment on the general level of effectiveness characterizing the volume.

To put matters in better perspective, let me add that these issues constitute a tiny portion of the book and considerably less than a major part of the review. The book is a fine and rewarding one, and the review, for the most part, deals with more important questions than are covered in these letters.

SAMUEL KRISLOV

University of Minnesota

EDITORIAL COMMENT

Volume Sixty-six. A while ago (June 1972, pp. 585-586) we published a short, snappy analysis of the contents of Volume 65 of the *Review*. Now we can do the same for Volume 66. In keeping with trends in the profession, we are pleased to be able to refer readers to Table 1, which gives 16 sets of figures for Volume 65, and includes lots of blank space for those who want to fill in comparable figures for Volume 66. Our comments will adhere only in part to the rather arbitrary format provided by the table, and in part we will wander off in other directions.

For example, we have noticed a bump in the number of articles devoted to Congress: six if you count articles on congressional apportionment, four otherwise. This is very suspicious; we all know that in a previous life the managing editor used to lurk around Capitol Hill. How ironic that none of the congressional articles are by associates, former associates, colleagues, former colleagues, students or former students of the managing editor. For some years now congressional scholars have been making the claim that theirs is a burgeoning field of study. Perhaps the word is beginning to reach the *Review*, even without the benefit of the managing editor's encouragement.

Nothing we have printed in the *APSR* in our time has earned us more favorable comment than the long symposium in Volume 66 on positivism and historicism. Why? We do not know. Is it an occupational disease of political scientists for us to be what David Braybrooke might call frustrated paraphilosophers? Is there something about the din of battle that pulls us out of the woodwork, rivets our attention, and sets the applause meter skyrocketing? Of course it is possible that no motives as base as these are involved, and that it is the substance of the issues debated in the symposium that really interested readers. In that case, it would appear that for many of us, there is something intriguing about cosmic hiccoughs emanating from far parts of our solar system, which may or may not make waves closer to home.

At any rate, the favorable response to this particular symposium gives us courage to persist in our occasional use of this device to stimulate thought and scholarship. The growth of this format reflects, we suppose, the managing editor's view that when we can get it, good conversation is better than a good monologue. Mounting symposiums is risky work, since it entails finding commentators who are not only appropriate but prompt, making certain that contributors get timely opportunities to see the

materials they are supposed to comment on, assembling all the parts of the package in the proper order, and getting it into print seasonably.

We think all this effort is worthwhile in several circumstances. Occasionally we accept an article that criticizes, continues, or amplifies the work of another scholar in such a way as to create a presumption of scholarly privilege. Once in a while we accept an article that divides our readers in a fashion that suggests they had better have it out in public. In these cases, we try to provide a forum for the direct exchange of ideas on controverted topics. We think that direct exchange leads to less misunderstanding in a scholarly community over the long run than is the case when scholarly discourse develops, as sometimes happens, into piecemeal guerrilla warfare.

A third occasion for a symposium comes about when articles arrive from disparate sources dealing with similar or closely related problems, or with a topic that seems especially timely or urgent or newly emergent or crucial from the standpoint either of the theory or the practice of politics. In this case the symposium format may underline a theme, or announce a new community of interest. We do not do this third type of symposium as much as we could, if we knew more, or knew better how to predict or to identify important new areas of study.

Another continuing feature of the *Review* which made its debut in Volume 66 is the review essay. We think this is an appropriate vehicle for the stimulation of thought about major figures in political science, for the presentation of a synoptic view of reasonably well-bounded fields of study, or for the extended analysis of a cluster of books. No doubt other rationalizations will occur as time goes on.

A stray thought tugs at our mind as we contemplate the uses of the review essay. As our discipline flourishes and proliferates, it is bound to be increasingly true that in any given issue of the *APSR*, fewer and fewer major articles will address all our readers. Specialized knowledge, to which we are willingly and unavoidably committed, means that any one reader will as time goes on find the front of the book harder to read, and less of it directly engaging to his interests—whatever they are.

This creates a special opportunity for features in the back of the book: reviews, review essays, correspondence, editorial comments. Here, surely, we should all feel comfortably at home. If the front of the *Review* is a series of boutiques, the back should resemble an old-

Table 1. Some Comparisons

	Volume 65 (1971)	Volume 66 (1972)
1. Articles	46	42
2. Review Essays	0	3
3. Comments and Rejoinders	10	10
4. Political Psychology, Mass Voting, Attitudes, Participation	19	
5. Articles Having At Least One Table or Figure	35	
6. Presidential Addresses	1	1
7. Focus on U.S.	20	
8. Data About One or More Foreign Countries	12	
9. Formal Theory	13	
10. Public Law	2	
11. Public Administration and Organization Theory	1	
12. Policy Analysis	3	
13. Political Philosophy	3	
14. Elite Politics	2	
15. International Politics	2	
16. Congress	1	6

fashioned general store, where one shopper in search of a few yards of muslin can pause and exchange a word with another who wants a keg of nails.

Underlying this conception of the back of the *Review* is of course the notion that enough of us still have a need or at least a desire to cultivate our peripheral vision, that there are some things worth sharing that we can all share, that specialization, while important and probably indispensable for the increase of knowledge, is not all-important.

Having Fun? Colleagues occasionally ask the managing editor whether he enjoys the job. The fact is, like nearly everything else when seen from up close, there are joys and sorrows. Things to like about the job: The secretarial help. The chance to talk on the telephone. Advance looks at what interesting people are writing. The unmerited high status in the profession. Things not to like: Getting nasty letters. Having to say no. The high visibility in the profession that occasionally draws hostility.

The job description for the managing editor is somewhat deceptive. There are the daily chores that we all know about: answering the mail, assigning referees to manuscripts, reading evaluations, and deciding whether or not to publish. These activities can be worked without strain into even the busiest day provided good secretarial help is available and provided also

that they are done on a daily basis and are not permitted to pile up.

The imposition of a daily routine on the managing editor probably does levy a hidden toll insofar as it interrupts the longer cycles associated with the production of serious scholarship. But that is a risk that most managing editors knowingly face when they take on the job.

In addition to these daily tasks, however, are activities that are more time and energy consuming than many people realize. The managing editor is a constitutional officer of the Association. This makes him a voting member of the Council, and takes him to the three or more Council meetings that are held each year. As the person responsible for the source of the largest part of the Association's income—and for a major fraction of its expenditures as well—the managing editor ought no doubt to participate as a member of the Council. At a minimum this arrangement gives other Council members a chance to complain about the contents of the *Review*.

A certain number of queries and invitations also come into the office, we assume *ex officio*. Our judgment is that we ought to respond affirmatively to a decent percentage of these, so as to fulfill the main long-range duty of the managing editor, which is to try to stay abreast of what's happening in the discipline. In a discipline as variegated and amorphous as political science, this is not an easy task. No single political scientist, no matter how well acquainted, can afford to rely for long on the network of colleagues he has when he assumes the managing editorship. So part of our job is to attend to the continuous renovation and renewal of the managing editor's knowledge of the profession.

These long-range items are not part of the conventional job description for the managing editorship. Nevertheless, they constitute real obligations, they take time, and they are important sources of the drudgery and fascination of the job.

Articles Accepted for Future Publication

Paul R. Abramson, Michigan State University, "Generational Change in American Electoral Behavior"

Peter H. Aranson, Georgia Institute of Technology, Melvin J. Hinich, and Peter C. Ordeshook, Carnegie-Mellon University, "Election Goals and Strategies: Equivalent and Non-equivalent Candidate Objectives"

Robert Axelrod, University of California, Berkeley, "Schema Theory: An Information Processing Model of Perception and Cognition"

Harry W. Blair, Bucknell University, "Minority

- Electoral Politics in a North Indian State: Aggregate Data Analysis and the Muslim Community in Bihar, 1952-1972"
- Steven J. Brams, New York University and Morton D. Davis, City College of New York, "The 3/2s Rule in Presidential Campaigning"
- Christopher Bruell, Boston College, "Thucydides' View of Athenian Imperialism"
- Walter Dean Burnham, Massachusetts Institute of Technology, "Theory and Voting Research: Some Reflections on Converse's 'Change in the American Electorate' "
- Blair Campbell, University of California, Los Angeles, "Helvétius and the Roots of the 'Closed' Society"
- William Cavala, University of California, Berkeley, "Changing the Rules Changes the Outcome: Party Reform and the 1972 California Delegation to the Democratic National Convention"
- Harry Eckstein, Princeton University, "Authority Patterns: A Structural Basis for Political Inquiry"
- Peter Eisinger, University of Wisconsin, Madison, "Racial Differences in Protest Participation"
- David J. Elkins, University of British Columbia, "The Measurement of Party Competition in Multi-Party Systems"
- John A. Ferejohn, California Institute of Technology, "The Paradox of Not Voting: A Decision Theoretic Analysis"
- Peter C. Fishburn, Pennsylvania State University, "Paradoxes of Voting"
- Ronald P. Formisano, Clark University, "Deferential-Participant Politics: U.S. Political Culture, 1789-1840"
- Virginia Gray, University of Minnesota, "Innovation in the States: A Diffusion Study"
- A. James Gregor, University of California, Berkeley, "On 'Understanding' Fascism: A Review of Some Contemporary Literature"
- Nobutaka Ike, Stanford University, "Economic Growth and Intergenerational Change in Japan"
- Donald B. Johnson and James R. Gibson, University of Iowa, "The Divisive Primary Revisited: Party Activists in Iowa"
- Kenneth Jowitt, University of California, Berkeley, "An Organizational Approach to the Study of Political Culture in Marxist-Leninist Systems"
- Stanley Kelley, Jr., Princeton University and Thad W. Mirer, University of Wisconsin, "The Simple Act of Voting"
- Warren Lee Kostroski, Wittenberg University, "Party and Incumbency in Postwar Senate Elections: Trends, Patterns and Models"
- Eugene B. McGregor, Jr., University of Maryland, "Politics and the Career Mobility of Bureaucrats"
- Arthur H. Miller, Ohio State University, "Political Issues and Trust in Government: 1964-1970"
- James T. Murphy, Wesleyan University, "Party and Pork: Party Conflict and Cooperation in House Public Works Committee Decision Making"
- John C. Pierce, Washington State University, and Douglas D. Rose, Tulane University, "Nonattitudes and American Public Opinion: The Examination of a Thesis"
- David Ray, Stanford University, "Membership Stability in Three State Legislatures: 1893-1969"
- William H. Riker, University of Rochester, "The Paradox of Vote Trading"
- Douglas D. Rose, Tulane University, "National and Local Forces in State Politics: The implications of Multi-level Policy Analysis"
- Lester M. Salamon, Vanderbilt University, and Stephen Van Evera, University of California, Berkeley, "Fear, Apathy, and Discrimination: A Test of Three Explanations of Political Participation Among the Poor"
- Stephen G. Salkever, Bryn Mawr College, "Virtue, Obligation and Politics"
- Kenneth A. Shepsle, Washington University, "On the Size of Winning Coalitions"
- Brian Silver, Florida State University, "Social Mobilization and the Russification of Soviet Nationalities"
- Arthur G. Stevens, Jr., University of Virginia, Arthur H. Miller, Ohio State University and Thomas E. Mann, American Political Science Association, "Mobilization of Liberal Strength in the House, 1955-1970: The Democratic Study Group"
- Timothy A. Tilton, Indiana University, "The Social Origins of Liberal Democracy: The Swedish Case"
- Glenn Tinder, University of Massachusetts, Boston, "Beyond Tragedy: The Idea of Civility"
- Vernon Van Dyke, University of Iowa, "Human Rights Without Discrimination"
- Herbert Weisberg, University of Michigan, "Models of Statistical Relationship"
- Roger E. Wyman, Rutgers University, "Middle-Class Voters and Progressive Reform: The Conflict of Class and Culture"
- William Zimmerman, University of Michigan, "Issue Area and Foreign Policy Processes: A Research Note in Search of a General Theory"

Man's Tools and Man's Choices: The Confrontation of Technology and Political Science*

VICTOR C. FERKISS

Georgetown University

Technology is such a pervasive element of contemporary human life that we usually are no more conscious of it than of the air we breathe. Technology is nothing new. From the time the first proto-man started using sharp rocks for weapons or tools, technology has reflected and shaped society's ideas and institutions. What is new is the dominant role of technology in modern society. If one accepts the definition of technology as "the institutionalization of utilitarian norms," centering on "the application of rational principles to the control and reordering of space, matter and human beings,"¹ one recognizes that the rise of technology is virtually synonymous with what is often termed development or modernization. Technology is the application of the principles of science—knowledge for its own sake—to human concerns. Because of the ubiquity of technology in modern society and its long history as a force in human life most social scientists have tended to take technology for granted, dealing with it as one of many variables which affect or are affected by human behavior, but not as

something which might be central to their concerns. Within political science, for instance, only students of international relations have paid much attention to technology as such, treating it as one of many determinants of military power; only since the advent of atomic weapons has it been suggested that particular technologies might actually be at the core of the power relationship, as when it is said that no nation can rationally use nuclear war to advance its political objectives since no nation can "win" such a war.

But despite the relatively little attention given technology by academic social science and, until recently, even by professional historians, the last decade has seen a burgeoning of interest in the social implications of contemporary technology on the part of the public and of the general intellectual community. Discussion about the impact of the machine on human life and values goes back to the nineteenth century, but recent scientific and technological advances have given new life to old concerns and fears. To older debates about the impact of industrialism on society have been added newer, usually fearful, queries about the effects of television, the computer, and new discoveries in the life sciences. Rising concern with environmental degradation has promoted widespread questioning of the values underlying contemporary industrial society, an extreme form of this questioning being exemplified by elements of the "counter culture" movement.

Perhaps the purest distillation of the mood of fear and disillusion infecting many in modern Western society is to be found in the work of Jacques Ellul, whose argument that in our era the norms of technological growth and efficiency must necessarily reign supreme, finds widespread (if far from universal) acceptance among intellectuals. Less pessimistic in tone but virtually as deterministic in its assumptions is the contention of such social scientists as Daniel Bell and Zbigniew Brzezinski that we are moving into a "postindustrial" or "technetronic" age in which technology has become the major force shaping social dynamics and institutions.

Such assertions about the pervasiveness of technology in human life as are made by Ellul, Bell, Brzezinski and the rest strike at the very

* William Braden, *The Age of Aquarius: Technology and the Cultural Revolution* (Chicago: Quadrangle Books, 1970. Pp. 306. \$7.95.)

Nigel Calder, *Technopolis: Social Control of the Uses of Science* (New York: Simon and Shuster, 1970. Pp. 376. \$7.50.)

William G. Carleton, *Technology and Humanism: Some Exploratory Essays for Our Time* (Nashville: Vanderbilt University Press, 1970. Pp. 300. \$12.50.)

Martin Greenberger, ed., *Computers, Communications and the Public Interest* (Baltimore: The Johns Hopkins Press, 1971. Pp. 315. \$12.50.)

William Kuhns, *The Post-Industrial Prophets: Interpretations of Technology* (New York: Weybright and Talley. Pp. 208. \$6.95.)

Emmanuel Mesthene, *Technological Change: Its Impact on Man and Society*. Harvard Studies in Science and Technology. (Cambridge, Mass.: Harvard University Press, 1970. Pp. 127. \$4.95.)

Herbert Muller, *The Children of Frankenstein: A Primer on Modern Technology and Human Values* (Bloomington: Indiana University Press, 1970. Pp. 431. \$10.00.)

Eugene S. Schwartz, *Overskill: The Decline of Technology in Modern Civilization* (Chicago: Quadrangle Books, 1971. Pp. 338. \$8.95.)

Alan F. Westin, ed., *Information Technology in a Democracy*. Harvard Studies in Science and Technology. (Cambridge: Harvard University Press, 1971. Pp. 499. \$12.50.)

¹ Robert A. Nisbet, *The Social Bond: An Introduction to the Study of Society* (New York: Alfred A. Knopf, 1970), pp. 244-245.

existence of political science as an autonomous discipline. Whatever formula one prefers to use to define its subject matter—power, decision making, or the authoritative allocation of values—political science necessarily assumes that human choice (essentially, conscious human choice) is at the heart of the historical process. It would be possible of course to conceive of and try to construct a science of politics which was deterministic in its premises, on the model of classical physics; indeed, such an enterprise is not without its adherents. But such a science would not only be more deterministic than contemporary political scientists would be willing to accept, it would be reductionist as well. Reductionist theories which make political behavior a function of some other kind of behavior or which deny the ability of men individually or at least collectively to decide on how the future will be shaped (within whatever physical or logical limitations may be postulated) make the study of politics a minor branch of something else. Worse yet, they strike at the human self-image as embodied in politics as such. The reduction of chemistry or pharmacology to physics might deflate the egos of practitioners of the former disciplines, but the chemical compounds themselves will not care. Making political institutions and behavior wholly dependent on technological factors not only destroys the autonomy of political scientists as scholars but of men as citizens.

Evident, though largely implicit, in the rapidly growing body of literature fueling the society-technology debate are three possible approaches to the relationship of technology to politics. The first of these is *technological determinism*, which awards to technology the status of a completely independent variable. This viewpoint is embodied in the belief that whatever man can do he will do. Politics and culture are relegated thereby to the role of purely dependent variables. In technological determinism not only is technology an uncaused, unwilled cause which causes other things, but technological change is the sole and irresistible cause of all changes in all other fields of human activity. Under technological determinism all the other sciences which deal with human life are reduced to describing particular classes of events, all wholly determined by technology.

A second approach is to treat *technology as a conditioning variable*, one among many factors exogenous in its origins to the political process having necessary, significant, and definable effects on political ideas, institutions, and actions, like those historical and social factors (geography, genetic inheritance, population

size, etc.) which are traditionally discussed in the first chapters of treatments of particular political systems in texts on comparative government. Technology as a conditioning variable, however, is often treated more deterministically than other such variables, because it is usually regarded as monolithic in its nature and social consequences and unidirectional in its development, impinging on all political systems in essentially the same way, if in differing degrees. Basic to this approach is the tacit assumption that, although its effects may be limited and may be mediated by other social forces, technology exists independently of any social decisions about its nature, and that its second-order consequences are universal and irresistible. The literature on political development or modernization especially exemplifies this acceptance of technology as a conditioning variable.

The third approach to the relationship between technology and politics postulates a *technology-politics interaction system* wherein technology and politics are mutually interacting variables, with technology, once existing, conditioning political life, while politics (human social decisions) condition the nature and extent of a society's technology. Studies of the history of science policy in modern nations, for instance, make it clear that Research and Development programs are not the products of a social stork but are the result of human decisions, and that, for financial reasons if for no other, not everything that can be done will be done, at least not at any given time or in any given context. By restoring technology to its place as a human creation rather than a *deus ex machina*, this approach forces us to focus on the public policy implications of scientific and technological possibilities.

The attitudes of any student of the technology-politics relationship can be classified as exemplifying one (or sometimes more than one) of these three approaches.

Technological Determinism

Curiously, one of the most extreme recent examples of an explicit willingness to swallow whole the position of technological determinism is to be found in a collection of essays by an author close to academic political science, William G. Carleton, Professor Emeritus of History and Political Science at the University of Florida. *Technology and Humanism* is something of a disappointment because few of the essays in the book (originally published in the *Antioch Review*, the *Yale Review*, and other journals) come to grips with any of the

basic issues concerning the role of technology in contemporary society.

One essay which does deal directly with the power of technology, however, is "The Century of Technocracy," first published in 1965, which begins with the sweeping statement: "The overriding contribution of the twentieth century to history is technocracy: the technicalized, cybernetic, computerized society increasingly run by scientists, engineers and technicians. This society is drastically changing man's institutions and transforming his behavior and basic values" (p. 255). Taken at face value, what Carleton is saying here is that political science as he and we have known it is obsolete and must be wholly restructured around the technological variable. But, once having said this, he goes on almost as if nothing has happened. Scientists, he asserts, are becoming dominant everywhere; ideology is in decline and will be replaced as a savior in people's minds by machine technology. But Carleton fudges on the issue of determinism. Apparently, though we cannot control technological growth and change, we can tame it. This seems to be the implication of his exhortations not to let the machines control us but to control the machines ourselves for humanistic ends by means of the democratic process. While it might be unfair to accuse Carleton of Pollyannaism, the mildness of the bulk of his remarks, both substantively and rhetorically, contrasts strangely with the shocking implications of his general statements about the nature of the contemporary political scene.

Though William Kuhns, Professor of Communications at Loyola University in New Orleans, adopts the stance of detached expositor, his book, *The Post-Industrial Prophets: Interpretations of Technology* is a valuable introduction to the ideas of the advocates of technological determinism. His subjects include Lewis Mumford, Siegfried Giedion, Jacques Ellul, Harold Adams Innis, Marshall McLuhan, Norbert Wiener, and Buckminster Fuller. Mumford is not, of course, a determinist, and his polemic against the consequences of contemporary technology seems sometimes to be based on an excessive optimism about man's ability to control his technology. Neither is Giedion, whose primary interest is in documenting technology's effects. Nor is Wiener, whose concern about the use man makes of the computer implies the existence of options. But the figures to whom Kuhns devotes most of his balanced, lucid, and thoughtful book are determinists, and Kuhns's own humanistic concern with human freedom illuminates the implications as well as the origins of their views.

Kuhns's division of the writers he discusses into schools of thought reflects his particular preoccupations as one of McLuhan's former associates. He argues that writers with a nineteenth-century bent, such as Mumford, Giedion and, essentially, Ellul, identify technology with the "work-performing, energy-transforming machine," while we are now learning "to identify technology with media and other forms of information control" (p. 8). This is a useful distinction, and insofar as it is a valid one it increases the problem of maintaining the autonomy of political science as a discipline. So-called "cybernetic" or communications models of politics are already claiming to give us superior insights into the political process. To the extent that they provide us with comprehensive descriptions of political action, and to the extent that communications patterns are technologically conditioned or determined, to that extent politics itself becomes technologically conditioned or determined. But while Kuhns is not concerned with politics as a discipline, he is concerned with the autonomy of man as a being; and he is optimistic. "While the mechanized conception of technology led almost inevitably to a polarization of man and machine, the media or information-control interpretation leads to a conception of organic continuity between man and his technologies" (p. 9). Put another way, if the old technology presented the specter of man's becoming dependent or subordinate to things, the new technology liberates him by making it possible for him to further extend his intellectual and spiritual range.

Yet whatever the normative implications of their work, the communications theorists to whom Kuhns feels closest do make politics a dependent variable of communications. Harold Innis, a distinguished Canadian economist who, unlike his disciple McLuhan, deals with historical change in an orderly fashion, has focused on the rise and fall of power structures. He argues that the media in use in any society (writing, printing, oral communications, etc.) are the independent variables and that when a new medium infiltrates an existing political system it then slowly subverts it. Since Innis's theory is essentially a kind of cybernetic determinism, it forces him to regard such things as censorship and information monopolies as the true sources of political power. McLuhan's divergence from Innis actually results from his extension of Innis's ideas. Less concerned with social structure than his master, he treats the media as not simply affecting the form of a society through changing the society's "temporal or spatial biases," but as having a direct influence on the

human sensorium. "The medium is the message," his now famous catch-phrase, asserts that how one is communicated with crucially affects one's perception of the world—and one's personality itself—and does it directly, without the mediations of any other social force.

Buckminster Fuller is a prophet of technological determinism whose position is perhaps even more radical than that of the communications theorists, since for him even communication—to say nothing of politics—is simply an epiphenomenon of energy. For Fuller, the proper level of social analysis is the energy-man-object ecology of the planet as a whole. He is a systems-oriented thinker who has a utopian belief in the fundamental harmony of the world which is obscured and distorted by politics; this harmony, however, could be institutionalized, he believes, in a new organization of the whole based on economics in which energy and mobility patterns would be the fundamental variables and which would utilize the computer as a tool for systems analysis and control. Political scientists would do well to read Fuller; he has a great appeal to certain of the young who are dissatisfied with the present but cannot accept the negativism of Mumford or the pessimism of Ellul and find the essentially optimistic McLuhan limited in range. The new scientific utopianism which embraces thinkers as diverse as Fuller and B. F. Skinner may present a more potent challenge to political science than does traditional utopianism because it claims to be based on the data of the "hard" sciences which so many contemporary political scientists have chosen to revere.

Actually even Innis and McLuhan have a utopian streak, and Kuhns shares their normative bias. They divide cultures into spatial and temporal ones, the former based on writing and the latter on speech or (in McLuhan's paradoxical and debatable formulation) on visual media such as television. For this school of thought, oral tradition and sensibility "are interpersonal rather than impersonal." They tend toward "immediate and local forms of authority over distant, centralized forms," encourage "dialogue rather than a monopoly of opinion" and make learning "an involving and creating activity rather than a simple experience of information transmission" (p. 167). Whether the belief in the salvation of democracy through modern communications technology is valid (and Kuhns has his reservations) is a substantive question which is secondary to the assumptions which underlie it. Whether one answers yes or no, to ask the question is to assume a

radical dependency of politics upon the media which is by no means self-evident. Also, Kuhns and his friends never really ask to what extent human political choice can determine the nature or the content of the media of a society, apparently assuming either that political decisions cannot influence them or at least that they do not do so.

Technology as a Conditioning Variable

Many of those who write about society and technology—perhaps most of them—are ambivalent about whether technology wholly determines our lives or merely affects them, and, if the latter, how much these effects can be controlled by human volition. This is to be expected. Unless one denies that changes in technology can really change anything, and insists that it is all simply a matter of old wine and new bottles, obviously technology is important. Yet to consider it all-important is to make it our master, and few will admit to being slaves if there is any basis on which they can deny it. This ambivalence—and sometimes downright confusion—is reflected in both the analytical and the descriptive treatments, although it is perhaps more pervasive in the latter, where it is easier to avoid a clear-cut resolution of the issue.

Herbert Muller, Distinguished Professor of English and Government at Indiana University, is almost explicit about his ambivalence in *The Children of Frankenstein: A Primer on Modern Technology and Human Values*. Muller takes the position that technology is just another factor in our lives, an important one but not so important as to change the basic rules of the social game: men are still men. But at the same time he asserts that technology has outmoded our eighteenth-century constitution, designed for the governance of an agricultural society, and he accepts the thesis that we live in a postindustrial society. In contrast to Carleton, he claims that we are in no danger of domination by a scientific elite. Technology can be used for good or evil; it is up to us. Yet he is uncertain about whether our system can be inventive enough to deal with the social problems posed by technology. Muller's work is rambling, bland, and often irritatingly avuncular in tone, but it is also balanced and remarkably comprehensive. Though the author embraces aspects of all three approaches to the man-technology issue, basically he seems to regard technology as a conditioning variable. Despite its title this is a book that even the most timorous can read immediately before retiring, possibly even with soporific effect.

Smaller in size but more intellectually ambitious is *Technological Change: Its Impact on Man and Society* by Emmanuel Mesthene, late director of the now defunct Harvard University Program on Technology and Society. Unfortunately, Mesthene tries so hard to be judicious as well as comprehensive that he leaves us confused about the extent to which technology is to be regarded as an autonomous force in society. But, as his title indicates, his main thrust is to accept technology as a given and to concern himself primarily with its consequences. Though he explicitly states that "technology is not independent of the society in which it develops and flourishes" (pp. 20), and that it is not "a somehow autonomous force that develops according to its own internal laws and lets the consequences fall where they may" (p. 20), the "sounder" view with which he associates himself is that social institutions play a role in "fostering" technological development and "mediating between technology and its effects" (p. 20). This is obviously quite different from arguing that we can control, shape, or reject technology in such a fashion as to avoid these effects.

Mesthene is deeply concerned with values, yet here too his ambivalence about the autonomy of technology is manifest. He argues that while "technology creates new possibilities [it] leaves their disposition uncertain" (p. 60). Thus technology is value neutral; the tools are given us, we can do good or evil with them—a knife can cut meat, excise a gangrenous limb, or murder a fellow human (whether such tools as H-bombs are good for anything is an issue Mesthene does not discuss). Yet he admits that not only do technologies have necessary consequences, about which differing value judgments can be made, but that changes in technologies can effect changes in the values of a society, which comes close to saying that technology is not value-neutral but self-validating. This implication in turn is only one step away from the view of Herbert Marcuse and his followers that in modern society technology has itself become an ideology in the strict sense of the word.

When he discusses the impact of technology on political life as such, Mesthene in effect treats it as a conditioning variable, the effects of which he approves. Technological advance, for instance, leads to "the enhancement of the public sphere" (p. 63) as a result of centralization and the enlargement of the effective social unit (both geographically and socially). Mesthene believes that technology can create the conditions for more direct democracy by making information more accessible to the public. We

can dismiss the bogeyman of Ellul and others who see technology as necessarily submerging the individual, he holds, since technology, all things considered, will lead to something not unlike McLuhan's global village, save that for Mesthene, a former teacher of Greek philosophy, the ideal of the life lived in public is Periclean Athens. Although Mesthene has some reservations about direct democracy in terms of the ability of the general public to make operational decisions, he, unlike Kuhns, is not concerned about what might be the fate of the individual in such an environment. But, then, individualism never was a value for classical Greek philosophy.

Less deterministic, either explicitly or implicitly, are recent works by two authors who deal with radically different aspects of the impact of technology on society. William Braden, a knowledgeable, sophisticated Chicago newspaperman, is concerned about the impact of technology on the values of youth in *The Age of Aquarius: Technology and the Cultural Revolution*; while Eugene S. Schwartz, identified by his publisher as a "former senior scientist specializing in information sciences at the Illinois Institute of Technology Research Institute" in his book *Overskill: The Decline of Technology in Modern Civilization* discusses the physical impact of technology upon the human race. Both Braden and Schwartz could be classified among those treating technology as a conditioning variable in politics, but, because Schwartz deals in polemic and advocacy (though his work is heavily documented), while Braden is essentially a reporter and synthesizer, it is easier to know where Schwartz stands than where Braden does.

The question Braden asks in *The Age of Aquarius* is why what he assumes to be large segments of educated young people are in revolt against modern American society; (the book was written at the height of the perhaps short-lived "youth culture"). He therefore focuses on the issue of what relative weight should be assigned to problems of personal psychological adjustment as against objective social conditions in triggering youthful dissatisfactions. His extensive reading and his lengthy interviews with such persons as Bruno Bettelheim, Edward Hall, Richard Flacks, and Christopher Lasch seem to have convinced him that contemporary America is in the midst of a cultural crisis, stemming from a humanistic revolt against technology and its results. Paradoxically, technology is not only the enemy; it is also the precondition of the revolt; since, as Braden points out, adolescence as a distinctive

sociopsychological status is the product of an affluence dependent on a high-technology economy.

It is often difficult to disentangle Braden's own views from those of the writers and interviewees he quotes or refers to (without, alas, citation). His own judgment about the extent to which man can resist or control technology is unclear. He comes close to embracing the Muller-Mesthene position that technology is something which is here and which has both good and evil consequences and that what needs to be done is to accept the former and reject the latter—a formula as unexceptionable as it is difficult to operationalize. But despite its lack of intellectual focus, *The Age of Aquarius* is a rich and rewarding meal for anyone interested in the interrelationship of culture, society, and technology. Time alone will tell whether the title of the book is a misnomer, whether it should not instead have been *The Year of Aquarius*.

For Schwartz, technology is not evil per se, though some technologies may be. The real problem is that if technology is permitted to grow at its present exponential rate, humanity is physically doomed. We can and we must work, he says, toward the creation of a less efficient society. Growing skepticism about the ability of technology to sustain ever larger populations, especially without insupportable side effects, is becoming increasingly common among social scientists as they begin to assimilate the ideas of the ecologists and population specialists. Increasing attention is being paid to the gloomy extrapolations of Jay Forrester and his Systems Dynamics Group at M.I.T. But Schwartz goes them one better in pessimism. Forrester and his colleagues believe that within technology itself lies the source of our salvation and that we can rely on the computer to do the multivariate analyses and produce the systems designs necessary for the kind of planning we will need if we are to avoid catastrophe. Schwartz denies that technology can save us from the dangers technology creates. Chicken Little is right; or, as Schwartz puts it, "the residue of unsolved problems arising from a multiplicity of quasi-solutions converges to a point where no techno-social solution is possible" (p. 76).

But even though Schwartz assigns to technology an overwhelming and, in effect, monocausal role in the doom of the world he is not a determinist, because he believes that somehow, even at this late date, man can save himself by an act of will. Population must be stabilized and resource utilization must be drastically cut

back; our standard of living must be greatly reduced and men must become more self-sufficient than the "dependent urban dweller of today" (p. 297). Essentially, then, Schwartz believes in the primacy of politics over technology, though what form such a politics—such an act of anti-technological choice—would take, he does not even begin to tell us. While *Overskill* is a tract, it is a sophisticated one, and its very extremism makes it especially valuable heuristically to any social scientist who might be inclined to take the present physical context of the political system for granted.

Technological-Political Interaction

Of all the recent literature which deals with the interdependence of technology and politics and assumes that man is capable of choice even in dealing with his artifacts, none is more valuable, especially to the newcomer to the field, than Nigel Calder's *Technopolis: Social Control of the Uses of Science*. Calder, a distinguished and prolific British science editor, has written a book which is badly organized both as to data and ideas but is rich in information and insights, and the bibliography though short is excellent. Calder sees both modern technology in general and the second-order effects of particular technologies as radically transforming political ideas and institutions; he also recognizes, however, that our technologies are the result of human choices, especially choices about allocation of the scarce resources upon which technological development depends. Like other perceptive students of technology he sees communications as the most important current interface between technology and politics. Calder argues that a complete two-way information system (he calls such a device the "World Box") could lead either to centralization or to decentralization depending on human decision, and that it could play a vital role in what he regards as the clearly soluble problem of controlling the expert. He speculates that bureaucracy could be abolished, since, once the possession of information becomes more general throughout the society, special expertise (which in his opinion is the fruit not of skill but of secrecy) will become devalued and the functionally necessary roles of control and coordination will be performed by "in and outers" dividing their time between government and the universities or professions, acting always under a keen and watchful public eye.

Though on the technical feasibility of computer planning of world systems Calder is closer to Forrester than to Schwartz, he also recognizes that any increased ability to analyze

and control future world systems will not do away with the need for political decisions about what questions are to be fed into the computer and about what weights should be assigned to particular values. In accordance with his belief that the science-technology issue will dominate the politics of the future, Calder attempts to categorize public opinion in terms of beliefs and attitudes about science and technology. Those who are inclined to let science and technology dominate society he calls the Zealots; those who are more skeptical about the benefits of such an arrangement are the Mugs, a suggestive if hardly definitive typology. Unfortunately he offers no data to indicate how the public lines up at present in terms of his categories. He himself, however, makes no secret of his hope and conviction that the Mugs will win.

The most clearcut recognition of the technology-politics relationship as one of mutual causation is usually to be found in case studies of particular problems, where the complex intertwining of cause and effect, necessities and alternatives becomes evident from a consideration of the empirical data and the political and social history of a particular problem. Two recent studies which focus on information technology are good illustrations. One is *Information Technology in a Democracy*, edited by Alan F. Westin, Professor of Public Law and Government at Columbia, and published under the auspices of the program which had been run by Mesthene at Harvard. The other is *Computers, Communications and the Public Interest*, the report of a series of symposia held under the aegis of Johns Hopkins University and the Brookings Institution and edited by Martin Greenberger, Professor of Computer Sciences at Johns Hopkins. Both deal with a number of subtopics but cannot avoid tackling three substantive questions which point up some of the complex interactions between political choice and technologically conditioned human powers.

The first of these is the problem of civil liberties created by the ability of new forms of machine technology to maintain virtually absolute surveillance over the individual, a capacity which will be enhanced if and when (possibly quite soon) personal checks are abolished and we move to an economy based wholly on the credit card. The second is the problem of political control presented by total information systems similar to Calder's "World Box" which would make it possible for citizens to maintain surveillance of their government and would restore direct democracy though push-button electronic voting devices permitting expression of opinion on the issues of the day; alterna-

tively, of course, such a system could lead to the totalitarian world envisioned in 1984. Finally, there is the question of how societal values can be implemented more efficiently by combining total information technology with systems analysis. Could such a combination enable the world to deal effectively with the vastly complex problems of an increasingly interdependent technologically based society—problems such as urban decay, undesired international conflict, pollution and starvation, whose causes, consequences and remedies perhaps only the computer can fully analyze?

As might be expected, different contributors are interested in different aspects of these questions, and the focus ranges from the global to the minute. No consensus emerges from these two volumes. Their value lies in sharpening the reader's wits and increasing his knowledge. Perhaps the most provocative single paper in either volume is that of Anthony Oettinger in the Westin collection, which discusses the importance of the difference between "after the fact" and "real time" information processing and how this affects our ability to control organizations and societies. Socioeconomic planning designed to deal with the problems created by planetary interdependence would require a tremendous amount of information of a kind which is difficult to collect and process on a real-time basis, and the efficiency of any control functions built into such a system would be seriously impaired by such a lag. Oettinger's description of the difficulty of getting necessary information in time to the right people within corporate structures should give pause to those who envision running the world by computer. Even if we could program all our values into such a control system (and this, of course, is primarily a political rather than a technical problem—if Disraeli were alive today he would surely aspire not to write a nation's songs but to program its computers), we would still confront the problem of getting the necessary feedback from the real world into the information machine in time to make useful decisions.

What tentative conclusions about the role of the study of technology in political science and the fate of the study of politics itself can we draw from the works under discussion? The fact that the nearer we get to the mundane details of real life, the greater the apparent role of the political variable suggests that there is something inherently wrong with the concept of technological determinism. The widespread acceptance of the irresistibility and autonomy of technology is itself a force which threatens the autonomy of politics both as a field of study

and as social reality; and since what is taken for granted cannot be effectively refuted, a major task of political science would seem to be the analysis of the reasons for the domination of this idea—or, more properly, this ideology—and its refutation. At the same time, much more needs to be done to create a body of empirical studies of the interface between technology and politics for two reasons: (1) to expand our understanding of the limits of political choice in relation to technological possibilities; and (2) to provide the raw material for a synthetic literature on which to base the study of technology as a crucial variable affecting modern politics.

Because man is a symbolizing animal and sees reality not directly but through structured

perceptions, the study of communications must play an especially important role in any such enterprise. But to make communications the essence of politics would be a mistake. There is reality as well as perception, and cybernetic reductionism is as simplistic and non-productive in the long run as any other kind of reductionism.

Finally, the need to pay more attention generally to technological variables in politics should, by forcing upon us a clearer understanding of the relationship between man's tools and man's symbols, between man's creations and his choices, lead to a clearer understanding of man himself as a social being, which, after all, is the real subject matter of political science.

BOOK REVIEWS

Bach, Beethoven, and Bureaucracy: The Case of the Philadelphia Orchestra. By Edward Arian. (University, Alabama: University of Alabama Press, 1971. Pp. x, 158. \$7.50.)

Edward Arian's *Bach, Beethoven, and Bureaucracy* is a remarkably important work in the best, (and most radical,) tradition of the policy sciences. Arian studies the Philadelphia Orchestra from 1912 to the present from a unique perspective. For twenty years he was a member of the orchestra's string section and active in the musician's union. He has since become a Ph.D. in political science. The core of the book is Arian's argument that the Philadelphia Orchestra has become a large, impersonal, bureaucratized institution which maximizes values other than the pursuit of music for music's own sake.

Arian contends that economic, rational, bureaucratic management of a symphony orchestra has massive artistic, social, and cultural costs, and that these costs overwhelm the benefits of businesslike management. In order to survive, management must rely heavily on private charity, recording royalties, and box office sales. The major cost of relying on private charity to fund the orchestra is that fund raisers and large donors must control the orchestra's Board of Directors. Of the 128 people who served as directors from 1901 to 1969, 84 were listed in the Social Register, and they were predominantly big businessmen, bankers or attorneys. There are no professional musicians on the Board. The Board chooses the music director of the orchestra and essentially controls performing conditions and repertory. Consequently, financial and status concerns overwhelm musical concerns.

Recording royalties are the second major source of financing, accounting for at least 14 per cent of the orchestra's earned income. Marketing considerations, not musical ones, determine what gets recorded. Further, recording repertory determines performing repertory. Choice of repertory becomes a form of free advertising. Rehearsing a work for performance minimizes the time that it takes to record it later in the same week and reduces labor costs. Arian computes that in the 1960-61 and 1961-62 seasons, 44 per cent of the works performed served as an economic aid to recording. He comments, "Since the public buys what it is most familiar with and likes best, there is a strong inclination to cater to mass demand by programming and recording those compositions

with the greatest popular appeal. Conservative audience tastes are at once created, sustained, and catered to, and a circle of conservatism is completed and continues to feed upon itself" (p. 23).

Dependence on the box office has similar effects. Innovative repertory, new music, or any other variation from the standard chestnuts mean empty seats, and the orchestra cannot afford to perform for free in schools, hospitals, poor communities, prisons, or anywhere else.

Arian's second line of argument is more provocative. He contends that the bureaucratization and routinization of art results in alienation from work among people who have chosen a career for its psychological gratifications.

Musicians, although trained to be soloists, are employed as anonymous members of ensembles, with no control over what music they play or over how it is to be interpreted. When it is played well, the rewards go to the conductor or soloist. No one notices the third man in the second violin section. This results not only in boredom and frustration with work, but with hostility toward the conductor, the first chair players, the management of the orchestra, and even the audience. Arian reports that, among orchestral musicians, cardiovascular and gastrointestinal disorders are unusually high. He does not discuss the rumored alcoholism and drug usage among performers, but he does make it clear that a lot of what we take to be unprofessional behavior is frequently a manifestation of alienation from work.

Let me suggest that these conditions are not limited to orchestras. Academicians are severely underrepresented on universities' Boards of Trustees. Organizational and financial considerations determine our course offerings and staffing decisions. Each of us is trained to become President of the APSA and to supervise dissertations, but we end up teaching mass lectures of introductory courses. Anyone who has sat through a faculty meeting or a departmental meeting knows the acrimony and bitterness toward the organization that follows from real or perceived powerlessness and lack of self-fulfillment. Analogous situations develop in hospitals, clinics, and law firms. Arian has studied an orchestra, but the demands of bureaucratic organization and economic pressures seem to have the same impact on artists and professionals that assembly lines have on factory workers. Were these findings generalized to the whole system, we would be forced to reconsider the

entire structure of work life. If the people with the most psychologically rewarding and prestigious jobs are alienated from their work and manifest the normal consequences of such alienation, then the system is in jeopardy.

Arian thinks that federal funding will be the source of ways out of this dilemma. He argues that federal funding of the arts could not have a more conservative impact on the arts than their current dependence on private charity and that private censorship has been greater than governmental censorship. Ultimately, Arian argues, that the solution lies in the complete restructuring of the symphony orchestra. The orchestra must exist to serve music. This means expanding the audience, encouraging the composition and performance of new music, enabling individual musicians to play as soloists and in small ensembles, and so on. All of this can be done, he argues, only with federal funding and with the development of a new breed of arts administrator who cares about the arts, as opposed to being a super salesman.

I wonder if the solution will be so simple. Perhaps the mere perpetuation of large scale organizations, such as the Philadelphia Orchestra, inevitably generates alienation and powerlessness among its members, and conservative policies. I would suggest that in addition to providing federal funding for these organizations, there ought to be artists' subsidies, a form of guaranteed annual income for creative and performing artists. This would free the artist from any form of organizational pressure and would permit the uninhibited pursuit of art. Perhaps this will be followed by scholars' subsidies.

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Thomas Jefferson as Social Scientist. By C. Randolph Benson. (Cranberry, N.J.: Fairleigh Dickinson University Press, 1971. 333 pp. \$10.00.)

Thomas Jefferson: A Well-Tempered Mind. By Carl Binger. (New York: W. W. Norton & Co., 1970. Pp. 209. \$6.95.)

Liberty and science are the two fundamental principles of liberalism, in themselves separate and diverse, which liberalism must make inseparable. On the one hand, liberation of the body's appetites from natural fears and social conventions is dignified by the accompanying, progressive emancipation of the mind from tradition and falsehood; on the other, this liberation of the mind is justified and supported by the benefits it brings to the many who participate in it only imperfectly or indirectly, by liv-

ing in an Enlightened Age. When liberty and science appear to diverge, therefore, especially when social science (the crux of the two principles) seems incapable of aiding liberty, or indifferent or even hostile to it, the liberal senses a crisis of liberalism, not merely a passing difficulty. In recent years the American liberal has had to live through the troubles of the Democratic party and of the university, his ancestral residence and his vacation home, and the experience has given him a psychosomatic pain. He has had to defend both while losing confidence in both. What task could be more useful and urgent to him and to others depending on him than to reconsider the thought of Thomas Jefferson, who held that liberty and science must progress together, who argued the special significance of America as an experiment for mankind in the possibility of such progress, and who, to carry on this American experiment, founded both the Democratic party and the liberal university?

Professor C. Randolph Benson's book presents Jefferson as a social scientist in the comprehensive view we require. He sees in Jefferson the spirit of Comte, whose comprehensive, "positive" science was to replace the reactionary votaries of religion and their destructive critics as well. But as Benson recognizes, Jefferson would have found Comte's authoritarian politics abhorrent. Today it would be inadequate but not misleading to read "B. F. Skinner" in place of "Comte"; and the retreat from liberalism sounded recently by that eminent social scientist illustrates our compelling need to assess both principles of liberalism together. How, then, were liberty and science joined together in Jefferson's thought, and what has happened to disjoin them?

These are not Benson's questions, to be sure; Benson wants to consider Jefferson as a precursor of modern social science. But his intention leads him toward the predicament in which we find ourselves. Part I of his book is on Jefferson's background, and makes clear that Jefferson looked on that background not as a formative influence but as an opportunity to make a new beginning, the experiment of men "trusted to govern themselves without a master." Part II, the bulk of the book, explains Jefferson's thought on "social structures and functions" in nine chapters divided conveniently if not systematically. Part III puts together "Jefferson as both social scientist and reformer."

To explain why Jefferson is but a "precursor" of modern social science, Benson allows that his social science was pre-Darwinian, and thus based on Newtonian and Locke-

an natural law, whereas ours is post-Darwinian, and not based on an unchangeable nature of man. But this difference goes deeper than Benson concedes, for Jefferson's pre-Darwinian natural law appears to be the basis not only for his social science but also for his understanding of liberty. Jefferson could not have put his trust in the rights of man to secure liberty if *man* were mutable and if the human were not definitely separable from the subhuman. He could not have denied the justice of slavery if he had not found in his scientific researches that the Negro was not inferior to whites in moral sense. He also declared his "suspicion" that the Negro is intellectually inferior to the white, but this inferiority was not the critical one to him. Benson calls Jefferson's suspicion on this point "ethnocentric" (p. 226), and no doubt it is; yet now that social science has abandoned the moral sense as defining man, it cannot match Jefferson's honest candor in studying the intellectual endowments of different races or groups of men.

Thus we observe that Jefferson's pre-Darwinian thinking produces a difference with today's social science on what he called "natural aristocracy" and it calls "elitism." Jefferson is quite sure that a natural aristocracy of talents exists that must be distinguished from, and made ascendant over, the pseudo-aristocracy of the wealthy and the well born. But post-Darwinian social science is obliged to doubt the existence of a natural aristocracy; so it must either deny the need for aristocracy (because all men are by nature equal?) or acquiesce in the rule of a pseudo-aristocracy. Post-Darwinian social science could never assert that the purpose of secondary education is to rake the geniuses from the rubbish. Nor could it admit that with mass education "the briar and the bramble can never become the vine and the olive." Jefferson went on to say that "their asperities may be softened by culture"; so he still hoped for "great advancement in the happiness of the human race" (p. 179). But the ground of his hope remained the difference between briar or bramble and vine or olive, in his own metaphor of unchanging species: Men may be softened if we know what a man is, so that we know what a prickly or a softened man is.

Benson remarks that a surprising amount of Jefferson's thought concerned religion. He is right; religion was a problem in those days. But although Jefferson fought all his life for religious liberty, he did not go to the extent of our post-Darwinian indifference. Because he thought the nature of man was immutable and hence knowable, he was willing, as he said, "to make com-

mon cause even with error itself"; for man's natural moral sense may need to be sharpened by religion. We might ask, for example, which makes the more powerful indictment of slavery—the social scientist's accusation that it is ethnocentric, or Jefferson's famous blast? "I tremble for my country when I reflect that God is just, that his justice cannot sleep forever. . . ." Let us take note of the admission that God's justice can sleep for a long time. Yet Jefferson assures us that it will awaken; and he speaks with the confidence of a man who knows the truth of the matter, in contrast to the social scientist who has to worry that he himself may be ethnocentric. It is not out of greater honesty that liberals today do not make common cause with error, but because they do not dare.

Finally, it might appear from Jefferson's pre-Darwinian certitudes that he was more optimistic or complacent than liberals today. But on the contrary, he believed that a certain "pugnacious humor" seems to be a law of man's nature. This humor is responsible for war and slavery, and it must be restrained not only by the moral sense but also by the free government established in the American experiment. He knew that the experiment might fail because of the force of that humor, but he believed he had ground for hope in the better side of man's nature and in man's self-interest. This ground, as Benson shows in a critique of Daniel Boorstin, saved him from the complacency of living in a "lost world" of local and temporal circumstance. But in the post-Darwinian world of accidental change, no experiment is valid for mankind; and the alternative to despair is complacency. Who then lives in a lost world, Thomas Jefferson or ourselves?

Dr. Carl Binger is a psychiatrist able and willing to certify, in his book reviewing Jefferson's life before 1800, that Jefferson had a "well-tempered mind." Dr. Binger does not, therefore, examine the record of Jefferson's apostasies during his presidency—a record which caused Henry Adams to doubt that a well-tempered mind could hold Jeffersonian principles. Still, he finds enough contrariety in the period he has chosen to decide that Jefferson's mind was composed of opposites, especially the masculine and the feminine. These elements, he says, combined in an unconscious process to release Jefferson's singular creativeness. Perhaps we should doubt that "combined" is the right word for what his masculine and feminine elements did together in the dark, but at present we can wonder whether the result was pre- or post-Darwinian. Is the well-tempered mind a new creation, or as Dr. Binger

also says, a "normal" mind in the Platonic sense of "best"? In practice, Dr. Binger uses the latter, pre-Darwinian understanding with good sense and also with occasional, unobsessive resort to the formulas of a professional wig-picker. One could ask whether it is accurate to describe as "piling on the guilt" (p. 97) Jefferson's command to his motherless daughter to reread the letters he had written to her for her instruction. Was he not helping his daughter to live responsibly and thus avoid guilt? But on the whole it is welcome these days to read an analysis which assumes that masculine and feminine are elements of a well-tempered mind rather than roles in an ill-tempered society.

Benson and Binger offer a modern look at Thomas Jefferson, and they share the fault of underestimating the difficulties of appreciating Jefferson today. But both books are respectful of Jefferson's merits, unpresumptuous of their own, and well written.

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Academic Scribblers, American Economists in Collision. By William Breit and Roger L. Ransom. (New York: Holt, Rinehart and Winston, Inc., 1971. Pp. 275. \$8.50, cloth; \$3.95, paper.)

The intention of this book is to describe the ideas which have influenced recent American economic policy. Twelve economists are surveyed, beginning with Alfred Marshall, whose *Principles* was published in 1890, and ending with Milton Friedman, whose ideas are at the center of economics today (and are explained here very well). In between are Veblen, Pigou, Chamberlin, Keynes, Hansen, Samuelson, Lerner, Galbraith, Knight, and Simons.

Even to try to survey so much is commendable, and the authors have done more. They take the reader through the ideas of the twelve in a way that shows they are competent theorists themselves. By that standard, economists judge each other. They will not censure a man who is vague about Regulation Q and may think the better of him for being above such details. Even if he is cavalier about single-equation systems, he will not be scored off. But let him miss an implication of the principle of maximization, and he is in jeopardy. A corollary is his attitude toward theory. If he says it is unrealistic, irrelevant, or tautological, he will be suspected of not understanding it as well as he should and be reminded of what Lady Bracknell said in *The Importance of Being Earnest* ("Never speak disrespectfully of Society, Algernon. Only people who can't get into it do that").

The authors are quite inside the society of theorists. They are able to use the basic writings of the twelve and do not have to take their ideas from a commentary, a paraphrase, or a textbook. Thus what they say about Samuelson comes from his *Foundations* and other writings that earned him the Nobel Prize. The chapter on monopolistic competition rests on Chamberlin's celebrated work and on the criticism that has revealed the power of neoclassical economics. That power is indeed a leitmotiv of the book.

What is included in a book of this kind should be left to the authors. They in turn should leave us to regret their choices. About Marshall, they might have said more of his equivocal attitude to the market and self-interest and less of returns to scale. They are properly critical of Keynes but might have been more so in view of the many things that are wrong with his *General Theory*. "We are all Keynesians now," President Nixon said. He was not speaking for the economics profession. The remark is an interesting example of policy being influenced by ideas that have become defunct, something Keynes himself had noticed, though not about himself.

The authors, while trying to be accurate and clear, have tried also to be interesting, a difficult thing to do and better not done at all. Mies van der Rohe once said he didn't want to be interesting—he wanted to be good. What may be interesting here is the biographical chit-chat, but it is less valuable than the economic theory which could have been explained in the same space. The explanations, as they stand, are as accurate as they can be. They would be more accurate if they were longer.

How clear they are is another matter. Some are very clear, like that of the ideas of Abba Lerner who is a model of clarity himself. Others, like the Hicks-Hansen synthesis, will be uphill work for the outsider. But they will be helpful to an undergraduate. Graduate students will find some of them useful. A crisp new econometrician will do well to read the chapters on Marshall, Samuelson, and Knight, there to discover that economic theory has a past, present, and future, or simply to discover it exists. The chapters on Veblen and Galbraith ought to be read by economic theorists, who unfortunately are inclined to ignore what is not congenial to them. The entire book is useful as a history of some leading ideas of the century. Readers from outside of economics, although they will come across some stiff bits, will find it worth their while.

The book, however, does have a limitation:

from the authors' viewpoint, a serious one; from the reader's, less so. The reader can use it to inform himself about economic ideas and need not be troubled by whether or not they have influenced policy. The question must trouble the authors because they wrote the book to show that influence. I do not think they have. To have done so would have required a much longer and more complex book. The history of that relationship indicates policy has influenced ideas at times, has been influenced by them, has ignored them, has been consistent with them without meaning to, and has departed from them by chance. Each of these connections can be illustrated by foreign trade policy.

To believe ideas rule the world is a pleasant thought for the economist (so long as he doesn't think much about the world his ideas are supposed to have made). It is one of the illusions fostered by Keynes. He said, about "the ideas of economists and political philosophers," that "the world is ruled by little else," that "madmen in authority . . . are distilling their frenzy from some academic scribbler of a few years back." He gave the book its title—an unfortunate one. Economists are not that bad; neither are political philosophers.

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The Political Theory of Anarchism. By April Carter. (New York: Harper and Row, Torchbook Library Editions, 1971. Pp. vi, 116. \$6.00.)

Nihilism: A Philosophical Essay. By Stanley Rosen. (New Haven: Yale University Press, 1969. Pp. xx, 241. \$8.50.)

Terms of analysis become terms of polemic, to the confusion of scholars and actors alike. The subjects of the two books under review amply illustrate this; their diverse treatments also indicate something of the sort of rethinking which is needed for the era of postbehavioral politics.

April Carter's title seems somewhat infelicitous. At the outset Miss Carter (lately lecturer in politics at Lancaster University and currently tutor in the Open University) disarmingly grants that anarchist political theory seems a contradiction in terms. Yet she adds that anarchist thinkers are "spokesmen for values which the politically established and victorious have too often forgotten or suppressed." Is it simply a matter of positing "values"? Which ones—and in what sense *are* they, after all, "political"? Even on a sympathetic reading, the answers do not emerge readily from the following pages.

But in her conclusion, Miss Carter explains that her interest is the links between anarchism and more "orthodox" political theory. Specifically, she suggests that the contemporary relevance of anarchism is not its potential for a systematic new theory or pure anarchist movement (she considers both possibilities dubious), but as an aid to the *reinterpretation* of liberalism and socialism, providing some ideas which may be "partially realized" in the activities of popular groups over time.

Better if this came earlier, and more clearly. But in principle, it provides for a judicious mining of "anarchism." Thus, Miss Carter's first three chapters basically compare anarchism with three "orthodox" paradigms: Hobbesian individualism, Tocquevillean constitutionalism, and Marxian socialism. If there are few surprises here, and fewer philosophical depths, the discussions do offer more than a pure proclamation of "values." Implicitly they indicate something of the complex links among normative, empirical, and practical discourse.

The most interesting sections are those in which Miss Carter tries to grapple, on a more explicit and more fundamental level, with the notion of "politics." True to the anarchist spirit, she distinguishes a "social" principle of cooperation from a "political" one of domination and coercion. Yet she recognizes the Greek idea of politics proper as an area of activity among equals, and takes the primary definition of politics to be "the public" realm. And she sees a need to re-explore such ideas, to develop a sense of "citizenship" and "community" which she believes has almost vanished from the contemporary activities and meanings of politics. Her fourth chapter partly brings together these scattered comments. There she examines some "elements which appear to stand right outside the normal political sphere," but which "all have relevance to any attempt to define the sphere of politics and the nature of political activity" (p. 89). And there her figures are the individualist (Max Stirner), the artist (Camus), the moralist (Tolstoy), the hero (Hobsbawm on bandits), and the coward (Falstaff).

Potentially, this sort of discussion leaps beyond the other portions of Miss Carter's book. But only potentially. Miss Carter fails to consider Falstaff as a kind of rebel (a mocking comedian, a wise fool), as well as a man of caution. Her treatment of Stirner as an anarchist, while conventional, is none too penetrating. (R. W. K. Paterson's *The Nihilistic Egoist* [Oxford: Clarendon Press for Hull University Press], also published in 1971, offers a far more

problematical view.) Her only reference to Camus is to the abridged Penguin edition of *The Rebel*, from which Camus' most extended discussion of the pitfalls of artistic rebellion (sur-realism) and his comments on Stirner are missing. (Vintage offers a complete translation, although copyright restrictions may make it hard to obtain in England.) Conceptual problems underlie these lacunae: Miss Carter really provides no "political" integration. She does contrast her explorations with Hannah Arendt's "attempt to set limits to the political realm" in *The Human Condition*. But ironically, Miss Arendt would seem to offer something of the sort of conceptual resource which Miss Carter needs. She does not argue (as Miss Carter believes) that politics is completely separate from other realms such as science and art, but presents a phenomenological interpretation of that "cultural interpenetration" with which Miss Carter says she is concerned. To be sure, many of Miss Arendt's conceptions are double-edged. But so are the issues. Small wonder, perhaps, that from all her investigations here, Miss Carter seems to draw only a vague notion of "responsible citizenship"—one never clearly articulated, but ultimately presented simply as another abstracted "value." Shifting uneasily between reinterpretation and proclamation, her book cannot really be said to cohere.

Quite another matter is the book on "nihilism" by Stanley Rosen (professor of Philosophy at Pennsylvania State University). Superficially, the analytic task here might seem easier: Nihilism has no positive connotations, and Professor Rosen's approach is thus one of coherent attack. But (precisely because it is "negative"), nihilism poses far more profound problems than does anarchism. And what is significant about Professor Rosen's attack is the level on which it operates. Complex, densely written, treating a subject on which too little has been done in English (especially by students of politics), his book immediately plunges its readers into a conceptual feast. Those who expect to get a relatively straightforward ideological dissection or history of ideas will find themselves confronted with much richer fare. Even in the chapter entitled "Historicity and Political Nihilism" both the idiom and the focus are likely to prove unsettling. For Professor Rosen takes nihilism to be fundamentally a *philosophical* problem; and he means by that, things undreamt of in empiricist epistemologies.

Even in his statement of this thesis, Professor Rosen makes few concessions to his readers. But a summary of it serves most readily to indicate the nature of his discussion. The thesis can

be characterized as an elaborate extension of the Nietzschean definition of nihilism. Where "everything is permitted," he tells us, everything has equal worth—which means that nothing has any worth. But, he adds, this is only secondarily a matter of morality—and only secondarily a matter of our current historical situation. Nihilism is not a contingent phenomenon of historical practice, but a universal "theoretical" one, for it arises from the "dialectical" nature of man. "More fundamentally put, history, which in its modern sense has given rise to the modern form of nihilism, is a philosophical problem" (p. 137). Specifically, notions of morality derive from notions of rationality. And modern nihilism is essentially a problem of "the contemporary crisis of reason," "a radical deterioration in our conception of what it means to be reasonable" (pp. xiii-xiv), whereby the notions of the reasonable and the good have been detached, so that it is impossible to speak meaningfully of the goodness of reason. So it is that, in Professor Rosen's view, where everything is permitted, nothingness is manifest even in speech. Lacking any rationale, every "speech of justification" is really reduced to the senselessness and insignificance of total "silence." Reason becomes merely a matter of superimposing man-made categories on the world, a process of world-mastery which is also one of world-creation; man and nature are fused, and man's work becomes its own end and the only source of its meaning; philosophy thus becomes indistinguishable from poetry—and the goodness of *poiesis* becomes a purely "subjective" affair. Hence, for Professor Rosen, much contemporary "linguistic" philosophy is actually nihilistic; and the same is true of historicism, which takes the "ontopoetic" form of sacrificing the past for the silence of an unknowable future.

Politics thus recapitulates epistemology: no *logoi* without *logos*. Not for nothing is Professor Rosen the author of a previous work on Plato's *Symposium*. Here the *Republic* looms large in his recommendations for the resurrection of reason. But the treatment is by no means simple or doctrinaire. Professor Rosen acknowledges debts to two quite diverse masters, Leo Strauss and Alexandre Kojève. And he chooses as the special targets of his attack those two notoriously obscure moderns, Wittgenstein and (especially) Heidegger. A formidable array: the details of Professor Rosen's handling of it are impossible to reproduce here. By treating "nihilism" so broadly, and by taking Heidegger as his main focus, Professor Rosen at times appears to mirror the obscurity he

attacks. Theory, reason, speech, practice—one would like to have the crucial terms explicated in more leisurely a fashion. And similarly one would like amplification of many passages—the defense of Plato against Heidegger, with Plato's insistence on a return to the cave being taken as a *political* analogy which indicates a defense against nihilism (Is there no "silence" implicit in Plato's use of analogies?); the brief references to Marx (Could he not be said to have unified theory and practice only theoretically, rather than to have replaced the former by the latter?); the dispatch of Wittgenstein and ordinary language philosophy in a single chapter, as a conventionalist form of *poiesis* (Why no reference to the famous passage on calculation and *Lebensform*, in the *Remarks on the Foundations of Mathematics*? And how can Wittgenstein's notion of family resemblance be reduced to a "common form" which is "very much like a Platonic Idea" [p. 10]?).

Within this plump volume there are several slimmer ones struggling to get out. Yet it is a powerful book. The "dialectic" of conservative and radical with which Professor Rosen ends is provocative indeed, in its reintroduction of a classical content into a usually denatured term. His treatment of Plato contains much of value. And one general implication of his book is that "ontology," in some significant sense, is unavoidable—a point which needs reiterating nowadays. Some readers are liable to be put off by the obscurity and complexity. Others may reject the philosophical stance; many will choose to accept one set of notions and ignore others. It is doubtful that Professor Rosen's overall thesis will win much acceptance. But those who ignore his book do so at their peril. It vigorously poses issues which have been too long neglected by both political scientists and political theorists in English-speaking academe.

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Politics and Social Insight. By Francis G. Castles. (Beverly Hills, California: Sage Publications, Inc., 1971. Pp. 137. \$6.96.)

This book, while issuing yet another call for interdisciplinary exchange, is at the same time germane to the continuing debate between positivism/behavioralism and historicism/anti-behavioralism in the social sciences. (For example, see this *Review*, September, 1972). The possibility for interdisciplinary studies rests on Castles' assumptions concerning the nature and methods for attaining knowledge in the social sciences. Acknowledging a partial validity to historicism and the relativity of knowledge, Castles

nevertheless argues that social scientists can assume their knowledge does have "pertinence to the problems of the real world" because it "can be tested by evidential reference to the real world," i.e., an empirical assertion can be "contradicted by all men irrespective of their normative frame of reference" (p. 44). Castles' rejection of historicism is biased by his implicit assumptions of the accessibility of the "real world" and the intersubjectivity of human knowledge. He rejects, however, an ardent empiricism by insisting upon the scientific utility of nonempirical statements that give "psychological impetus to scientific imagination and insight" (p. 45). By implication, the criterion of utility is apparently insight, not verifiability. He does accept empirical techniques as essential, but fears a "disproportionate emphasis on them." He bases his "limited confidence in the evidential basis of a science of man" (p. 45) upon the testability of empirical statement, falsification, and a "methodology of faith." Asserting faith in insight he turns to "metatheoretical models, and other untestable 'noble abstractions'" (p. 47) to provide hypotheses capable of being tested. While his insistence upon the "need for insights prior to investigation" (p. 46) is somewhat similar to historicism, their purpose is to generate testable empirical research. According to Castles, although metatheory is unscientific (unfalsifiable), it does serve a useful function and should be judged only by the utility of its insights. The book, then, fails in its sketchy, incomplete, and somewhat traditional attempt to develop a coherent middle position between positivism and historicism.

Castles, a British sociologist, emphatically rejects a narrow interpretation of politics, as well as any institutional and methodological restrictions on its study. Utilizing a paradigm which includes formal political structure and "wider social reality" he dismisses attempts to identify politics logically with any limited part of it. While eschewing the possibility of self-contained boxes such as "Descriptive Politics" or "Political Analysis," he also dismisses a political sociology that avoids political evaluation and prescription because of its "neurotic fear of being thought unscientific," and a political philosophy that is "unwilling to admit explicitly" its "reliance on the findings and perspectives of other disciplines" (p. 8). Since social science disciplines interpenetrate one another, to refuse to admit "the relevance of other disciplines" (including literature) is to limit the available range of insights. The same is true for research methods. There can be "no *a priori* assumption that the perspectives and methods of

a particular discipline are inappropriate" (p. 4). His argument, in my opinion, is compelling, but it is dependent upon the validity of his paradigm.

Finally, the author develops some familiar perspectives—the metatheories of functionalism, conflict, and anomie/mass society—which he deems important and fears will not be provided by a political sociology (by political scientists) which "takes from sociology only certain highly visible elements, and not all those perspectives that would be available to a general sociology of politics" (p. 11). Exactly why political scientists are more imprisoned in their discipline than sociologists is left unexplained. Castles takes Rex's ideal types of individual interactions, "perfect co-operation, perfect conflict, and perfect anomie" (p. 13), develops these into three idealized ways in which individuals can react to societal norms, and finally, creatively delineates how these concepts express the core insights contained within his three metatheories. "Perfect co-operation" becomes the core insight of structural-functionalism, with its emphasis upon system stability; perfect conflict between individuals prefigures social conflict theory; and perfect anomie, or individual normlessness, stimulates the approach of contemporary theorists of mass society. His development of these three orientations is well done, but offers very little that is new.

Unfortunately, in a book as brief as this many ideas are merely hinted at; his two-page conclusion only suggests that these conflicting metatheories might be complementary—each concentrating on a different level of human behavior (i.e., conflict at the group level, etc.)—and necessary for a balanced perspective. The book is dated; most of its sources are pre-1968; nevertheless, it is a worthy interdisciplinary effort that could be utilized in a variety of undergraduate courses.

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Civil Disobedience: Conscience, Tactics, and the Law. By Carl Cohen. (New York and London: Columbia University Press, 1971. Pp. 222. \$2.95.)

Civil Disobedience: Theory and Practice. Edited by Hugo Adam Bedau. (New York: Western Publishing Company, Inc., 1969. Pp. 282. \$1.95.)

It has often been observed that American political thought has tended to treat fundamental philosophic problems as self-evident truths. Not

given to philosophizing about politics. American political thinkers have instead produced a body of thought preoccupied with mechanical problems and characterized by legalistic, technical discussion. The best contemporary evidence of the continued validity of this criticism is the recent profusion of works on civil disobedience. Only in America could Abe Fortas's *Concerning Dissent and Civil Disobedience* be condemned as authoritarian, repressive, and fascist. Virtually all of the writers on the subject concede the necessity—indeed, the desirability—of civil disobedience under certain circumstances. The debate is, thus, reduced to questions of where, when, and how. A particular thinker's liberalism or conservatism can then be measured by placing him on an "ideological" scale ranging from those who see the streets as a sort of public forum to those who suggest that they might also be used for the conveyance of traffic.

Both of the volumes under review manifest the narrowness of this theoretical framework. Professor Cohen's book is full of observations and insights which, in the hands of a Burke or a Burckhardt, might serve as the premises upon which a radically thoroughgoing critique of civil disobedience could be constructed; Professor Cohen, however, comes down on the disobedients' side. Professor Bedau's anthology presents not divergent points of view but variations on a theme. This is not to deny that either is useful reading on its own terms. But the reader must be aware of the limitations of those terms, lest he approach either book with unwarranted expectations.

Professor Cohen sets himself the task of developing a philosophic framework within which "individual acts [of civil disobedience] may be more fully understood and appraised" (p. 210). Despite a yeomanly effort, he does not entirely succeed.

For Cohen, "civil disobedience is an act of protest, deliberately unlawful, conscientiously and publicly performed" (p. 39). It is *not* simply public protest; it is *not* conscientious objection; it is *not* rioting; and it most definitely is *not* revolution. In his uncompromising assertion that civil disobedience is reformist only and not revolutionary, Professor Cohen will not win the accolades of those such as Professor Howard Zinn who maintain that it can be either. Nor will he endear himself to the apologists of the New Left by maintaining that civil disobedients "should be punished" (p. 76), not for their civil disobedience, but for their deliberate, albeit conscientious, violation of a particular law.

Having set forth what civil disobedience is

not, Cohen distinguishes four genera of civil disobedience: direct and indirect, political and moral. (By recognizing as legitimate indirect civil disobedience, i.e., the violation of a law which is not itself the object of protest, Cohen, of course, goes beyond what Fortas would accept.) He further sorts out several species of political disobedience (civil disobedience toward the realization of a desired result rather than simply a public statement of personal conscience), i.e., pressure, confrontation, and resistance.

After establishing these criteria of recognition, Cohen turns to the task of identifying criteria of justification. From the outset, he disclaims "any effort to show that civil disobedience, taken generally, is always justified, or is never justified" (p. 93). Maintaining that justifications are contextual, he examines the legal, moral, higher-law, and utilitarian justifications for civil disobedience, but though he rigorously and inclusively states the problems, he resolves none of them. Rather he turns to a critique of seven arguments against civil disobedience. But to demonstrate the inadequacies of your opponents' arguments does not substantiate the validity of your own. Indeed, in his last two chapters Cohen examines and finds wanting two of the most common contemporary arguments advanced to defend much civil disobedience, those based upon the First Amendment's protection of the freedoms of speech and assembly and upon the Nuremberg Judgments. These unquestionably are the most satisfying presentations and manifest a familiarity with and understanding of judicial precedents and processes too often lacking among legal philosophers.

Cohen's examination, however, is disturbingly bland and bloodless. Interspersed throughout the text are nineteen case histories; dealt out like so many playing cards, these actual examples of civil disobedience are sometimes used for illustrative purposes but are never analyzed at length. Had Cohen done so, he might more clearly have recognized the morally disturbing potentials which civil disobedience presents. Although he cautions that "civil disobedience is not child's play" (p. 35), Cohen never seems to appreciate the tensions which exist in real-life disobedience situations. The behavior I have witnessed as a graduate student and faculty member in California during the past decade bears not the slightest resemblance to the rosy fantasy which Cohen apparently uses as his model. He asserts that civil disobedience "invariably . . . is . . . civilized" (p. 31); my own observations suggest that

many civil disobedients seem to have a commitment to hostility.

In the end, Cohen's analysis is equivocal. But perhaps his equivocation arises because he has forthrightly attempted to deal with a difficult subject not susceptible to the facile treatment, rhetorical sleight of hand, and oversimplification characteristic of Zinn and his ilk. Unlike Zinn, who advocates unlimited freedom of expression for the "good" guys only [see Zinn's book review, 85 *Harvard Law Review* 897 (1972)], Cohen recognizes not only that there must be some limitations upon civil disobedience but also that what's good for the Weathermen is good for the Ku Klux Klan. In this respect, Cohen's analysis, whatever its shortcomings, is a welcome, even-handed redressing of the balance in the literature on this subject.

Much the same may be said of *Civil Disobedience: Theory and Practice*. Professor Bedau has drawn together in one reasonably brief and inexpensive volume several thought-provoking statements on the subject. Following an introductory essay by the editor and an excerpt from Thoreau, the book is divided into three parts. The first section deals with civil disobedience against racism. The second is entitled "Against War," and the third presents four abridged essays debating the justifications for civil disobedience. The book's virtues, however, are also its vices. Perhaps if one has seen one reader on civil disobedience he hasn't seen them all, but it seems that way. Certainly, *Civil Disobedience: Theory and Practice* duplicates several collections of readings already available. Moreover, Bedau has achieved a brief volume either by selecting several essays too short to permit developing important themes or by utilizing the editorial scissors too freely. But, on the whole, the merits outweigh the defects, and the volume's inclusiveness and its price make it pedagogically attractive for use in political theory or public law instruction.

It should be apparent from these somewhat impressionistic appraisals that Cohen's analysis and Bedau's anthology are both worthwhile either for personal reading and edification or for class assignments. But the American Camus has yet to make his appearance.

RICHARD Y. FUNSTON

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On Historical and Political Knowing: An Inquiry into Some Problems of Universal Law and Human Freedom. By Morton A. Kaplan (Chicago: University of Chicago Press, 1971. Pp. 159. \$6.50.)

On Historical and Political Knowing, though

a long soliloquy, is nonetheless much too short for tackling all the questions the author feels he is up against: "What do we mean by truth, by meaning, by freedom, whether of a particle of a man, or an organization? What do we mean by prudence of action in an uncertain world?" (p. xii). Professor Kaplan, dismayed by what he perceives as defection from the ideals of science, attributes the mischief to certain "current misconceptions" that "lie, at least in part, in the realm of philosophy" (p. vii). The philosophical foe is divided into two camps: the "common language" philosophers, represented by Dray and Scriven, who would seemingly abandon the methods of systematic social science in favor of raw, untestable intuition; and the ideologists—those ancient enemies Hegel and Marx—whose siren songs apparently still have the power to lull social scientists into shipwreck upon the rocks of dogmatism. The "good guys" include Peirce, Dewey, Morris Cohen, Karl Popper, and Carl Hempel, though it is chiefly Hempel (and his formulation of the covering law model of explanation) upon whom the author relies in his philosophical battles.

Professor Kaplan is no unreconstructed positivist or scientific hardhat preaching the *realpolitik* of tough empiricism. Rather, he is sensitive to a dimension of scientific inquiry that he variously calls "existential" or "subjective," a dimension which sets limits upon what can be objectively known by positive science. Professor Kaplan is somewhat ambiguous in describing how this theoretical limit operates upon inquiry, for he cites as evidence of its existence such diverse things as relativity theory, Heisenberg's uncertainty principle, and Gödel's incompleteness theorem: "In the realm of logic and mathematics, Gödel has shown that any mathematical system that can be shown to be consistent is necessarily incomplete in the sense that it cannot be used in principle to prove all true statements involving the symbols in the axioms" (p. x). Such brief references, however, cannot be considered more than ceremonial, and, in any case, Professor Kaplan is wrong about what Gödel showed; many mathematical and logical systems are both consistent and complete. At times Professor Kaplan portrays the theoretical limit of science as posed by a "problem of self-reference: the fact that the act or state of knowledge cannot include itself" (p. xi). At other times, the epistemological borders of science are set by the consideration that "all scientific models, whether biological or physical, are necessarily closed and complete; and all existential worlds including even those of physics are necessarily open and

inexhaustible" (p. 143). "The world of knowing is open" Professor Kaplan insists, "the world of the known is closed. The world of knowing is the world of continuing interaction. The world of the known is the world of closed scientific explanations and of analytic truths" (pp. 144–45).

Are we being offered a counsel of hope, a celebration of existence over theory, freedom over prediction and control? Not at all. Freedom, in Professor Kaplan's conception, "cannot be examined apart from the constraints imposed by the characteristics of an entity. A ball on a surface has the freedom to act in accordance with the laws of mechanical equilibrium but not to run along tracks. A railroad train has the freedom under appropriate circumstances to move along the tracks in whatever direction the tracks go" (pp. 146–7). Are those examples the best paradigms for exploring the concept of freedom? Professor Kaplan, to be sure, maintains that freedom has to be redefined "for different kinds of entities" (p. 146). "The freedom of a ball to roll is dependent on its globular shape. The freedom to act morally depends upon a framework for and a capacity for moral choice" (pp. 156–7). If there is a difference between such "freedoms" as the "freedom" to roll and the "freedom" to act morally, Professor Kaplan's apposition nonetheless conveys a belief in their similarity. His entire discussion of freedom emphasizes what many would take to be the very opposite concept; namely, that of constraint. For he contends that "every freedom requires a set of consistent constraints. These constraints are not mere limitations on the specified types of freedom but are the very grounds for their existence" (pp. 157). I don't think the "ideologists" whom Professor Kaplan deplores would find his conception of freedom too objectionable.

The most interesting discussion in *On Historical and Political Knowing* is contained in those sections where Professor Kaplan attempts to settle the hash of the "ordinary language" school. At issue are certain criticisms of Hempel's covering law model of scientific explanation, criticisms that Professor Kaplan finds irrelevant, or worse. Now, the covering law model of explanation was, among other things, an attempt to provide an anatomy of the notion of explanatory power. "Why do explanations explain?" was one of the basic questions that the model was supposed to answer. But before that question can be answered, one must first decide what an explanation is. For Hempel, an explanation is basically a certain kind of linguistic entity—an *argument* having premises

and a conclusion, and not a single sentence as many of Professor Kaplan's examples erroneously suggest:

(a) "Poison kills" is a truistic explanation of an apparent "why" type (as is "power corrupts") but a weak and unreliable one. (b) "Poison deprives the blood of oxygen" is a better explanation that begins to answer "why" type questions. (c) "Poison deprives the blood of oxygen, leading to cellular brain damage, leading to non-transmission of neural messages across synapses, leading to the cessation of breathing and the stoppage of the heart, in the absence of which life is not possible" is still a much better explanation of the "why" type (pp. 18-19).

Hempel's "ordinary language" opponents tend to conceive of explanations basically as acts of communication rather than linguistic entities. Each of the major components singled out by Hempel as an essential element of explanatory power—deductive validity, generality of premise, truth of premise, etc.—has been disputed as not being essential for explanatory communication. Consider, for example, Professor Kaplan's own illustrations cited above. Neither (a) nor (b) nor (c) is an argument, and therefore none of them can be "deductively valid." None of them is true ("systemic" poisons such as prussic acid operate in the manner indicated by (c) but "corrosive" poisons such as lye do not). But these "failures" need not prevent any of the above sentences from being part of a successful explanatory act of communication. For example, if someone wants to have explained to him why Jones expired after taking prussic acid, asserting (c) might do the necessary explanatory job, even though (c) is false, provided that it was clear to all parties that one was talking about the particular incident and not about all incidents of poisoning. Nor is asserting (c) necessarily a better way of explaining something than asserting (b) or (a), as Professor Kaplan maintains, for suppose a person didn't understand what synapses were. It must be conceded to Professor Kaplan that opponents of the covering law model have not yet explained, in satisfactory fashion, why explanations explain, but it is surely wrong to consider, as Professor Kaplan tends to do, that the attack on the covering law model of scientific explanation is an attack on science itself.

HASKELL FAIN

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The Party of Eros: Radical Social Thought and the Realm of Freedom. By Richard King. (Chapel Hill: University of North Carolina Press, 1972. Pp. 227. \$7.50.)

Political Hysteria in America: The Democratic Capacity for Repression. By Murray B. Levin. (New York: Basic Books, Inc., 1971. Pp. 312. \$8.50.)

Radical Libertarianism: A New Political Alternative. By Jerome Tuccille. (New York: Perennial Library, 1970. Pp. 130. \$1.25.)

King, Levin, and Tuccille all explore radical ideas, groups, or thinkers operating in American political life. King examines the views of left-wing theorists in America who have been noted for their defense of sexual liberation—theorists such as Reich, Goodman, Marcuse, and Norman O. Brown. His study presents a chapter on each of his figures, reporting their history, the pattern of the development of their thought, and their essential message. It is basically a book to explore for information rather than argument. Levin considers the fate of radical dissenters in America since the days of the post-World War I Red Scare. His purpose is to study such broad themes as American political psychology, American political ideology, and their interactions with our political experience. His conviction is that for most of this century our liberal consensus has allowed us to repress dissenters far too often and in violation of the possibilities of America. Tuccille enthusiastically reports on the emergence of a genuinely radical-libertarian faction within American conservatism. He discusses the program of contemporary radical libertarians and its recent history, and he puts forth a vigorous argument for this political position.

Whatever their similarities, however, these three books are substantially different in style and quality. Professor King's *The Party of Eros* is a work of careful scholarship. Unlike so many books about currently fashionable thinkers on the Left, his book avoids uncritical adulation as well as polemical denunciation. Generally, King is a calm, thoughtful chronicler. His treatment of Marcuse is an especially powerful illustration of his approach. King refuses to succumb to the temptation to leap into the controversies that surround Marcuse and his work with yet another highly tendentious position at the expense of the goal he seeks: a precise, disciplined elucidation of Marcuse's ideas.

King does develop his own view sharply and quite fully in a concluding chapter. There he indicates his substantial reservations about "the party of Eros." He also links "the party of Eros" to some extent with the gurus of the counterculture. This judgment seems to confuse genuinely creative social thinkers with second-rate popularizers, such as Charles Reich.

King often raises probing questions, which

he poses quietly and sometimes devastatingly. But at other times his balanced considerations are a bit lifeless. Perhaps the problem is that he chose to analyze each man's thought chronologically, book by book. This tends to turn his chapters into excellent book reports. King also may be faulted for a curious emphasis in a book about sexual radicals: he seriously neglects Brown, while dealing at length with Goodman, a mild sexual radical at most. In fact, King's chapter on Goodman is fascinating and revealing, but his brief consideration of Brown is less successful. He does not enter Brown's world, nor does he seem to want to. There is little evidence that King appreciates Brown's puzzling, but also dazzling, creativity. Consequently, King does not give us a full sense of Brown, and since Brown is surely the most radical thinker of the group, this is a disappointment. When one finishes King's *The Party of Eros*, however, one knows he has read a book that is learned, thoughtful, and balanced.

Professor Levin's *The Politics of Hysteria* is interesting when it discusses the Red Scare of 1919. Insofar as it is a case study of an example of "the democratic capacity for repression" it seems to have lessons to teach. But as the reader moves through each of the seven chapters, the book becomes progressively more focused on exposing a seemingly endless list of sins in contemporary America, especially including our bondage to sundry myths of the liberal tradition. In his chapter six, Levin makes clear his dependence on Louis Hartz's analysis of America in *The Liberal Tradition in America*, but he converts Hartz' doubts about American liberalism into stinging condemnations, regarding the liberal tradition as intolerant, repressive, nationalistic, "totalistic" and generally quite without any saving graces.

Levin's book gives a social-psychological overview of the supposed propensity of Americans to fall victim to myths and conspiracies. His fourth chapter concentrates on this particular weakness of America, but Levin's analysis is so general and so intensely normative that it falls well short of the standards of many practicing political psychologists, including those he cites, such as Murray Edelman.

Underlying Levin's discussion is an outraged radicalism. Levin protests inequities in this country in the name of social justice and individual liberty. But at the same time he is apparently no admirer of the common person; he depicts the average American as little more than a fool, readily manipulated by elites skilled at myth-making and trickery. Thus while he be-

wails the elitism he sees in American society, he ignores the elitism that is central to his own outlook.

Jerome Tuccille's *Radical Libertarianism*, represents yet another style of book. Tuccille is one of a group of writers today, including the more famous Karl Hess, who have written and thought about the connection of the radical Left and libertarian Right. Tuccille's book, which is utterly devoid of Levin's chic misanthropy as well as King's scholarly accomplishment, is designed to encourage an awareness of the right-wing form of libertarianism. He presents, in a spirited and subjective way, the dubious case for a political outlook which includes a kind of free capitalism plus virtual political and social anarchism.

This might be a good book for a contemporary issues or modern political theory course. It is easy to read and often entertaining, especially in its wonderful description of the clash at the 1969 Young Americans for Freedom Convention between the libertarians and the more traditional conservatives. It also raises many issues of importance, including the utility of present political labels, the future of conservatism, the role of the state, and the validity of thinking in terms of abstractions like the "public interest."

Tuccille wants to get rid of everything statist from the draft to welfare to pollution controls. His reliance on the reduction or abolition of the state to solve every problem is often excessively monistic: for instance, holding lotteries and bazaars is not likely to solve the welfare mess, nor are private means particularly likely to stop pollution. But Tuccille is a constantly lively and inventive disputant, who can teach something to any reader.

ROBERT BOOTH FOWLER

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Political Theory and Political Science: Studies in the Methodology of Political Inquiry. By Martin Landau. (New York: The Macmillan Company, 1972. Pp. xi, 244. \$6.95.)

Martin Landau lets the cat out of the bag early: "There are, frankly, many people who are just dumb, or who ply fashion and ride bandwagons—or whatever. The theory of probability tells us that this will be so in any population, whether traditionalist, behavioralist, or post-behavioral revolutionary. Vitiating, then, scientific methods on this count is like invalidating baseball because there are lousy batters" (p. 12). He puts the cat back in the bag two hundred pages later in a comment on the science-art controversy in public administration

that can be extended to all political science: "... this controversy, and its attendant confusions, has led to a good deal of acrimonious debate ('quarreling' is perhaps more accurate) which has more to do with the problem of professional status and self-identity than with the intellectual issues involved" (p. 202).

In between, Landau gives the lie to Schiller's dictum that "against stupidity the very gods themselves contend in vain." In a series of sophisticated essays on the methodology of political investigation, Landau neither plays God nor puts up with dumbness, fashions, or professional quarrels. Carefully selective in the choice of some very smart people as his intellectual targets, or better, of their arguments, he cuts through metaphorical nonsense, spongy conceptualizations, opportunistic double-talk, empty rhetoric, ideological dogmas, sloppy thinking, professional distemper, political *Weltschmerz*, and other disciplinary maladies to which we have been treated, if not accustomed, in recent years.

However, Landau's critiques are not for critique's sake. The objective, throughout, is to clarify what science can or must mean in political science, and he does so with great elegance in thinking, with great economy in writing, and with great seriousness in purpose. Beyond these virtues, there is another that has of late been in short supply—an extraordinary dose of mental sanity. He enjoins us, for instance, that before using, misusing, or abusing Kuhn's interpretation of normal science and scientific revolution, we had better consult the historians and philosophers of science, who, on any number of grounds, are something less than enthusiastic about this particular theory of knowledge making. Or he suggests that we apply Mannheim's sociology of knowledge to Mannheimian analysis itself and see what we get—intellectual chaos (of which we have a great deal in any case). But all of this is done without the kind of pejorative style that, in some academic circles, has become a substitute for thought. Indeed, Landau proves himself to be a twentieth-century thinker (if I may use a perhaps obsolete term) in the tradition of the Enlightenment. If that makes him a traditionalist, so be it. If it makes him a post-, post-post, or post-post-post-something-or-other, that's even better.

Although written over a period of ten years, the eight essays brought together in this volume do not constitute the usual potpourri of miscellaneous writings by a prolific author. They are held together by a single theme that is given its most precise statement in the last chapter, on "Due Process of Inquiry." I have rarely read so

sensible a statement of what the "rules of method" in scientific discourse are all about; and the reader may well want to begin with this chapter as a more systematic introduction to Landau's thought as a whole than can be found in the first chapter, not previously published, on "Science and Political Science: Some Observations on Prevailing Complaints" in which Landau defines his own observational standpoint in the context of contemporary disciplinary polemics.

Landau's approach to methodology is by way of inquiry into the vocabulary and grammar of political science. The approach has its richest payoff, it seems to me, in another previously unpublished paper on "Objectivity, Neutrality, and Kuhn's Paradigm"; but its simplest and perhaps clearest treatment is found in the earliest essay, published in 1961 and entitled "On the Use of Metaphor in Political Analysis" (Chapters 2 and 3). The approach is further applied and exemplified in Chapters 4 and 5, "On the Use of Functional Analysis in American Political Science," and "The Myth of Hyperfactualism in the Study of American Politics." I am reciting the chapter titles to give an inkling of the broad range of Landau's concerns and to alert the book's large potential audience, but also to suggest why I cannot possibly devote sufficient space to reviewing the subject-matter of any particular chapter.

The importance of the "dictionary problem" is demonstrated in Chapter 6, "Development Theory: Some Methodological Problems," which begins with an analysis of Riggs's optical lexicon and ends with a discussion of the difficulties involved in epistemic correlation between facts and values, or means and ends. The problems of grammar and vocabulary are most critical where theoretical knowledge and applied knowledge meet, and where the languages of science and the languages of common sense are being confronted. Enthusiasts of the "cult of relevance" might give particular attention to Chapter 7, "Political Science and Public Administration: 'Field' and the Concept of Decision-Making." Because their memory is short (and, I daresay, their knowledge of our discipline's history limited), they may discover why the "post-behavioral revolution," so called, is mostly old hat for those who have lived through the years of torment when the subfield of public administration was in search of a "field." Even what Landau, responding to Easton's curious reversal of position, has to say on "relevance" in the opening chapter may become relevant.

Needless to say, perhaps Landau's thought is

so agreeable to me because I agree with most of it. Beyond this, however, Landau emerges in this book as one of the most erudite of contemporary scholars in political science—and erudition is in short supply along with sanity. He is clearly at home in our own discipline, but also in the other sciences and in the philosophy of science. I have really only two related comments to make. On the one hand, for those of us who spend most of their time in the crucible of empirical research with its emphasis on technique, Landau's rigorous conception of method and its problematics will serve as a reminder of the fragility and perishability of our labors if they are not grounded in "due process of inquiry." On the other hand, while I appreciate Landau's insistence on methodological clarity, if not purity, I must ask him to sympathize with some of the theoretical and technical compromises that theory-oriented empirical researchers tend to make and, in the present state of disciplinary development, perhaps must make, if they are to come up with at least some tangible discoveries before senility overcomes them. Granted that we stand on the shoulders of giants and move pebbles rather than boulders—to mix a metaphor. But, then, as Burton's saying goes, "a dwarf standing on the shoulders of a giant may see farther than a giant himself." I am confident that Martin Landau appreciates this more than do lesser minds.

HEINZ EULAU

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Social Determinants of Moral Ideas. By Maria Ossawska. (Philadelphia: University of Pennsylvania Press, 1970. Pp. xii, 190. \$9.50.)

Maria Ossawska has written a charming and informative book on the sociology of morals, a nonsubject that may some day become a subject. Although she is a distinguished Polish philosopher, not a professional sociologist, Professor Ossawska writes more about society than about morals, perhaps because she doesn't question what "society" means, but thinks "morals" is indefinable. One might suppose that the indefinability of a key term would make her pessimistic about the future of her subject, but that is not the case. Professor Ossawska believes there is a genuine sociology of religion, and that "religion," too, is indefinable, so she proceeds happily to try to develop a sociology of morals.

In order to be a social science, the sociology of morals must first contain descriptions of the morals of a society at a given period of time and, second, formulate laws specifying that one

moral code emerges, flourishes, wanes, and dies under particular social conditions and that another moral code has the same fate under other social conditions. Ossawska foresees little difficulty in moral description, and quotes Durkheim approvingly: "Moral facts are facts like any others; they consist of rules of action which can be recognized by some distinctive characteristics; thus it must be possible to observe them, to describe and classify them" (p. 17). It is in finding social determinants of moral ideas (promised in her title) that Professor Ossawska has greater difficulty.

Still, the difficulty in sheer description of the moral ideas prevalent at any time is revealed by Ossawska's taste for ideal types. If one can't actually describe an accepted moral code in all its nuances, and in its variations from a philosophical statement of ideal belief, one can substitute the philosophical statement for it. Thus the actual beliefs of a hedonist society are unlikely to be exactly what Aristippus or Epicurus said about pleasure and pain, but if one can't describe what the people in that society believe, with genuine differences from group to group and person to person, one recourse is to restate Aristippus or Epicurus and call that more or less systematic exposition an ideal type. In choosing the ideal type rather than the living belief, Ossawska writes as a philosopher rather than a sociologist, although she has the massive authority of Max Weber behind her choice.

Yet the social determinants of moral ideas are more difficult to establish than the prevalence of those ideas. Ossawska spends fifty pages constructing ideal types of the bourgeois ethos and the nobility ethos. The very names suggest that these moral codes, or some variant of them, emerged with particular socioeconomic classes and will probably wane or die with their disappearance. However, Ossawska denies that correlation and, indeed, can discover no social determinants for either ethos. The morality of Hesiod, she points out, as enunciated in his *Works and Days*, was very similar to the ideal bourgeois type, and the ethos of the nobility can be found in some pastoral groups of mountain dwellers.

When Ossawska utters one of her rare positive conclusions, the difficulty of establishing particular social determinants for moral ideas is most apparent. A typical conclusion is that the lack of disapproval of homosexuality in contemporary Japan is probably the result of overpopulation. One standard scientific reading of this assertion is: Under conditions of overpopulation, the people of a nation do not disapprove

homosexuality (Japan is overpopulated; the people of Japan do not disapprove homosexuality). On this reading, it makes sense to say that the assertion, the Japanese do not disapprove homosexuality because Japan is overpopulated, is true only if there is a general law to the effect that such attitudes always occur in overpopulated nations. To establish the general law one would have to examine other overpopulated nations, like India, and discover the same attitude. The law, I suspect, would not fare well.

On still another reading, Ossawska's statement becomes: Under specified conditions of personality and character (based, to be sure, on particular traditions and social environment), the people of a nation respond to overpopulation by ceasing to disapprove homosexuality. One trouble with this reading is that, when we specify all the traits of personality and character, we will probably be talking only about the Japanese. And it will be very difficult to know what traits are relevant to this special response to overpopulation. If we find there are other peoples with the traits we think relevant, they will have to live in an overpopulated nation or we cannot use their behavior as evidence of the truth of Ossawska's assertion. Such are some of the hazards in trying to develop a sociology of morals.

Like the sociology of religion, the sociology of morals would be almost denuded of experiment and would rely heavily on descriptive and statistical findings, questionnaires, interviews, and history. Comparative and historical study would search for social conditions present when a moral code is also present and absent when the code is absent. Just as the sociology of religion does not question whether any religion is true, the sociology of morals will not question whether any moral code is good. But both subjects can be concerned with the adequate or inadequate functioning of a religion or a moral code in a society at a particular time. For example, under some conditions, prevalent moral ideas may perform the function of adjusting and adapting social activities to each other; under other conditions, the same moral codes may be nonfunctional or dysfunctional. Religions are in just the same case. Under some conditions, they provide a social cement; under others, when there is, for example, an encounter of religions, as in seventeenth-century Europe or contemporary Ulster, they make for bloodshed and chaos.

An examination of the functioning of morals may bring social and political conclusions that social study without a moral component would

miss. Ossawska would not approve because she is suspicious of all functional theory, but if we cannot improve on her practice I fear the sociology of morals will be still-born.

RALPH ROSS

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Sexuality and Class Struggle. By Reimut Reiche. (New York: Praeger Publishers, 1971. Pp. 175. \$6.95.)

As regards sex, W. C. Fields once remarked, "There are some things better and some things worse, but there is nothing quite like it." Perhaps something similar can be said about the treatment of sex in social science literature: there is nothing quite like those books that attempt to blend eros and Marxism, psychoanalysis and socialism. One thinks immediately of works by Reich, Adorno, Marcuse, and Osborn, among others, and, at another level, of the sex-and-politics graffiti that in recent years youthful revolutionaries have scribbled on German, French, and American walls. The message of most of these books and graffiti was that capitalism and war promoted and thrived on sexual repression, whereas socialism and peace were both cause and effect of good healthy orgasms. It followed from these assumptions that efforts to restrict sexual freedom, whether emanating from religion, law, conventional morality, or the institution of marriage itself were, in a word, counter-revolutionary. While it is unlikely that such revolutionaries as Chairman Mao, Fidel, and Brezhnev subscribe to these views, most young radicals, especially the German Marxists, continue to regard the sexual revolution as an indispensable accompaniment of economic and political revolution.

German Marxist Reimut Reiche—one is tempted to refer to him as the counter culture's Third Reiche since he follows hard after Wilhelm and Charles—takes sharp issue with the position that monogamy, fidelity, homosexuality, and the institution of the family itself will necessarily, like the state, wither away in a Marxist society; indeed, he is at pains to demonstrate that, far from disappearing, such practices and institutions will take on a purity of meaning and function that they are incapable of achieving in any capitalist order. In Reiche's opinion, the capitalist *Playboy-Penthouse* ethic, because it is dominated by market values, inevitably emphasizes the quantitative aspects of sexual freedom as opposed to the qualitative side. But for Reiche, the quantitative or "anything goes" aspects constitute a "false

sexuality," the sexual counterpart of the "false consciousness" that the Marxists have always opposed to true working-class consciousness, i.e., awareness of the class struggle. True sexuality, according to Reiche, does not consist of unlimited single and group copulation, access to pornography, legalized prostitution, lessons in masturbation, and the like, but is basically made up of "full genital sexual enjoyment." True sexuality, in short, involves what Erikson has called "mutuality" or the capacity to feel, to respond, to empathize, to give as well as to receive.

Indeed, if I understand Reiche correctly, true sexuality cannot exist without love or at least deep affection, and here he is parting company not only with the practitioners of sexual politics, in Germany and elsewhere, but with the orgasm supremacists who dominate the encounter-group movement. Unlike the former, Reiche does not assume that free love in a commune is an adequate substitute for the bourgeois family; unlike the latter, he does not believe that sexual satisfaction is almost entirely a matter of tactility and technique. Building on what is essentially a psychoanalytic model of child and adolescent development, Reiche argues that there can be no maturity for the individual unless he can leave behind him the oral, anal, and phallic states of development as he moves toward "genital primacy" and the capacity for healthy object relations.

Reiche also maintains that such growth is impossible except in a socialist society, and here, of course, many who are equally critical of our sex-as-a-commodity culture will be inclined to disagree. Does "true sexuality" exist in any of the socialist countries, and if so, where is the evidence? Perhaps more important, what is the historical, philosophical, or logical proof for the proposition that relations between the sexes in a socialist society will be inevitably more humane than such relations in a capitalist society? It is not enough to demonstrate that sexual needs are perverted in the contemporary western capitalist world, and it is even less adequate simply to assert, as Reiche does, that "true sexuality" can be achieved only under socialism. Would that this were so! How glorious it would be if we could still believe, as many of us once did, that socialism would usher in the era of perfectible man! Perhaps it all is a matter of faith, and in that event it is absurd to demand more evidence or logic from Reimut Reiche than from, say, Pope Paul. Surely Mr. Reiche is as much entitled to his "next world" as the Pope is entitled to his—and considering that Reiche's includes "true sexuality," who

would want to emigrate from it to eternal but sexless salvation on the Mount of Olives?

ARNOLD A. ROGOW

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Systematic Thinking for Social Action. By Alice M. Rivlin. (Washington, D.C.: The Brookings Institution, 1971. Pp. 150. \$5.95.)

I have no idea how far back in history it is necessary to go to identify the early pleaders for rational decision making in government. Certainly a knowledge of either Latin or Greek would be essential, and the scrolls with the earliest admonitions must be in very delicate shape.

But the voices of both the distant and immediate past have done little in the human resource area either to create order out of chaos, to bring about effective distribution of public funds instead of ad hoc allocation based on political expediency, or to institutionalize systematic assessment of program efficacy. This book, based on a series of lectures that Alice Rivlin gave at the University of California at Berkeley, is both a concerted call for continued efforts to improve the way government works and a sensitive analysis of why the strategies of the past several decades have yielded so limited results.

Dr. Rivlin is admirably qualified. A well-trained economist now at the Brookings Institution, she has extensive experience in government, including a stint as Assistant Secretary of HEW with responsibilities for planning and evaluation. In the essays in this book, her worldly knowledge is intertwined with her scholarly learning. Indeed, an expanded bibliography might have made the volume more valuable for the student and for those not fully familiar with the literature in the area.

The early chapters of this brief book focus on (1) cost-benefit analysis; (2) specification of social goals, indicators of social change and comparative payoff from programs; and (3) evaluation of program impact. They are followed by more extended discussion of strategies for program evaluation. In each discussion, Dr. Rivlin makes use of her intimate knowledge of federal programs on education, income maintenance, and health. She argues strongly for innovative programs and their assessment, large-scale social action experiments and program accountability. She is quick to acknowledge the limited utility of work up to now and careful to specify why this is so. Thus, to cite some illustrations, she suggests that lack of a behavioral model of the population has hindered the analysts' contribution to decision mak-

ing on income security, and that the state of research methodology has curtailed the potential benefits of experimental evaluation studies. Readers who are expert in the technology of cost-benefit analysis, evaluation research, and so on may feel her discussions are both elementary and simplistic. But one must keep in mind that the volume is based on a set of public lectures.

My major criticism is Dr. Rivlin's stance in the volume. I think she sees the policy researcher far too much as a passive technician, rather than as a change agent who initiates and promotes modification in government programs and policies. True, better behavior models, more elegant methodological designs and more efficiency in the conduct of research would help. But not as much, to my mind, as would smarter people who are politically sensitive and who possess intense zeal and ambition to improve the quality of life. In brief, Alice Rivlin is not emphatic enough about the importance of the very characteristics that underlie her own successful participation in government: Technical astuteness and basic social science knowledge are important but the millennium demands analysts with a special posture, a strong point of view, and a vigorous insistence on rationality.

HOWARD E. FREEMAN

Brandeis University and Russell Sage Foundation

Organizational Growth Through Decisionmaking: A Computer-Based Experiment in Educative Method. By Beatrice K. Rome and Sydney C. Rome. (New York: American Elsevier Publishing Co., 1971. Pp. xiv, 242. \$15.00.)

The authors call the empirical work that forms the core of their book an "experiment," but I have some reservations about that description. What they report are their observations on a single group of 24 subjects who, for a period of three months, operated a very elaborate computer simulation of an organization rather than a real one. The "experimenters" took a role in the simulation as a supervising authority to which the subjects were subordinate. Without ever appearing personally, they used their position in the simulation to send down directives which they hoped would promote the development of organization. They also engaged in manipulations of the computer program which were not divulged to the people operating the organization it simulated. They were, in sum, considerably more influential than is usual among participant observers. While it is true that they exercised a degree of

control usually identified with experimentation, they did not have either a commitment to the repeated observation of certain fixed or systematically varying conditions or a systematic basis for deciding how and when to intervene in their subjects' patterns of behavior. "Experiment" does not, therefore, give an entirely correct impression of the study they report. They grew their test-tube organization not with the purpose of testing the validity of existing ideas but with the hope of stimulating in themselves new insights into the development among a set of individuals of the property of being organized. The results do not appear to me to justify either the tremendous cost of the study or their years of effort in executing and analyzing the "experiment."

From scattered remarks in the book I would estimate that ten to twelve man-years and perhaps a year of computer time have been invested in this project. (There are six man-years—24 graduate students for three months each—in subject time alone.) Nonetheless, the authors provide only very sketchy information on their programs and procedures and no indication that such material can be found in other documents. This makes the book of little use to those interested in employing simulations in the study of organizational behavior. Nor is the book a contribution to the ongoing development of the body of organizational scholarship. No major work of any theorist of organizations or bureaucracy is discussed or even cited.

Such shortcomings might be tolerable in a book that was dense with interesting ideas, but *Organizational Growth Through Decisionmaking* is not. It does begin with an interesting question: Assuming that organization develops gradually rather than appearing full-blown, what are the constituent elements or relationships which being organized entails, and is there some sequence in which they are most likely to be observed? Unfortunately, neither their exploration of this question nor their proposed answers are at all profound.

All but the first 36 and last 18 pages of their text are devoted simply to recounting the history of their laboratory group's growth and activity. The Romes promise to show that each of the 44 "developments" they describe is distinct from the others, is universal in character, and occupies a necessary position in the unfolding of a logical sequence of stages. But all they actually deliver is a summarizing sentence or two at the close of their detailed factual description of each "development." These remarks are generally little more than common-sense restatements of what has happened (e.g., Development

20—"The subjects here demonstrated a competence to discriminate between technical action and action to execute policy and a competence to translate this discrimination into deed" [p. 131]). They do not provide good reasons—often they provide no reasons at all—for believing that each described development represents a single coherent stage of growth, likely to occur in most organizations, and likely to occur in a particular ordinal relation to other stages of growth. Yet in the conclusion of the book this is what the authors assert they have shown.

In fact, given certain disturbing features of their research, there are compelling reasons to believe that many of the events they report are not likely to occur in the order they observed—or even to occur at all. Although the Romes stress, as most of us would, that people organize to pursue purposes, they deliberately refused to specify for the subjects what was to be achieved in the operation of the organization. Since the Romes took the role of an unseen superordinate body sending down orders to the laboratory group and evaluating its performance, the subjects were left to guess what the experimenters wanted them to do. The relevance of the resulting behavior to a typical organization thus seems questionable.

Such doubts are reinforced by a number of additional facts. The organization which the subjects were to run was so complicated that even after months of operation "... the task of servicing the entire input load [was] too difficult for the group to solve . . ." (p. 185). Moreover, the experimenters appear to have been blind, if not cruel, in their reaction to the subjects' mounting frustration. The Romes report, with satisfaction, that the second leader of the group did not act on his expressed desire to "sabotage" the whole simulation. They never ask why he felt that way. They quote, without any apparent sense of its peculiarity, the following exchange of messages with the group leader: "You're working us too hard. You want blood or something?" Our reply, "Yes, indeed," was not further acknowledged by him" (p. 195). They see nothing requiring explanation in the subjects' references to their superordinate body as "Big Brother," as being "unreasonable" and "incomprehensible," and as having "an authority complex." Eventually the subjects held a private meeting to reorganize the group's leadership. During that meeting they refused to admit any nonsubject personnel, they made no notes, and they turned off the experimenters' tape recorder. The Romes, who amassed 11,000 recorded messages during this study, thus got no data on its single most

important event. Somehow they were unable to recognize that this was an act of rebellion against them *as experimenters* rather than against the role position they were enacting in the simulation. They blandly report the whole incident as another of their universal stages of organizational growth—"Asserting Administrative Autonomy (Development 33)." The effect is rather like reading an episode from the asylum of *One Flew Over the Cuckoo's Nest* retold from the point of view of Big Nurse.

Taken together, the absence of scholarship, the poor research design, weak intellectual work, and inhumane treatment of subjects provide reasons for rejecting virtually everything in the book except its guiding question. Financial responsibility rests with the Systems Development Corporation and the Air Force Office of Scientific Research.

MICHAEL D. COHEN

Stanford University

The Sociology of Progress. By Leslie Sklair. (New York: Humanities Press, 1970. Pp. 287. \$10.00.)

On the face of it, this is a very annoying book. The young author publishes here a revised version of his doctoral thesis at the University of London, serving as an object lesson on all the ills of behavioral sciences. His central thesis is based on a definition he introduces—the difference between innovational and noninnovational progress. If we take the author at his word, the difference makes no sense because progress is defined as "the end point, temporary or permanent, of any social action that leads from a less to a more satisfactory solution of the problems of man in society" (p. xiv). Innovational progress is explained to be what we knew to be progress all the time—"progress by means of the production of new things, ideas and processes" to which the phrase "with maximum impact on society" (p. xiv) is added, though after reading the book I still do not know what this means. Noninnovational progress is achieved by "maintenance and diffusion of the familiar things, ideas, and processes" but the author is loathe to leave well enough alone and adds "with minimal impact on society" (p. xiv). Now aside from arbitrarily reducing here four possibilities to two (i.e., maximal maintenance and minimal innovations), as noninnovational progress hardly qualifies by the author's definition, he asks how can you improve an existing human society and have minimal impact on it?

I dwell on these twin concepts because Professor Sklair reminds us several times that they are "the crux of the matter." He then proceeds

with a poor man's Parsons tour de force of showing us how Rousseau, Comte, Ellul, Marx, Weber, and several others all were struggling to reach his distinction but did not quite make it. In the process he succeeds in making several scores of generalizations about man, history, society, theory, ethics, and their relations to each other.

Nevertheless, this is a far from trivial book. It deals, in its tortured way, with one of the core issues of the era. It asks, in effect, is it not time we cease to identify progress with the inventions science and terminology continuously foist upon us while disregarding achievements in other areas, from greater humanization to evolution of more responsive institutions? While the perspective is hardly novel, we must note that the book was published in 1970 and written as a dissertation before the need to worry about noninstrumental progress was popular. If Sklair succeeds in introducing this notion into behavioral theories, often a few steps behind the spirit of the times, his ink will have been well expended.

The second part of his volume also indicates his heart is in the right place; he boldly favors a "sociological ethics," a morality which draws and builds on the findings and conclusions of the study of society, man's needs, and aspirations. His ethics are based, first, on the assumption that the survival of human society, is morally worthwhile and that social justice is "on balance the most satisfactory basis of social organization" (p. 197). While I must admit to being somewhat annoyed that the author ignores 80 pages I devoted to the same issue in *The Active Society*, he clearly produces some important insights. Thus he breaks out of the sterile dichotomy that behavioral sciences must be either scientific or a basis for value judgments, by suggesting that universal human needs, which can be ascertained empirically could and should guide our decisions. And he ties his sociological ethics to his theory by reminding us that man's needs include many which only "noninnovational progress" could provide for.

The book is richer than this review reflects because I focused on its main thesis, and much of its value is to be found in numerous scholarly asides about various theorists of progress and sociological theorists which preceded Sklair. I expect that with his homing instinct for "relevant" and macroscopic issues well developed and with his command of the field, Sklair will be heard of and deserves to be heard.

AMITAI ETZIONI

Columbia University and Center for Policy Research

Reinhold Niebuhr: Prophet to Politicians. By Ronald H. Stone. (Nashville, Tennessee: Abingdon Press, 1972. Pp. 272. \$8.00.)

Ronald H. Stone, a former student of Reinhold Niebuhr's and currently Associate Professor of Social Ethics at Pittsburg Theological Seminary, has written a very helpful "intellectual biography" of Reinhold Niebuhr (d. 1971), one of this century's major theologians (although Niebuhr disclaimed the title of theologian), ethicists, and political philosophers. Conceding that Niebuhr's thought can be analyzed from several interwoven perspectives, e.g., theology, ethics or political philosophy, Stone concentrates on the latter, attempting to "correct onesided theological interpretations . . . without committing the error of reducing his theology to the level of politics" (p. 9). Stone's corrective to other interpretations of Niebuhr's politics is to recognize that there is no central doctrine from which the remainder of Niebuhr's thought is derived and to emphasize the importance of a chronological study, not only because Niebuhr's writings reflect a tension between immediate political situations and reflection but also because his thought evolved over time. Stone identifies four major periods in this evolving political philosophy: the liberal, the socialist, the Christian realist, and the pragmatic-liberal. He does not deny the continuities and the presence of some elements, often implicitly, prior to their full development, but he does think that

each phase is characterized by the movement of certain concepts of political philosophy from the periphery of his thought to the center, by a solution to the ideal-real issue which differs from the solution of the preceding period, and by Niebuhr's recognition that his thought in this phase is significantly different from his thought in the preceding period (pp. 10-11).

One continuous thread is Niebuhr's relationship, both positive and negative, with liberalism, which he criticized from Marxist and theological perspectives before he rediscovered and reappropriated it. Another important thread is the basic question of Niebuhr's political philosophy: the relation of man's imaginary communities to his actual communities.

After a chapter which sketches Niebuhr's early years, the book covers the main periods mentioned above and then turns to a fuller analysis of his view of foreign policy since World War II. In the sphere of foreign policy, Stone emphasizes both the influence and general adequacy of Niebuhr's political realism; but he also moves beyond exposition to indicate some weaknesses, for example, the need for a closer relationship between morality and poli-

tics, for a more careful analysis of terms than Niebuhr provided, and for case studies of decision making.

The concluding chapter, entitled "The Relevance of Faith," deals with Niebuhr's social ethics in relation to faith. Stone contends that although Protestant social ethics "cannot remain content with all of Niebuhr's answers, he quite consistently asked the right questions" (p. 231).

Although many readers will want more detailed studies of particular aspects of Niebuhr's thought, including its relation to other patterns of reflection, and will make harder and perhaps deeper criticisms of his thought, they will find in this volume a sound and valuable overview, by no means uncritical, of his intellectual odyssey. In addition to its value for the scholar, the book should also be very useful to students coming to Niebuhr for the first time.

JAMES F. CHILDRESS

University of Virginia

The Logic of Science in Sociology. By Walter L. Wallace. (Aldine Publishing Company, 1971. Pp. 139. \$6.00.)

In this extremely compact book, Walter Wallace lays out the ingredients of the scientific enterprise. He reviews the main components of science—observations, empirical generalizations, hypotheses, theories, and the rest—in a very lucid manner. In addition, by using an ingenious map that sets these components in relation to one another (figure 1, p. 18), he is able to trace the complex interactions among them. Most of the text consists of a recitation of the norms to be observed in measuring, developing propositions, constructing theories, and testing. Though abstract, the book is exceptionally clear for the most part; it is an ideal theoretical-methodological primer that might well be assigned reading for the first week or two of first-year graduate courses in both theory and research methods.

The work is scarcely original. It relies heavily, and by and large uncritically, on the received wisdom of theoretical methodologists such as Blalock, Braithwaite, Kaplan, Merton, Nagel, Popper, Wartovsky, and Zetterberg. Nevertheless, Wallace is so skillful in selecting from and synthesizing the ideas of these men that the volume, despite its unoriginality, stands as a useful and unencumbered exposition of the essentials of the scientific method.

One of the strongest features of the book is Wallace's insistence on the distinction between (a) the standards by which a theory is to be assessed, and (b) the biographical experience

of the scientist. Thus, on pp. 16–17, he summarizes the basic relations among the components of scientific knowledge (for example, between empirical generalizations and theory) but quickly adds that, in the scientist's experience, there are a variety of ways of arriving at these relations—by lucky guesses, by formal derivation, by trial and error, by teamwork, and so on. Many discussions of science are obscured by the mistaken assumption that the specification of the norms by which scientific knowledge is evaluated constitutes an idealized description of a scientist's behavior. Wallace does not fall into that error.

Early in the first chapter, Wallace, following Montague, distinguishes among the authoritarian, mystical, logico-rational, and scientific modes of testing the truth of statements. In particular, he indicates that the logico-rational model depends on the "rules of formal logic," whereas the scientific mode depends on "the observational effects of the statements in question with a secondary reliance on the procedures (methods) used to generate them" (pp. 12–13). This distinction between "logico-rational" and "scientific" later turns out to be somewhat misleading, because in his subsequent discussion of theory, Wallace repeatedly stresses criteria of logical structure as important in assessing the *scientific* adequacy of knowledge. The distinction between logical and observational criteria for truth is a valuable one, to be sure, but Wallace should have avoided any implied identification of the scientific method more or less exclusively with the latter.

Wallace's discussion of empirical generalizations and prediction in science would have been improved by a discussion of the *conditional* character of most scientific knowledge. For example, in discussing the functions of scientific theories, he indicates that "theories can predict empirical generalizations that are still unknown" (p. 57). That formulation leaves something to be desired. It suggests that scientific theory generates generalizations that can then be discovered in nature. It seems to me, rather, that theories provide the investigator with a knowledge of the conditions under which empirical regularities may be expected. The empirical regularities may have been perfectly well known all along; it is their new significance that scientific theory underscores. Furthermore, these regularities may never have occurred empirically, as Wallace's quotation suggests; and if they have, then this may have been only in a laboratory under which the special conditions appropriate to the theory can be created. If

Wallace had considered the use of experimental and other types of controls in generating scientific knowledge, he would have strengthened his account of the nature of empirical generalizations and predictions.

In general, the exposition is carefully constructed and consistent. It is occasionally marred, however, by carelessness. As examples: (1) On p. 63 and again on p. 66 Wallace stresses that a scientific theory should be "non-tautological." He never explains or illustrates this criterion. Nor is the criterion self-evident. Certainly the scientific investigator should avoid making empirical statements that are true by definition rather than by observation; but at another level—the level of formal theoretical structure—there is a sense in which it is *desirable* that a theory be tautological, i.e., that derived propositions are simply different ways of stating the general conceptual relations in a theoretical structure. (2) On p. 78 he characterizes the statement "all human groups are stratified" as a hypothesis. Surely in that form it reads more like an empirical generalization, because any "if-then" relationship—which is characteristic of a hypothesis—appears to be lacking. (3) On p. 99 he speaks of "predicting and testing new observations." The terminology is odd; one predicts events and tests propositions, but according to linguistic convention, does neither for, but rather makes, observations. (4) The word "sociology" deserves little place in the title of the book. The bulk of the text concerns scientific discourse in general and presumably applies to all scientific disciplines; everything that is sociological about the book lies in the few concrete illustrations of general points made about the scientific method.

NEIL J. SMELSER

University of California, Berkeley

Historians on the Homefront: American Propagandists for the Great War. By George T. Blakey. (Lexington: The University Press of Kentucky, 1970. Pp. 168. \$7.00.)

In April 1917, shortly after the United States declared war on Germany, several historians met in Washington to discuss "what History men can do for their country now." The group, which included J. Franklin Jameson, Frederick Jackson Turner, and James T. Shotwell, decided that historians could put their special talents to good patriotic use. Though not every historian could shoulder a rifle, each could surely "aid in supplying the public with trustworthy information of historical or similar character." To facilitate this, Jameson and the others created the National Board for Histori-

cal Service. In addition, the Committee on Public Information and the National Security League recruited historians for essentially the same purpose.

In *Historians on the Homefront* George T. Blakey describes the activities of these organizations in considerable detail. His account, while narrow in focus, is clear, judicious and straightforward. It is based on extensive research not only in published sources, but also in the records of the National Board for Historical Service and the Committee for Public Information, and in the papers of historians who took part in the war mobilization. Not surprisingly, Blakey finds that historians "succumbed to the pressures of national bias and placed war aims above scholarly restraint," that "their propaganda work represented a severe break with scholarly historical standards" (pp. 104, 148). Blakey concludes that there is no way of reconciling the canons of scholarship with the demands of patriotic service.

Most of the book consists of an elaborate proof of this argument. The author devotes separate chapters to the historians' pamphleteering activities, public lecture tours, efforts at censorship, and attempts to persuade teachers to instill patriotism in the classroom. Various agencies distributed more than 33 million copies of pamphlets written by historians, most of which, Blakey holds, "reduced war issues to black and white, infused idealism and righteousness into America's role, and established German guilt with finality" (p. 52). Moreover, scholars vouched for the authenticity of most of the "Sisson Documents," which purportedly proved that the Bolsheviks were in league with the Germans but which were later exposed as fraudulent. Other historians suggested that public-school teachers could make the study of ancient times relevant to the world of 1917 by pointing out that "ancient Caesarism and imperialism are living forces in Germany today."

Yet Blakey points out that many historians, including some who contributed to the propaganda barrage, had serious doubts about what was happening. Professional historians, who had themselves rebelled against a school of amateur filiopietists, and who took pride in their commitment to scientific method, recognized the potential hazards in government service. One Wisconsin professor, for example, warned that making inaccurate historical analogies in order to stimulate patriotism would play "into the hands of the pseudo-historians." Still another scholar wrote his former mentor: "I have often wondered what you would think of this kind of work—propaganda work—whether a

scholar should do it or not." Andrew McLaughlin of the University of Chicago expressed a similar ambivalence: "Perhaps few of us are able entirely to disentangle our scientific historical fibres from our swelling patriotic muscles, but most of us can try."

In 1917 many scholars supposed such a feat was possible, but few would ever think so again. With the wave of revisionism in the 1920s, those who had echoed the wartime line of Allied innocence and German guilt came under fierce attack. One of the sharpest critics, Harry Elmer Barnes, had himself worked with the National Security League during the war, and might in part have been seeking "intellectual absolution for his propaganda sins" (p. 133). Blakey notes that historians did not repeat their mistakes during World War Two. Having once been burned, they became battle-shy. Indeed, Blakey finds in the World War One experience a warning for all who would combine scholarship with political activism: "The craft of history since 1918 has become a very delicate art, producing a durable commodity only when cultivated in a political vacuum" (p. 151). Surely not everyone will accept this view, but Blakey has shown that a strong case for detached scholarship can, in fact, be made by studying the classic case of involved scholarship.

RICHARD POLENBERG

Cornell University

The Role of the Military Professional in U.S. Foreign Policy. By Donald F. Bletz. (New York: Praeger Publishers, 1972. Pp. 338. \$16.50.)

It is usually fruitless to quibble about book titles, but the prospective reader of this volume should be warned that the title is misleading. It suggests that the book will reveal the part actually played by military officers in determining U.S. foreign policy, perhaps explaining how the Pentagon brass led us into Vietnam, for example. We do not find that kind of revelation here. The author is not concerned with showing us how military professionals have influenced the course of U.S. policy but with arguing that high-ranking officers in the Army, Navy, and Air Force should be trained to understand and appreciate the political factors in military decisions. Without saying so specifically, he implies that the days when a hard-fisted bomber pilot like General LeMay could rise to the highest level of responsibility in the U.S. government are gone for good. In the future we are more likely to see as members of the Joint Chiefs of Staff officers who have not only graduated from

the National War College but have also earned the Ph.D. in political science. In the meantime, it is refreshing to see a book by an Army colonel open with a quotation from Plato's *Republic*. Perhaps it is a sign of the times.

With the nation and its armed forces now going through a painful period of self-examination, serious studies of this sort by military officers should be welcomed by the academic community. Their shortcomings as seen by professors of political science ought not to be allowed to obscure their merits. An Army officer with thirty years of service in a wide range of assignments who is now conscientiously attempting to draw some conclusions from it all deserves to be listened to. The author entered the U.S. Army as a private during World War II and has now risen to the rank of colonel. The Army encouraged his educational interests and made it possible for him to attend civilian graduate schools. He is now on the faculty of the Army War College at Carlisle Barracks in Pennsylvania and is a Ph.D. candidate in international studies at American University.

Colonel Bletz begins his analysis with the observation that since World War II, military officers have been called upon more than ever before to deal with political problems. A new dimension has thus been added to military professionalism. Mere military proficiency is no longer enough—if it ever was. The overall objective of the volume is, in the author's words, "to examine in detail the extent to which the requirements of this new dimension have impacted on the military profession in order to make a valid judgment on the degree to which the national interest is being served and to recommend courses of action for the future" (pp. 8–9).

Much of what follows this statement of objectives is not of great interest to civilian readers, and much of it is already familiar to students of American military history and foreign policy. The major theme of the book is the development of the military profession (and military schools) from the days of the Indian wars to the Cold War era, with emphasis on the military officer's role in implementing national political decisions. The main problem we now face, as Colonel Bletz sees it, is how to give officers in the higher grades more training in the social sciences without diluting their military expertise. At the same time we need to organize the decision-making process of the federal government so that well-qualified military professionals can contribute to the process.

Colonel Bletz is greatly concerned that the "politico-military equation" be properly bal-

anced. By this he apparently means that both political and military factors must be taken into account in proper proportions when we determine national policy. Virtually no one would dispute such an obvious principle; it is in its application that the difficulty arises: What are the proper proportions of each element in the equation in specific situations? What were they in Vietnam, for example? The book does not shed much light on such matters.

As for Vietnam, Colonel Bletz concludes that at the lower level of military professionalism—strictly military expertise—the war was well fought. The weakness came at the higher level of professionalism—politico-military expertise. The military techniques used in Vietnam, we are told, were frequently inconsistent with the political objectives. The “politico-military-equation” was apparently not balanced. Unfortunately, we are not shown in specific terms just what the trouble was.

In the future, Colonel Bletz hopes, the political science now being studied in the military schools will have greater influence on policy. Future generals and admirals will have some understanding of political ideas. But the skeptical reviewer cannot help wondering *whose* political science will have influence. On Vietnam, for example, will we be guided by Hans Morgenthau or Robert A. Scalapino? Our speculation on that point comes to an abrupt end on page 265 when we read Colonel Bletz’s cautious conclusion that, “it is not the business of the professional military officer to make political decisions” nor even to make recommendations of a political nature. All he can do is “ask the relevant questions” and hope that the “politico-military equation” will somehow be balanced by the civilian authorities.

HARRY C. THOMSON

De Paul University

Silent Politics: Polls and the Awareness of Public Opinion. By Leo Bogart. (New York: John Wiley & Sons, 1972. Pp. 250. \$9.95.)

This book deals with a complex of important matters. There are in it a great many facts—more specifically, descriptive statements of processes, findings, and uses for political survey research in contemporary America. Moreover, one gets the feeling in reading the book that Bogart could, might, is just about to, bring it all together in the context of social-psychological and sociological theory and from the perspectives of the scholar-journalist that he is.

But he doesn’t.

So it turns out to be a book like most books, falling far short of greatness.

First, review-fashion, a word about content. The title is not especially helpful. The book is a series of essays on the uses to which political polling is and might be put, from the perspectives of candidates/incumbents, citizens (active and passive), and media managers. The dilemmas of leadership, participation, representation, and change processes are observed and commented on in various places through the twenty-one (for some reason unnumbered) chapters and eight “parts.” The first two parts and the last agitate the old questions of the place of commonpersons’ opinions in direct and representative democracies.

The middle five parts of the book deal with opinion change at both the individual and social levels. Many interesting facts and some provocative commentary are presented in chapter headings like “Changing One’s Mind,” “The Causes of Inconsistency,” “Opinion Under ‘Proletarian Dictatorship,’” and “Shocking Opinions.” But things fall apart. The middle does not hold—nor, for that matter, do the ends very well.

The book is strewn with insights and intellectually titillating assertions—that polling is *essentially* conservative (p. 60), that extreme views are valuable in a democracy because they expand the possible range of middle movement (p. 101), that anonymity and opinion-inconsistency are positively related (p. 132)—but they are hardly ever dealt with systematically or in context. On the positive side, however, the work is a cache of occasional information of journalistic and commercial poll data of a kind that someone like Bogart has access to but we in the more sheltered academic world do not.

Yet, as I say, the work doesn’t come off as a *work*. It is an unsatisfactory book because it doesn’t seem to have any design and because it’s not thorough enough. Bogart never makes clear what he’s trying to do in the book as a whole; he gives only a vague hint in the last paragraph of his preface. He never articulates his views about what the relationship should be between popular preferences and public policy. In places a kind of simple direct-democracy seems to underlie his views of democratic theory; but in other places he seems to understand that public policy is never static and therefore that even if we knew what to do today, we wouldn’t know what to do tomorrow, that survey research is a tool for leadership (a scalpel or a dull ax) and not a mandate, and that the media can also use opinion research in a variety of self-serving and other-serving ways. But these and many other critical matters are more alluded to than discussed. Examples there are

aplenty, but there is never any sustained commentary.

Bogart has not sorted out and clarified the level-of-analysis-problem. He moves carelessly from the individual to the social levels—even, on page 76, carried away, personifying opinion, saying “. . . it responds to drama, it heeds the appeals of respected leaders, it cringes at the prospect of disaster, it is bored by the repetitious and the inconclusive.” Again, there are lots of hints and examples of linkage between individual opinions (or processes) and aggregate opinions, but they are not tied together or put into any context. On a different but related matter, that of the pervasive publicness of political elites, his short chapter “Private Opinions and Public Roles” is rich with illustrations but has almost no analysis at all.

Finally, it's unclear for whom the book is written. For students and scholars, no doubt; also for journalists and media managers; and, one supposes, for the attentive citizenry who care about American democracy in practice. Because there are a lot of interesting facts, and examples of survey use/misuse, and because there are many intellectually-provocative one-liners, all three presumed audiences will find it of some worth; but because of its designlessness all three will also find it less than fully rewarding.

BERNARD HENNESSY

California State University, Hayward

The Party's Over: The Failure of Politics in America. By David S. Broder. (Harper & Row, 1971. Pp. 280. \$7.95.)

An able and liberal political journalist here joins the ranks of those alarmed by what they understand to be the declining health of the American polity. Broder assigns the responsibility mostly to an unresponsive and deteriorating political process, and offers as his chief remedy the reinvigoration of responsible party government. His writing, conveying honest anxiety and earnestness, nicely combines moderation of tone, constructive criticism, and gravity of content. But the ground traversed is by now excessively familiar, and the remedies proposed—though Broder is not optimistic about their realization—are from an earlier era and are neither well thought through nor clearly related to the ills diagnosed.

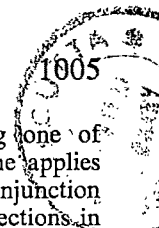
Our crisis, as seen by Broder, reflects not merely the severity of escalating unsolved problems but a loss of popular confidence in government, especially by blue-collar workers, minorities, and college students. This political malaise feeds on the failure of political parties

to perform in our time their historic function of clarification and unification for the citizenry: “The governmental system is not working because the political parties are not working” (p. xxiii). The reasons and/or symptoms include divided party government under Eisenhower and Nixon; constraints on presidential initiative stemming from narrow election victories, as with Kennedy in 1960 and Nixon in 1968; the personalized presidencies of Eisenhower and Johnson, each committed to different types of consensus politics hostile to effective party development; and increasing voter independency of party and willingness to split tickets. Above all, the pluralistically fragmented governmental structure and process, which gives some interests a persistent advantage, has failed to promote grappling with America's pressing problems.

The range and gravity of the crisis sketched by Broder contrasts strongly with the considerably narrower political process “causes” he emphasizes. Further, the process reforms advocated by Broder turn almost entirely on the notion of the political parties as “the instrument of national self-renewal” (p. xi). He endorses the thrust of the 1950 report of the APSA Committee on Political Parties, “Toward a More Responsible Two-Party System,” and in full awareness of its rejection by most political scientists. But Broder does a poor job, logically and empirically, in buttressing his preference, especially since he is fair in recognizing many of the durable and ongoing obstacles to party government in America.

Like many other commentators on the topic, Broder confuses reforms which strengthen presidential leadership with those which facilitate party government; the implicit assumption that the former must serve the latter is wrong. Similarly, developing party-based stronger collective leadership for Congress may or may not contribute to a more centralized and disciplined national party. As for Broder's enthusiasm (as of 1971) for the Democratic party's convention “reforms” as an important step toward greater party responsibility, it suggests not so much a flawed crystal ball (a common disability) as a meager understanding of what party government means and requires. In keeping with Broder's stubborn resistance to the conclusion that the times are increasingly unsupportive of the kind of tight, responsible two-party system he envisions as a panacea, he pays little attention to such decentralizing trends as separatism and community control.

Beyond the questionable consistency of major parts of Broder's prescriptions, there is the



larger problem of whether responsible parties could or would attend to resolving the major ills of the polity. Perhaps so, but Broder is better at asserting the linkage than at arguing or proving it. At bottom, then, the study is more successful in conveying a sense of alarm and urgency to readers about the necessity for change than in persuading them of the necessity, desirability, or feasibility of the particular process reforms urged by Broder.

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Critical Elections and the Mainsprings of American Politics. By Walter Dean Burnham. (New York: W. W. Norton & Company, Inc., 1970. Pp. xii, 210. \$6.50.)

This is a puzzling book. It starts off with regression-based discontinuity coefficients and t-tests applied to series of American election results, and it finishes with statements like the following, "The evidence points overwhelmingly to the conclusion that the American polity has entered the most profound turning point in its history" (p. 188).

I have read this book twice during the last two years, and while I am duly impressed and provoked by the conclusions, I must admit I still do not fully understand how Professor Burnham arrived at them. There are many statements in the book that suggest profound thinking about the relationship between democratic institutions, the social and economic order, and popular control of public policy. The empirical base derives largely from the analysis of aggregate voting patterns in the nation as a whole and in selected states and localities, as well as from some survey data on party identification. I do not so much find fault with the data analysis, which is often suggestive and insightful, as with the premises for the connections with Professor Burnham's overall conclusions.

Much of the book flies much closer to the ground than the author's concluding statements suggest. The first three chapters constitute a study of past realignments. Burnham begins by offering a definition of critical realignment, which he seems to mean as empirically derived from the common characteristics of previous examples. But unfortunately, his standards for the definition are not clear, and he makes no attempt to show systematically that the commonly recognized realignments had all or even most of these characteristics.

The closest he comes to relating a portion of his definition to empirical data on the past is in Chapter 2, "The Periodicity of American Crite-

cal Realignments"—periodicity being one of the elements of his definition. Here he applies the "discontinuity coefficient" in conjunction with a t-test to a series of national elections in order to identify critical realignment years; and finds that the midpoint years 1854, 1874, 1894, and 1930 stand out. He concludes that sure enough, with the exception of 1874, there is a striking periodicity in the data. He does comment on the marginal values of the coefficients for 1854 and on the very high values for 1874, but he does not make it clear whether we have discovered a new major realignment in 1874 or whether we have merely confirmed his observation about periodicity. It would have helped if he had indicated more clearly what his data were meant to show and if he had done comparable empirical studies of the other elements of his definition. Those who look to this book for a contextual treatment of the causes and consequences of past realignments will probably be disappointed. Yet this seems not to have been Burnham's intention, and he does have more to offer.

The remainder of the book is the most interesting part, in my judgment. Burnham discusses the rules changes and "systemic forces" of the "System of 1896" and their consequences in terms of reduced turnout and reduced partisanship. The rules changes were "devices by which a large and possibly dangerous mass electorate could be brought to heel and subjected to management and control within the political system appropriate to 'capitalist democracy' . . . a remarkable solution to the problem of adjusting mass politics to the exigencies of industrialism" (p. 90). This is a pregnant statement, and I am prepared to be convinced of its validity. The consequences of some of these changes for the political participation and power of the southern black are well known. Yet if Mr. Burnham has clear ideas on what these changes prevented the total electorate from doing to the capitalist system, or what the voters were prevented from doing for themselves, we would all be enlightened if he spelled them out.

"The Onward March of Party Decomposition" is traced, with some interesting tangents to changes in the nature of political campaigns and the institutionalization of the House of Representatives. Burnham does not know whether the decomposition of party is part of a pre-realignment process or whether it will make realignments as we have known them impossible in the future. He clearly finds the trend dangerous because parties are "the only devices thus far invented by the wit of Western man which with some effectiveness can generate

countervailing collective power on behalf of the many . . . powerless against the relatively few who are . . . powerful" (p. 133).

Burnham finds the foreseeable realignments almost as troubling as the continuing decomposition of party. Several possibilities seem to him to have "as large a civil war potential . . . as any critical realignment in our history" (p. 170). If this is the case, the absence of a crystallizing factor is doubtless fortunate, especially since "it is probable that any force great enough to produce 'classical' realignment under present conditions would also be great enough to break the system altogether, with consequences of incalculable magnitude" (p. 173). If the lack of party identification is to save us from such a realignment, the consequence may be the termination "of the historical role in reshaping our institutions and our political system which has been played by the American electorate from time to time" (p. 174). The book concludes with the observations that "a decisive triumph of the political right is more likely than not to emerge in the near future," or perhaps an *Obrigkeitstaat* along the lines of Imperial Germany (pp. 192-3).

This is a profound book, and whether he is right or wrong, Burnham has much to tell us. The book is like an iceberg, with much remaining below the surface. I wish the terms of the argument by which he arrives at his conclusions were clearer and lent themselves more easily to evaluation. On the other hand, when one contemplates the gravity of the conclusions, one can understand why he may have wanted to publish before all of the logical connections of his arguments were spelled out.

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Regional Economic Development: the Federal Role. By Gordon C. Cameron. (Washington, D.C.: Resources for the Future, Inc., 1970. Pp. 161. \$4.00.)

The Fiscal Organization of American Federalism. By Richard E. Wagner. (Chicago: Markham Publishing Company, 1971. Pp. 119. \$7.50.)

Although these two books have in common the subject of federalism and various policy issues of the United States federal system, they are so different that I shall discuss them separately.

Wagner's book is one of a number of recent technical economic studies devoted to the problems of collective choices respecting private

versus public goods, and types and amounts of public goods. The central problem is the familiar one of obtaining possible gratification of individual preferences in collective decisions. "If majority coalitions are wholly unconstrained, majority voting will tend to produce socially wasteful budgetary choices" (p. 13). The larger the governmental jurisdiction the greater the probability of budgetary inefficiencies. With no space to discuss the argument, I jump to several important conclusions:

(1) There should be greater flexibility to reshape subnational governmental jurisdictions. On efficiency grounds, "if a group of citizens within one state want to form a new state," . . . or, "if a group of citizens within an area incorporating parts of two or more states want to form a new state, they should be permitted to do so" (p. 64). For "the mere availability of the option of secession produces an equalization of bargaining power and protects against geographical discrimination" (p. 65). The propensity of wealthier citizens for forming enclaves to escape contributing to the support of poorer citizens can be overcome by making income redistribution primarily a national responsibility, which Wagner advocates.

(2) The more comprehensive the government and its budget, the lower the degree of taxpayer control; "consolidation diminishes taxpayer perception of changes in the price of any single budgetary component and increases the immunity of politicians to competitive pressures" (p. 53).

(3) Contrary to conventional wisdom, governmental consolidation within metropolitan areas therefore should be viewed with suspicion. Once the income redistribution function is taken over by the national government, budgetary efficiency becomes the main objective of local governmental organization, and in Wagner's view efficiency can be better attained by many smaller jurisdictions than by one or a few larger ones. Inefficiencies of small jurisdictions having to do with economies of scale and externalities can be handled by negotiation among jurisdictions or by contracting out.

(4) Of the various forms of federal grants, only unconditional grants satisfactorily meet efficiency tests. The prevailing multiplicity of conditional grants frustrates efficiency in many ways. As for block grants, "If revenue maintenance is not required, [they] are indistinguishable from unconditional grants. If revenue maintenance is effectively practiced, block grants degenerate into a particularly oppressive form of conditional grant" (p. 59).

These are provocative conclusions. It is not

deprecating Wagner's scholarship to say that many will disagree with much of their content, for much of the argument rests upon the primacy of individual preferences and decisions and a view of individuals as economic hominids, with the knowledge and inclination to follow the intricacies of making budgets, producing and delivering public goods. In the real world, most people pay little attention to local government and the processes of providing services other than to squawk when services fail.

Wagner falls into an error, common to economists, of assuming that small constituencies are better public decision makers than large. There is considerable evidence that in the real world, smaller jurisdictions, which according to American mythology are the representatives of democracy in its purest form, tend to be run by small cliques, to resist innovation, to have poor communications among constituents, to make decisions unsystematically, and otherwise fail to meet the conventional tests of "good government."

In short, while Wagner is well worth reading for a well-ordered presentation of one school of economic thought, many of his most important policy conclusions are infeasible and many, in this reviewer's opinion, are unsound. They are unsound not because of faulty reasoning but because of his premises, which reflect far too simple views of the nature of government and human beings and their relationships to society.

Cameron's study concentrates on the case for federal intervention specifically to raise employment and income in backward and lagging domestic areas. The backdrop is the Public Works and Development Act of 1965, the administrative issues created and left unresolved by that Act, and the subsequent operating experience of the Economic Development Administration (EDA) through 1969.

"Rapid national growth," says Cameron, "has not eradicated severe and persistent imbalances between the demand for labor and its supply in literally hundreds of small and medium-sized labor markets in every U.S. region" (p. 127). He lists four categories of areas: (1) rich labor-surplus areas in which massive immigration has caused labor market disequilibrium; (2) old industrial urbanized areas with ossified export structures; (3) rural areas still relatively productive, but with heavy unemployment caused by substitution of machinery or other technology for farm labor; (4) the chronically poor, unproductive and worked-out mining areas, characterized by low per capita income, often heavy unemployment, and large out-mi-

gration. The justification for federal action to relieve economically backward and distressed areas is that the federal government is the only instrument fiscally able to help them.

The need for assistance to at least the first three categories of lagging areas is clear enough, and clearly only the federal government can intervene effectively. Whether the federal government, given its own political and economic constraints, including serious knowledge gaps, can accomplish much is another, largely unanswered, question.

One of Cameron's major contributions is a thorough analysis of various alternative policy measures. He concludes that one of the most promising approaches is to concentrate resources on stimulating regional development centers—middle-sized cities which can be the nuclei for economic agglomeration. Concentrated investment is likely to have several advantages over the same amount of investment more spatially diffused over a region.

A second valuable contribution is the review of EDA policies and programs. Cameron gives the agency good marks, on the whole, for imagination and vigor. It was greatly handicapped by the necessity of placating congressmen by scattering its limited funds widely, and by the narrow range of its development activities.

I have two main criticisms of the study. First, the analysis is almost entirely abstract—of the "on the one hand this, and on the other hand that" variety, which demonstrates the complexity of formulating policies but does not lead to many firm conclusions. It offers few data and no illustrations or case studies. Cameron is highly respectful of the overwhelming power of "market forces," but doesn't adequately identify or analyze them. Much of the analysis appears implicitly to be based on manufacturing, though the author recognizes that manufacturing is now a slow-growth industry. There is little discussion of the implications for future regional development of current economic trends, particularly the rapid growth of tourist, vacation, retirement and other service industries. The case of Vermont (where this review is being written) illustrates how a state lacking resources and industry, but with the wisdom to preserve its environment, can evolve into an area of such great attractiveness that one of its principal concerns is deterring undesirable growth.

The second major criticism has to do with Cameron's failure to relate regional underdevelopment to the lack of any considered national policy respecting economic growth and the location of population and industry. As he

notes, some of the greatest problems have stemmed from federal behavior in encouraging the concentration of highly specialized centers (as in defense and aerospace industries) which are then devastated by shifts in federal priorities. One gathers that the federal decisional process is one of the main economic forces alluded to by Cameron, and that for reasons intrinsic to the federal establishment it is little better directed at reducing economic dislocation than are other economic forces.

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Lawyers Before the Warren Court: Civil Liberties and Civil Rights, 1957-66. By Jonathan D. Casper. (Urbana: University of Illinois Press, 1972. Pp. xi, 221. \$9.50.)

Jay Casper has written a fascinating book about lawyers in private practice who argued reapportionment, civil rights, criminal procedure, and loyalty-security cases decided by the Supreme Court with full opinion. He also provides an excellent concise review of the substance of litigation decided during the 1957-66 period of "activism and progress," the dominant trend of which was "toward equality and freedom" (pp. 18, 19), showing the Court's spotty record in the loyalty-security field and its unwillingness to go beyond narrow grounds in the sit-in cases, as well as its better-known criminal procedure shift from case-by-case to broad rule. Substance and process are joined in another theme, the conjunction between participants' goals and policy adopted, with contrasts between narrow grounds urged and broad policy adopted in criminal procedure, and broad, multiple grounds urged and narrow decisions handed down in the sit-in cases. The Court's role as a conflict-generating institution is also stressed: the Court opens or reopens issues for others, setting their agenda and refusing to let them avoid issues.

Casper is principally concerned with a lawyer's *clientele*, the group or interest a lawyer perceives himself as serving (his most salient reference group); this clientele may or may not be his particular client and may not be involved in the case at all. Agreeing with this choice of concept, I think he too quickly dismisses *role* as an alternative concept because it is undifferentiated. One could ask whether a lawyer representing a group was expected by relevant others to behave differently from one interested in representing only a specific client, important, given the controversy over NAACP and ACLU priorities in representation of individual clients.

Casper's categories are the Advocate, inter-

ested primarily in his particular client and in short-term considerations; Group Advocate, focusing on a particular group over the longer term; and Civil Libertarian, interested in broad, enduring social principles. Because the categories seem to be established from lawyers' goals, the author stresses that, although empirically related, (reported) goals and social interests served are conceptually distinct. Casper also distinguishes between lawyers' views on the roles of law (conflict-resolution and social control versus social change and sustaining democracy) and the bar (passive or active). Most lawyers either believe in law as a resolver of conflict and a passive bar (Traditionalists) or in law as a force for social change and active bar (Reformers). Most Advocates are Traditionalists, while Group Advocates and Civil Libertarians are mostly Reformers. The author makes good use of analysis of those who do not fit this pattern. Thus we see that several black civil rights lawyers were Traditionalists and that the experienced criminal lawyer may get interested in recurring issues *qua* issues even though he doesn't have a commitment to his clients. We also find out about the effect of ideology in the choice of career pattern, the question of who will be appointed in criminal cases, and about members of the Radical Bar and lawyer-politicians in reapportionment cases.

Areas of litigation differ in recruitment patterns and lawyer types. Lawyers in criminal procedure cases, predominantly Advocates, entered a case by court appointment at various stages of the case. Most lawyers arguing reapportionment and loyalty-security cases, Group Advocates or Civil Libertarians, were involved from the initial trial; one-third of the civil rights case lawyers did not enter until the Supreme Court level. Because of their ideology and the Court's stress at the time on both the facts of a case and the "totality of circumstances," Advocates generally argued technical grounds. This was reinforced by their clients' high personal stakes—long prison sentences or death. Advocates argued broad points to win cases, not because they wanted the policy; thus the Court's broad criminal policy was an unintended by-product of the Advocates' goals, reminding us not to infer motives from results.

The Group Advocate, perceiving in "we-they" terms, had personal, ideological, and institutional contacts with his clients, resulting in a personal commitment to them not shared by the Advocate and Civil Libertarian vis-à-vis their clients. Lawyers entered reapportionment and loyalty-security litigation primarily through friendship; they entered civil rights cases

mostly through group contacts. In the early loyalty-security cases, Radical Bar lawyers had both personal and ideological ties to their clients; the Civil Libertarians who took the later and safer loyalty oath cases had neither. Group Advocates, ambivalent about grounds to raise, may, as in the reapportionment cases, use broad ones for pragmatic reasons, but Civil Libertarians would be interested in them for abstract reasons. Casper also suggests that Group Advocates can be counted on in times of repression, whereas Civil Libertarians may be "fair weather friends."

Most of the cases Casper's lawyers argued were decided before a development attributable in large measure to decisions like *Gideon*: the recent substantial growth of public defender offices, which Casper suggests will produce more lawyers concerned about general problems of the disadvantaged. Yet one wonders. Institutional demands on public defenders can blunt their idealism and lead to increased plea-bargaining, "package dealing," and indifference to clients. In his balanced speculation, Casper recognizes that these results may occur at the trial level, but he suggests that frequency of involvement with issues might lead to stressing broader policy matters and that appellate public defender divisions would be more inclined to raise the large issues.

In this volume, in which one needs to look hard for something to criticize, Casper has given us an important addition to our knowledge about the lawyers' role in the policy process, and, because of the "clientele" focus, a contribution to the group theory of the legal process. Because of the study's importance and the questions it raises, it is sometimes difficult to keep in mind that only the Supreme Court is covered. What we now need is a replication for the post-1966 period and interviews both of Casper's respondents about other cases and of lawyers who petitioned for *certiorari* but were unsuccessful in getting to oral argument before the high court.

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Participation in American Politics: The Dynamics of Agenda-Building. By Roger Cobb and Charles Elder. (Boston: Allyn and Bacon, Inc., 1972. Pp. 182. \$3.50.)

Political scientists frequently use but rarely study the concept of the political agenda. While notions such as gatekeeping, demands, mobilization of bias, and nondecision making are familiar conceptual wares, the explicit questions of how and when problems gain agenda status

have, for the most part, not been treated systematically. Roger Cobb and Charles Elder offer an introductory framework and a useful perspective on how problems become political issues and assume their place on the political agenda. The two political scientists critically evaluate system, power, decision making, and group approaches, and then they construct their own group conflict paradigm of agenda building. Though the paradigm is probably of only modest value, Cobb and Elder successfully sort out, define, and examine many of the important questions about agenda building.

Perhaps most noteworthy is their distinction between two basic types of political agendas—systemic and institutional. The systemic agenda refers to issues that are defined by members of a political community as warranting government action. Cobb and Elder suggest three prerequisites for issue access: (1) widespread attention or at least awareness; (2) view that action is required; and (3) view that action is appropriate for government. The institutional (or formal) agenda consists of those sets of items that are explicitly up for consideration before a governmental body.

After reviewing four major approaches to agenda building as noted, Cobb and Elder, adopt a group conflict approach, which hypothesizes that political issues result from procedural or substantive conflicts between two or more identifiable groups for either scarce resources or scarce positions. And borrowing heavily on Coleman and Schattschneider, the authors view conflict as a dynamic process.

Cobb and Elder distinguish five characteristics of issues that influence their likelihood of reaching the public docket. Adapting Parsons's pattern variables, they focus their definitional dimensions on the nature of the issue conflict. They point to the scale of groups—organizational skills, extensity and intensity of communications, size, resources—as of major importance in issue expansion. In addition, they elaborate major roles the audience can play in the expansion of issue conflicts.

Cobb and Elder devote several chapters to issue expansion. Their basic position is that "the greater the size of the audience to which an issue can be enlarged, the greater likelihood that it will attain systemic agenda standing and thus access to a formal agenda" (p. 110). They discuss the types of symbols used to enlarge the scope of a conflict and the crucial role played by mass media in terms of various strategies to enlarge controversy and muster support.

Cobb and Elder do not foreclose inquiry into agenda-building. Their general paradigm, while

suggestive, is largely a catalogue of possible ways in which a problem may become a political issue. Few research studies are cited. While one chapter is devoted to case examples, it is primarily discursive and reportorial—and probably the book's most unsatisfactory chapter. As the authors recognize, the framework lacks "systemic elaboration and rigorous empirical inquiry" (p. viii). The paradigm offers ideas and hypotheses rather than ready-made explanations or research designs. Also, the exclusive focus on group conflict raises many of the familiar criticisms of omission often leveled against "group theory"—the failure to recognize, for instance, the role and importance of other political forces such as the executive, legislature, bureaucracy, and political parties.

The analysis of Cobb and Elder primarily centers on how issues achieve systemic agenda standing. But because they do not delineate or propose measurement methods, it is not clear how students of agenda building can operationally distinguish among systemic issues. As Charles Jones points out, many problems—e.g., race or education—are highly interrelated and represent a "bundle of actual problems." And in my view, rather than suffering from a lack of access, the systemic agenda seems to be overcrowded with issues. Cobb and Elder acknowledge that recognition on a party platform is indicative of an issue's having attained standing on the systemic agenda. If so, the 1972 Democratic and Republican platforms provide ample evidence of our bountiful systemic concerns. Moreover, television has transformed the living room into a public arena, introducing the viewer to an overwhelming number and variety of systemic issues. Attention to agenda building should perhaps focus more heavily on the formal agenda and the influences on the range and types of policy alternatives for issues considered by "authoritative decision makers."

The exclusive focus on group conflict means that other major sources of agenda issues are not examined. For one, the mass media are more than passive intermediaries for the interplay of interest groups. As Murray Edelman suggests in *The Symbolic Uses of Politics*, politics for most of us is a "passing parade of symbols." The mass media play a pivotal role in selecting, highlighting, and developing the symbols of politics as well as acting as "fourth branch of government" brokers for public and private decision-making groups. And second, government bureaucracies are a primary source for agenda items, systemic as well as institutional. These two examples underscore the conclusion that interest-group conflict is only one

approach to agenda building; others such as general systems and decision making may offer equally useful paradigms for how problems become public issues.

Despite shortcomings, Cobb and Elder have written an excellent overview of agenda building. Examining questions largely neglected, the book should be a notable stimulus to explanations and research on the "dynamics of agenda building." It should be read with particular interest by students of public opinion and participation.

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The Apportionment Cases. By Richard C. Cortner. (Knoxville: University of Tennessee Press, 1970. Pp. xii, 283. \$10.95.)

Reapportionment: Law, Politics, Computers.

By Terry B. O'Rourke. (Washington: American Enterprise Institute for Public Policy Research, 1972. Pp. 111. \$2.00, paper.)

It seems the fate of movements for reform in American government to be oversold. Perhaps this tendency can be traced from Henry George's single tax, through Bryan's free coinage of silver, to the "reapportionment revolution." But reapportionment is different, for the reform was adopted, and early results are in. Some judgments can be made. One remembers in wonderment the excitement of the middle 1960s, when the National Municipal League rushed to publish reapportionment decisions made by the most obscure courts. The Supreme Court had spoken, and "one man, one vote" in the state legislatures was the first step toward urban salvation.

These two volumes are based on different assumptions and written from different historical perspectives. Their purposes and formats contrast sharply. Yet they complement each other. Professor Cortner uses the term "revolution" approvingly; he is confident that the one man, one vote standard is a landmark in the continuing definition of the constitutional right of equality, and his book tells much of how it came to pass. Terry O'Rourke's voice is that of disillusionment. The Court's rigid mathematical standard has, he feels, created more problems than it solved. In particular, it facilitates "vastly more sophisticated" gerrymanders. Treating representation as involving an individual right—the right to cast a vote that is "undiluted"—the Court is precluded from considering the essence of representation, which deals with the influence of groups and parties.

The title of Professor Cortner's book is

somewhat misleading. Rather than encompassing the total development of case law in the field, he is largely concerned with two landmark decisions. *Baker v. Carr* was the Tennessee case which, in 1962, held that legislative apportionment is a justiciable matter setting aside the "political questions" doctrine of *Colegrove v. Green*. In 1964, the Court chose the Alabama case of *Reynolds v. Sims* as the vehicle for enunciating the one man, one vote standard.

Cortner is interested in the genesis of the cases, the personalities of those who argued and decided them, the routes followed through courts of appeal, the arguments made in briefs, and the technicalities cited to distinguish a particular case from previous ones which established an unfavorable precedent. Cortner recounts such details for these two cases in leisurely but lucid prose. In the process, he tells us a great deal about the politics of Tennessee and Alabama, and we perceive the evil for which a judicial remedy was sought through the eyes of the original litigants. The constitutions of both states provide for periodic, population-based apportionments; both had been last apportioned in 1901. Rotten boroughs had become absolutely fetid. Sixty years and federal court intervention were required before the two legislatures performed their unpleasant, but constitutional, duty.

It is this sense of the *status quo ante* that is missing from O'Rourke's volume. In commenting on the cases, he emphasizes dissenting remarks that prophesy the difficulties now being experienced with the one-man, one-vote standard, which he describes as a "facile and question-begging premise" (p. 16). While O'Rourke's analysis is largely an exercise in hindsight, Cortner's is remarkable for its uncritical praise of the "revolution." Although published in 1970, the book barely notes developments since 1964. Cortner's historical account of the Tennessee and Alabama cases is carefully researched, including extensive interviews with the litigants and their attorneys. Any future scholar interested in these two cases will perforce need to acquaint himself with Cortner's volume. As if to emphasize the book's status, the note describing its type face reports that "the paper on which the book is printed is designed for an effective life of at least three hundred years."

Cortner's final chapter is an able analysis of the conditions under which the Supreme Court may successfully make policy. He compares the apportionment cases, in which the Court was successful, with the desegregation and school

prayer cases, in which the Court's policy depends too much for implementation upon non-judicial actors. This chapter stands alone, for it depends only marginally on the account of *Baker* and *Reynolds* that has gone before. It is recommended as a succinct account of the possibilities and limitations of the Court's role in American politics.

Published as one of the "Domestic Affairs Studies" of the American Enterprise Institute for Public Policy Research, Terry O'Rourke's paperback volume has a slight air of impermanence. Basically, it is a status report upon a still developing issue. It consists of three independent chapters that are logically and chronologically related but lead their author to no general conclusions. In the first chapter, the author describes the *Colegrove*, *Baker*, and *Reynolds* decisions. He next describes the narrowing of the judicial standard to rigid arithmetical equality in 1967 and 1969 and its application to 80,000 units of local government in *Avery v. Midland County*. The author concludes the chapter by suggesting that the Court's treatment of the issue probably precludes it from affording relief to litigants who claim their political influence has been diluted by partisan gerrymandering.

The second chapter describes the political reaction to the *Reynolds* decision, including the abortive Dirksen Amendment and the equally abortive effort of state governments to call a constitutional convention. Then he considers the impact of legislative redistricting upon state governments and finds that the prediction of urban benefits has fallen far short of the mark. Reapportionment enthusiasts oversold their remedy, and, by the time it went into effect, the suburbs, rather than the central cities, were often the beneficiaries. He summarizes research by Dye, Dege, Friedman, Hofferbert, and others, which indicates that the quality of its legislative apportionment is not the only, or even a very important, determinant of the quality of a state's government. He concludes with figures for thirteen large, politically competitive states which demonstrate that the gerrymander is alive and well.

The first two chapters each end by setting the stage for the third, which sketches the history of the use of computers to outline proposed district boundaries, and provides a case study of their use in California. O'Rourke demonstrates—if there was any lingering doubt—that, with the aid of computers, it is a simple matter to draw district boundaries that are absolutely equal in population yet implement flagrant partisan gerrymanders. This can be done even while rendering districts that may be compact

and contiguous and respect local government boundaries. The Republican computer in California was particularly cleverly programmed in 1970.

O'Rourke's book is brief, well written, and hard-hitting. Hardly a definitive account of the reapportionment controversy, it does provide an admirable introduction to the current state of play on the issue. The moral garnered from reading these two books together is that, if we insist on calling the federal court decisions on reapportionment a "revolution," we will be charged by our students with a credibility gap.

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Constitutional Law and Politics, Three Arizona Cases. By Richard C. Cortner and Clifford M. Lytle. (The Institute of Government Research: "Arizona Government Series," 9; Tucson, Arizona: University of Arizona Press, 1971. Pp. 137. \$3.00, paper.)

The Arizona Train Limit Case, Southern Pacific vs. Arizona. By Richard C. Cortner. (The Institute of Government Research: "Arizona Government Studies," 8; Tucson, Arizona: University of Arizona Press, 1970. Pp. 47. \$1.50, paper.)

Great constitutional cases do not arise in a political vacuum, nor is their importance limited to a few principles of law. While political scientists have employed the case study as a device to demonstrate the political functions of the judicial process, there remain too few in-depth analyses of major litigation from the initiation to the impact stage. Political scientists at the University of Arizona currently are mining these raw materials from the political history of their state. The Institute of Government Research is publishing a series of short paperback volumes, each a case study of a major event in the state's history. While these volumes will be of interest to those concerned with interest groups, legislative behavior, and public administration, the two recent volumes commented upon here will be of particular value to scholars and to teachers of constitutional law.

In *Constitutional Law and Politics, Three Arizona Cases*, Cortner and Lytle examine three major civil liberties cases: the *Miranda* case setting guidelines for police interrogation, the *Gault* case attacking problems in the administration of juvenile justice, and the litigation involving the Arizona Loyalty Oath, *Elfbrandt v. Russell*. The authors introduce the legal and political background of each case early, following it by a discussion of how and why

litigation was initiated. They turn next to the strategies and techniques of litigants and attorneys and typically include an encapsulization of the major arguments submitted in briefs before the Supreme Court. The text of the decision is set out—at some length—followed by a brief epilogue.

Although the authors contribute little to our knowledge of *Miranda v. Arizona* (384 U.S. 436 [1966]), the discussion of *In re Gault* (387 U.S. 1 [1967]) is more valuable. *Gault* was the second major step taken by the High Court to reform the nation's juvenile court system. The description of what happened to fifteen-year-old Gault highlights the inadequacies of the system. Of interest is the depiction of how Gault's attorney established a record at the habeas corpus hearing which proved decisive upon review, as well as the discussion of the unimaginative, narrow, and formula-ridden handling of the case by the Arizona courts.

The emotional and practical drain of being the central figures in a major civil liberties test case is illustrated in the story of the Arizona Loyalty Oath Case (384 U.S. 11 [1966]). Of thousands of state employees, only six refused to sign the oath. The Elfbrandts alone saw the case through, having to support themselves during five years of unemployment and seeing that the litigation was subsidized.

Perhaps the most interesting of these studies and the most adaptable for teaching purposes, is the short volume on the train limit case, *Southern Pacific v. Arizona* (325 U.S. 761 [1945]). Professor Cortner lucidly describes the legislative history of the statute, the interest of the Railway Brotherhood in preserving it, the interest of the railroads in voiding it, and gives sufficient information to understand the problems of safety. He describes the many stages of the sixteen years of litigation and provides sufficient data to make comprehensible the relationship of the Arizona litigation to changes in the railroad industry and to litigation in three other states.

These studies, which read easily, are useful for supplementary reading in courses on the Supreme Court, judicial process, and—especially—constitutional law; these courses traditionally have focused too much upon the Supreme Court and upon the dissection of abstract principles and judicial logic at the expense of an understanding of the interrelationship between the courts and other actors of the political system in the constitutional process. (Teachers interested in a casebook integrating the case method with case studies, are referred to the same authors' *Modern Constitutional*

Law, Commentaries and Case Studies [New York: The Free Press, 1971].)

While the Arizona studies are useful additions to the literature and adaptable for classroom use, they tend to be a little thin. One hopes that in any future case studies the authors would offer more about the political genesis of the litigation. The non-Arizona reader could also desire an analysis of the factors in the situation unique to Arizona—the structure of its judicial system, the backgrounds of the judges of its supreme court, the party breakdown in the legislature. The political scientists would profit from a greater attempt to link Arizona events to litigation elsewhere, something accomplished well in the *Southern Pacific* case but not in the other case studies. Perhaps the texts of the Supreme Court decisions, which consume an unnecessarily large portion of the text and which are easily attainable elsewhere, could be sacrificed on behalf of additional analysis. And with growing attention to impact analysis, the all-too-short epilogue ought to be expanded into a much closer examination of the acceptance or resistance by Arizona institutions of the Supreme Court Decision.

Despite some weaknesses of execution, the direction of the scholars of the University of Arizona ought to be emulated by those of us in other states. If their state, one of the youngest "of the several states" has spawned three dramatic cases within one decade, how very many case studies can be cultivated from the gardens in our backyard. One would hope that the work of Arizona's political scientists will stimulate a large yield from elsewhere in the Union.

JEFFREY B. MORRIS

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The Threads of Public Policy: A Study in Policy Leadership. By Robert Eyestone. (Indianapolis: Bobbs-Merrill Company, 1971. Pp. 197. \$8.50.)

Eyestone's book is important in its own right and as an early book-length product of the City Council Research Project, a large-scale survey of 435 city councilmen in 89 San Francisco Bay area municipalities. The resulting data include individuals, aggregates and whole councils in units. In concept, design and the topical importance of suburban politics, the book is part of the growing body of work moving local government studies into the mainstream of political science research. While not ignoring wider economic and institutional factors, Eyestone seeks to focus on a political explanation for local decision making.

The author's idea is briefly that metropolitan areal settlement patterns result from the history of proximity to employment and market centers as affected by transportation technology. Different locations in the metropolitan area thus go through a sequence of development, but the regulating agents currently affecting settlement are subject to continuous change. This idea takes the form of a typology of "urban realms," based on the work of Vance, a geographer, and is described by the author as "intuitively understandable" (p. 41). The seven categories are based on at least three separate criteria: type of dominant land use, location within the metropolitan area in relation to commuting pattern, and time of settlement. Later in the book, the realm categories are collapsed into three—core, suburban and fringe cities. The classification is used as an introductory typology and later as an analytical variable. *Policy leadership* is exercised when the external developmental forces create the opportunity for local council choices which are determinant. Eyestone defines two kinds of local expenditures as particularly revealing of policy choice—planning and amenities (optional services including parks, libraries, recreation and health). Neither is a hard local service without which the city cannot function, yet each represents a decision by a council. While perceptions of policy problems vary from realm to realm, neither planning, mentioned more often in the developing cities, nor amenities is cited often in an absolute sense by any of the councils. With both aspects, some operational problems are present. For planning, expenditures are often related to a desire to protect the integrity of community values through land use control, rather than a conscious policy based on the advantages of planning *per se*. With amenities a similar problem is that those cities with residents able to provide their own amenities may not see it as a public policy matter. As the author later points out, the planning and amenities aspects are just one set of policies in which leadership can be exercised and studied, and it sometimes appears that other policy areas might have been more productive for clearcut policy-making analysis, although the purpose and specificity of the author's model deserve the consideration he gives it.

To study city policy choices, Eyestone develops the notion of policy profiles. Cities high on both planning and amenities expenditures are labeled advanced; those low on both, retarded; those low on either one and changing on the other, emergent; and high on one aspect with the other changing, maturing. In addition, there

are transitional sub-phases for those high on one aspect and low on the other. The 89 cities break about equally into the categories through the use of medians as cutting points. Using scale scores, Eyestone finds that councilmen in the maturing and advanced cities have slightly higher positive attitudes toward change, and fuller concepts of city future role. Councilmen in maturing and advanced cities tend to be more "political" in style, those in retarded or emergent cities more "pragmatic" and "fatalistic." Voting splits occur most often on advanced councils, least often on retarded councils.

The author turns next to establishing the correlates of the policy profile categorization. Knowing councilmen's perceptions of problems is not directly helpful in predicting policy choices. The data demonstrate that density increases regularly with the direction of policy as defined, generally population increase exerts the same effect, as does tax rate but not per capita expenditures. Using policy development as the dependent variable (by assigning scores as if it were a ratio scale), the author finds in a series of regression analyses that future role scores (though the scale seems to incorporate different dimensions) and the population variables of density/growth rate are relatively strongly predictive. Multicollinearity presents a problem in interpreting these findings, and it is also evident that most of the variables contribute little to an explanation.

The urban realm typology, as can be seen by rearranging Table 5-2, shows a regular and continuous though not complete relationship with policy. Yet when density is removed from the regression equation, the realm residual effect is insignificant. Since the realm rubric was constructed in such a way that density is an implicit variable (injecting elements of circularity for parts of the analysis) this finding ought not to be dismissed easily, because density itself appears throughout the analysis as a main factor, logical albeit stark. The author notes that a close examination of the cities which are exceptions to the tendencies found with the aggregate indicators would highlight policy leadership, but he does not fully exploit that class of cities analytically.

Again drawing on the realm types, the author then constructs models using the promising variables for planning and amenities separately. The models based on simple and partial correlations do suggest some of the relationships of aggregate and attitude variables in producing the expenditures, while the further steps of multiple correlation and regression coefficients

yield these results: In each group only one or two variables account for most of the variance or are significant; with the exception of density for amenities and future role for planning, the effect of the variables in the model is different in the three city categories; and in each multiple correlation the variables included explain less for the suburban cities. Hypothesized differences based on realm in relation to policy spending do emerge, while the aggregate variables continue to be dominant. The more explicitly political variables originally seen as linking the environment to policy leadership have a more diffuse and unpredictable relationship to policy. Thus the outlines of metropolitan development and policy are clearer than the choice mechanisms.

The book raises questions and presents or suggests ways of finding answers. Are local governments dealing on such a rudimentary level, or so constrained by their environments, that we cannot develop a political theory of local policy? Do the lines of local representation have too much slack to trace them productively? How is political style related to substance? What are the unknown variables needed in the model equations? To provide fuller answers, we need further monographs of the project and additional research efforts which can build on the base Eyestone has established. At the exploratory and multivariate stage at which we presently find ourselves in this kind of research, the mixed findings on the political variables and the willingness to grapple with typological problems add to the basic value of this book in developing an approach. The author's analysis improves our grasp on collective decision making and urbanization theory.

EARL M. BAKER

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President and Congress: Power and Policy. By Louis Fisher. (New York: Free Press, Macmillan, 1972. Pp. vii, 347. \$7.95.)

Repeated conflicts between Congress and the president over Viet Nam policy, the 1972 effort to grant President Nixon an item veto over federal spending, and a host of lesser tensions between the executive and legislative branches of government focus attention once again on the proper role of Congress and the presidency in national policy making. Few questions about the federal government have received as much scholarly attention as this one, but despite the abundance of such studies, Louis Fisher's book is a notable addition to the literature. Fisher deals with important—one is tempted to say

"relevant"—questions and he treats them well.

President and Congress is an historical review of the theory and practice of the separation of powers in the United States. Fisher argues that the political experience of the Founding Fathers inclined them in favor not of a rigid system of separated powers but rather a flexible system of checks and balances that allowed ample room for the president and Congress to decide, on a pragmatic basis, who was to do what in different areas of policy making. Many writers have, of course, noted and deplored the liabilities of a system in which policy-making roles are achieved as a result of struggle, not neatly prescribed by charters or organized along tight party lines. Fisher's contribution is to examine the variety of ways in which Congress and the presidency have adjusted themselves to a system of separated institutions sharing powers. Three issue areas are covered in the book: spending, taxing, and war.

What is most impressive about the history of these issue areas is the degree to which Congress has ceded broad discretion to the executive. Lump-sum appropriations that give only vague directions to the executive as to how money is to be spent, contingency funds (that, for example, allowed President Kennedy to establish the Peace Corps), the authority of the president to transfer money from one category of appropriations to another (the method by which President Nixon gave aid to Cambodia following the 1970 invasion), the reprogramming authority of the executive which permits the shifting of funds within appropriated items, "no-year" money which is spent when the executive decides to spend it, impoundment which allows the president to nullify congressionally-approved programs, and unauthorized commitments (Teddy Roosevelt sending the fleet around the world and forcing Congress to pay for the trip)—all of these add up to an impressive array of executive branch spending powers. Congress (more precisely, the Senate) has so far refused the president blanket item veto powers over appropriations, but if such a move should occur it would be in line with ongoing practices rather than being a sharp new departure. When it comes to taxation, Congress has (with the exception of the tariff) jealously guarded its powers; the historic legislative power of the purse, on the other hand, appears to be increasingly more shadow than substance.

Regarding the war powers, Fisher makes a persuasive case that Congress has ceded too much power to the executive, and he illustrates his case nicely with an examination of the Safeguard ABM issue. The alleged characteristics

of the executive branch that uniquely equip it to handle foreign affairs—unity, concern for the national interest, expertise—do not, in Fisher's view, hold up well under intensive scrutiny; thus there is no justification for the congressional inferiority complex in matters of war and peace. Foreign policy making in the executive branch is characterized by fragmentation, log-rolling, and disunity; defense procurement is marked by collusion between private contractors and the Defense Department; and the celebrated expertise of the executive has not prevented huge cost overruns or the firing of experts who fail to abide by the seemingly universal injunction attributed to Sam Rayburn: The best way to get along is to go along. The list of horrors, though familiar, is nonetheless strong support for the argument that Congress should reassert its prerogatives in foreign policy making and restructure its procedures to ensure that it is a significant participant in the day-by-day processes that determine American foreign policy.

Once the Johnson policy in Viet Nam became politically untenable, dozens of congressmen suddenly became concerned over executive warmaking powers. Fisher's arguments are likely to fall on receptive ears. But the crucial test for Congress is a prospective one, not Viet Nam. What Congress is willing to do, and what those in Congress specializing in foreign affairs are willing to do, depends heavily on the direction of the political winds at any given moment. The current situation favors congressional resurgence in foreign affairs, and there are many signs that the imbalance in favor of the executive branch is changing. But it takes more than a single disaster or an act of will for Congress to reclaim its power in foreign policy making. A more fundamental change is a new consensus inside and outside Congress—a consensus in which the president shares—that American involvement in foreign wars should be decided not on a piecemeal basis by the executive but only after long, searching public debate. The Viet Nam experience exposed the dangers of the accretion of presidential war powers. The legacy of Viet Nam, therefore, may well be a fundamentally different context for foreign policy making than has prevailed in this century.

Political scientists who are looking for systematic data collection and hypothesis-testing will be disappointed with this book. Many of the author's findings are, furthermore, commonly found in the literature on Congress and presidency. Still, the author presents a good case that in addressing old questions about gov-

ernmental institutions we should not be hamstrung by dubious arguments over the intent of the Founding Fathers. They, like their successors in office, used theory to embellish an argument, not to prove it. *President and Congress* is good traditional political history and, as such, is a useful primer for those interested in debating time-worn but still timely issues of American national government.

JOHN F. MANLEY

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The State of the Cities: Report of the Commission on the Cities in the "70's." By Fred R. Harris and John V. Lindsay, co-chairman. (Praeger Publishers, 1972. Pp. 114. \$4.95.)

Those of us who live in or study the city have been aware of an "urban crisis" long before the American public was temporarily shocked out of its complacency by the riots that occurred in 1967 and 1968. Since that time, some effort has been made to ascertain why the disturbances occurred and how they could be prevented in the future.

The most prominent and most widely distributed study of the 1967 disturbances was the Report of the National Advisory Commission on Civil Disorders (The Kerner Commission), which suggested that as a result of a continuing pattern of racial discrimination and conflict the nation was becoming polarized into hostile camps of blacks and whites—a situation that would steadily worsen unless improvements occurred in education, housing, jobs, and police-societal attitudes towards minority groups.

The State of the Cities, a report of the Commission on the Cities in the '70s, was sponsored by the National Urban Coalition and co-chaired by Senator Fred Harris and Mayor John Lindsay. It sets out to answer three main questions about urban life three years after the Kerner Report was released: "Has America changed? Are the cities better or worse? Are there new problems, challenges, that have arisen in three years?" It looks for its answers by visiting on the one hand several cities previously affected by disturbances, and on the other, cities like Phoenix and El Paso which escaped previous difficulties but had large minority populations. In each city, the Commission members toured the ghetto areas and spoke to governmental officials, leaders of business and civic groups, and the "man in the street." From these contacts, plus staff background papers, the final report was assembled.

The Commission concludes that despite widespread agreement that the poor conditions of life in the urban ghettos were largely respon-

sible for the disorders of the 1960s, "most of the changes in those conditions since 1968 . . . have been for the worse" (p. 4). We are told that housing is still poor, schools are even more turbulent, crime and drug addiction have increased, welfare rolls are higher, and police-minority-group relations are just as hostile. Reference is also made to the tendency of an increasing proportion of blacks and Mexican-Americans to inhabit the central cities, while the suburbs continue to be overwhelmingly white.

This report dramatically highlights the continuing nature of urban problems in our society, and also demonstrates the failure of government policy makers to cope effectively with these problems. In several respects, however, the study is disappointing. Its main shortcoming is a lack of originality. Over the past several years many studies such as this one have been released by governmental and citizens groups. Each documents the poverty, discrimination, and feelings of despair experienced by ghetto residents, and each suggests essentially the same solutions. The Commission itself acknowledges this point, and instead of making new recommendations, it suggests instead that those proposed in the past by groups like the Kerner Commission be fully implemented.

Another deficiency relates to the contents of the document. The actual report is fairly short (32 pages), and much of it is highly impressionistic. The details of conversations held with residents of the cities visited, plus a narrative account of problems faced by them in their daily lives can provide the reader with valuable insights. It is difficult, however, to generalize from these findings. One might also ask why so few cities—six in all—were examined. The only other source of documentation comes from the background papers which are mainly condensations of previously published governmental or scholarly reports. In their present form, they make little contribution to increased understanding of the urban scene. It would seem that what is needed at this point is not another report by a "blue-ribbon commission," but rather, research into why previous remedies have not been enacted or proved successful, and what concrete plans for action can be formulated.

RICHARD D. FELD

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The Winning of the Midwest: Social and Political Conflict, 1888–1896. By Richard J. Jensen. (Chicago: University of Chicago Press, 1971. Pp. xvii, 358. \$12.50.)

Professor Jensen of the University of Illinois at Chicago has made a significant contribution

to the ever-growing field of 19th-century political history. His analysis goes beyond emphasis on a single election or issue to stress the importance of underlying conditions in American society that were operating to upset the political equilibrium in six midwestern states.

The Midwest became a Republican stronghold at the time of the Civil War electoral realignment. There were few signs in the early 1880s that within a decade this dominance would be seriously threatened. The succession of Democratic victories in state and congressional elections from 1889–1891 gave some indication that the change in voting behavior involved more than short-term defection. The stability of party identification which had been so characteristic of the post-Civil War years appeared to have been undermined. If the voters who had crossed party lines had undergone full-fledged conversion of loyalties the election of 1896 might indeed have been "critical" and a lasting partisan realignment might have resulted. When the reorientation occurred, however, the Republicans were again dominant, and a party system had been established that would persist in this region for another generation.

It is Jensen's theory that the succession of landslides was the direct result of the intensification of social conflict. He stresses the importance of religion in 19th-century American life, and emphasizes its role in determining party affiliation and in shaping political issues. Pietists struggled with liturgicals in every denomination, and the conflict became intense whether the issue concerned the prohibition of the liquor traffic or restrictions on church schools. When it appeared that the moralists had achieved success in several states through control of the Republican party machinery, the political results were decisive. Only if pragmatic professionals could curb the crusading amateurs, or if new issues appeared, could the GOP hope to recover its former position of supremacy in the Midwest. The recognition of diverse cultural and moral belief systems among the electorate prevented the continuation of old-style monolithic campaigns, whether pietistic or partisan. The Republicans made a remarkably successful adjustment, and McKinley's pluralism attracted increasing support from nearly all ethnic groups in 1896. The Democratic party moved in the opposite direction, and Bryan's moralistic and millennialist crusade alienated many of the voters who had contributed to the success of the party earlier in the decade.

The author does not ignore economic matters, but he argues that some historians have

overemphasized class conflict. He concludes that in these states and in this decade economic issues were less important than cultural conflicts. Campaigns waged on the tariff or monetary issue, for example, did not arouse the same high level of antagonism among immigrant voters as those that involved religious and ethnocultural allegiance. The impact of the severe 1893 Depression was significant, but the elections of 1894 constituted a decline for the Democrats rather than a great advance for the GOP. This view supports the conclusion of Paul Kleppner in his recent study of Wisconsin, Michigan, and Ohio that the shift away from the Democrats cut across all social groups. The citizens who chose to express dissatisfaction by staying away from the polls were important, but those who sought a political alternative in a third party were not. Whatever success agrarian radicalism had in other sections, the Populists were not a major force in this part of the Midwest. The evidence in both studies indicates that even under the impact of widespread economic distress, the tradition of party loyalty helped retard the decline of Democratic strength. After 1896 the voters who had changed political direction found themselves part of a political organization that had become more tolerant of social diversity and that held power during a period of improving economic conditions. Thus what may have been temporary deviation became a more durable relationship within a new majority coalition.

This analysis of party affiliation in a period of political change suggests possibilities for further research. We need studies of state politics at other times and in other regions to determine whether the Midwest was unique and whether a similar polarization of religious and cultural attitudes played a significant role in altering the voting behavior of the American electorate during the 1850s and the 1920s.

The critical essay on the sources is both comprehensive and discriminating. The documentation reveals skill in locating new and significant information. The material presented in the tables and in the appendix indicates to some degree the extent of the statistical investigations. Professor Jensen has been a leader in the group of historians who have been promoting the use of quantitative analysis in historical research. His book is an excellent example of what can be accomplished when sophisticated statistical techniques are used to discover patterns of stability and change, and to clarify historical relationships over a period of time.

JAMES A. MELDRUM

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Environmental Law and Policy: Readings, Materials and Notes on Air Pollution and Related Problems. By James E. Krier. (Indianapolis: The Bobbs-Merrill Company, Inc., 1971. Pp. 480. \$13.50.)

Professor Krier's collection of readings is an excellent compilation of materials on the pollution problem and, taken as a whole, is also a provocative examination of alternative approaches to pollution control. Although its focus is on environmental law, it deals in broad-ranging fashion with many problems of public policy.

Krier states (p. 293): "The materials that follow focus primarily on the air pollution problem, but the issues they raise are of much broader significance. Consider the materials not in isolation but in the context of all the machinery of the legal system and the limitations of the real world." The materials selected, the manner in which they are organized, and the author's probing notes and questions facilitate the reader's ability to put the materials in a broad context.

About half the readings are court cases and articles from law journals. Most of the landmark cases, from William Aldred's Case (decided in 1611) to Calvert Cliffs' Coordinating Committee v. Atomic Energy Commission, are included. The other readings cover a wide gamut of authors and subjects. Economists are well represented, particularly J. H. Dales, but also Kneese, Woloizin, Gerhardt, and Mills. There are several excerpts from Naderite John Esposito, the transcript of a television debate on the internal combustion engine, and a detailed and very good analysis of the inner workings of the San Francisco Bay Area Air Pollution Control District. Most of the selections were written in the late 1960s, but more recent events, such as implementation of the 1970 Clean Air Act and the National Environmental Policy Act, are also covered.

The book is divided into three sections: the problem of pollution; the pollution problem and the courts; and pollution control legislation and its administration. The first section provides Krier's basic framework which is drawn not from the law but from welfare economics. It is best summarized in a selection from Calabresi (p. 139):

Problems of misallocation of resources and externalities are not theoretical but empirical ones. The resource allocation aim is to approximate, both closely and cheaply, the result the market would bring about if bargaining actually were costless. The question then becomes: Is this accomplished most accurately and most cheaply by structural

rules (like anti-trust laws), by liability rules, by taxation and governmental spending, by letting the market have free play or by some combination of these?

The extent to which the courts can efficiently allocate resources is discussed in the second section. As might be expected, the courts have not been consistent in their application of doctrines such as nuisance and trespass. Some of the judges have weighed the social costs and benefits in arriving at their decisions, while others have read the law to preclude a balancing of the social consequences. The limitations of the courts as a vehicle for setting pollution policy are described, although the key problem of the courts' ability to deal with scientific questions is not given much attention.

The selections and commentary dealing with pollution control legislation reveal Krier's doubts about the effectiveness and efficiency of the Federal air pollution legislation. Two rather bland descriptions of the 1967 Clean Air Act are followed by savage blasts from Esposito. The concluding portion of the section on legislation is devoted to a discussion of pricing systems, and the reader is led to side with the theme of a reading from Larry Ruff (p. 424): "About all that can be said in favor of this price method is that it is logical, fair, comprehensive, practical, and much better than any of its alternatives." Krier's economic framework leads him to the same conclusion most economists have reached—namely, that a pricing or effluent fee scheme would be superior to the current regulatory system.

A book of readings, if it is to be an effective educational tool, should have a framework and a point of view. As the above indicates, *Environmental Law and Policy* meets this standard admirably. At the same time, its selections are balanced and well-chosen, and it successfully forces the reader to consider for himself the difficult questions posed.

The Krier volume would be an excellent text for courses in environmental law or the role of law in meeting societal problems. Its legal focus makes it less useful for courses in public policy, but the variety and high quality of its selections would make it a very good supplementary text for such courses.

J. CLARENCE DAVIES, III

The Council on Environmental Quality

Ends and Means of Reducing Income Poverty.

By Robert J. Lampman. (Chicago: Markham, 1971. Pp. 168. \$3.95, paper.)

Robert Lampman has written the primer on American poverty. He has distilled more good

sense, pertinent information, and focused concern about American poverty into 168 pages than is contained in any collection of books and articles ten times this length. A catechism on this book should precede immersion into the eschatology and econometrics that characterizes most of the poverty literature.

The style of this book is refreshing. Lampman has a single-minded concern about American poverty. This book is drawn primarily from his own writings on this subject for more than 20 years and from those of his colleagues at the Institute for Research on Poverty at Wisconsin. At the time poverty became a major political issue, Lampman was one of perhaps a dozen Americans who had done serious scholarly work on which to base policy strategy, and this book was written to maintain public concern about poverty against the competition of other issues and against a legitimate skepticism about the efficacy of many public programs. He is a nominalist and wields Occam's razor with zest. Income poverty is defined as a state of low income. Poverty is a different condition from alienation, disability, discrimination, despair, inequality, powerlessness, and unemployment; although these conditions are often related, eliminating any one of these conditions will not eliminate the other conditions, and each deserves to be studied on its own merit. Once one accepts this mental set—a focus on income poverty—most of the data fall into place, and the major policy alternatives become apparent. Finally, Lampman is a rather old-fashioned economist, in the tradition of the Wisconsin institutionalists. One can only infer his theoretical structure from the strength of his argument. There is not an equation, a statistical test, or an indifference curve in his book, but he draws intelligently from the theoretical and empirical work of others. The major technical strength of the book is a very careful reading of the census data. For these reasons, and not in spite of them, Lampman has more to say about American poverty—to both scholars and men of affairs—than do most of us, singly or in combination.

The first three chapters efficiently summarize the intellectual and political background shaping American poverty policy. These chapters do not bring the reader quite up to date, because they exclude both the recent developments in welfare economics and public choice theory and the recent debate over welfare reform, but they provide a sufficient stage for the analysis of contemporary conditions.

The middle four chapters are the main strength of this book. These chapters summa-

rize and analyze the current characteristics of the poverty population, changes in the level and composition of the poverty population since 1947, and the projections of this population. Lampman uses the Social Security Administration definitions of poverty incomes without reservation, recognizing that this yardstick, like any yardstick, is arbitrary but nonetheless useful. His major findings deserve repeating:

- The poor are much like the rest of us, except their incomes are lower. For example, the representative poor family is white, lives in an urban area outside the central city, has an able working family head, and is not on public assistance.
- Poverty is disproportionately high among the aged, the disabled, families headed by females, minority races, those with little education, and those who live in rural areas, although only little education among these conditions characterizes the majority of the poor. In contrast, most of the people in each of the above groups are not poor.
- For many of the poor, poverty is a transitional status. Around one-fourth of the poor in one year move to a higher income group in the next year.
- The proportion of the American population with poverty incomes fell from 30 per cent in 1947 to less than 12 per cent by 1970, despite a relative increase of four population groups with a higher incidence of poverty.
- Much of the reduction of the poverty population has been due to economic growth. Other major contributors were the increase of public transfers and the movement of people to higher income occupations, industries, and regions.
- The American tax system has been only mildly equalizing for several decades. Although total transfers have increased rapidly, less than 40 per cent of these transfers accrue to the pre-transfer poor. Even this smaller amount has had a major effect in reducing poverty, reducing the per cent of the total population in poverty by 10 per cent and the poverty gap of the post-transfer poor by over \$10 billion.
- Recent projections of the poverty population suggest that the poor will number around 8 per cent and the poverty gap will be reduced to about \$5 billion by 1975. The composition of the poverty population is not expected to change much, despite the relative immunity

of major groups in this population to economic growth.

These conclusions suggest that elimination of American poverty, in terms of establishing a floor under family income, is achievable in this decade, without a major increase or reallocation of public funds and without a major restructuring of our society.

The final two chapters summarize the broad strategies for reducing poverty and considerations for selecting among these strategies. Although these chapters are not definitive, whole sections deserve quotation as guidelines for maintaining perspective and focused concern about American poverty. Lampman finally opts for rather marginal changes in government programs—\$2 billion to raise public assistance minimums in the low income states, \$4 billion to supplement the incomes of the working poor, and \$4 billion to increase job-related education and training and to create jobs for the poor. My own quibbles with his suggested strategy are minor: My sense is that he underestimates the value of a wage-subsidy system for the poor and overestimates the value of education and training, but these are technical details which could be worked out. One hopes that the views expressed in his final sentences are widely shared:

Ending income poverty does not require and will not achieve a transformation of society. It is a modest goal. . . . Dedication to the goal of eliminating income poverty requires that we scrutinize every expenditure made in the name of the poor to make sure that a great share of the benefits go to them. We should proceed, not with utopian expectations, but with the belief that achievement of this particular goal, along with progress toward other social and economic goals, will be a worthy achievement in the slow evolution of the good society.

WILLIAM NISKANEN

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America on Trial—The War for Vietnam. By General Thomas A. Lane, foreword by General A. C. Wedemeyer. (New Rochelle, N.Y.: Arlington House, 1971. Pp. 297. \$7.95.)

Evidence can be assembled to argue that the late President Ngo-dinh-Diem was overthrown by the South Vietnamese generals in 1963 at the behest of American public opinion rather than of Vietnamese public opinion; that if he had not been murdered on the spot he would have returned to power within two or three years; that more blood has been shed in Viet-

nam because he was overthrown than would have been shed if he had not been; that the Indochina Communist Party is heir to the old Bolsheviks of the Comintern; and that the communist bloc has remained "monolithic" in this one arena of the world revolution if in no other. But General Lane shows no inkling of what the evidence is: he argues from prejudice alone.

Working from a handful of superficial secondary sources, and with no direct knowledge of his own to supplement them, he bludgeons the reader with an unremitting tirade of chauvinistic invective. U.S. administrations, he declares, have persistently shrunk from hitting the Chinese and Vietnamese communists hard enough. The culminating sin has been the Nixon doctrine, arrived at on misleading advice, the identity of whose author—Sir Robert Thompson—gives the sinister show away. That British counter-guerrilla expert, General Lane declares, lent his international reputation in 1969, for selfish British reasons of state (p. 211), to the false report that things were going well in Vietnam and thereby abetted President Nixon's soft-on-Communism foreign policy. In fact, General Lane's finger points at the British as multiple culprits—over Nationalist China and Korea as much as over Vietnam—because they have persistently "vetoed" (pp. 46 and 287) America's pursuit of a foreign policy in American interests, with the unworthy purpose of safeguarding their wretched colonialist possessions in Hong Kong from Red Chinese retaliation (p. 76). What is needed now ("by 1973"—pp. 229–31) is the mounting of an "invulnerable nuclear capability" in defiance of Russia and China, to coincide with the return home of American forces from both Asia and Europe. Rebellions will then break out, apocalyptically, in all Communist countries. Thus the Free World will be saved.

As political or strategic analysis, this book is laughable. The author contrives to introduce some element of inaccuracy into nearly every statement he makes; the silliest is that, going from China to Vietnam, one "crosses the Himalaya mountains" (on p. 29 and again on p. 68). He deserves to be thrown to the Concerned Asian Scholars.

DENNIS J. DUNCANSON

University of Kent, England

Federal Funding and National Priorities: An Analysis of Programs, Expenditures, and Research and Development. By Leonard Lederman and Margaret Windus. (New York: Praeger Publishers, 1971. Pp. 213. \$15.00.)

The authors of this book had a very limited purpose; they wished to demonstrate exactly what the title suggests about trends in federal spending over the course of a decade (1961–1971). They deduce from these statistics changes in national priorities. In addition to gross spending for federal programs, they also separate out expenditures for research and development, and relate these to their parent programs. As they state in their preface, they are concerned with “what is,” not with “what should be.” Their major source of information is *The Budget of the United States*.

This book can have some value as source material to political scientists, although its authors are economists. They have accomplished their limited objective in a workmanlike manner. This reader, however, found it disappointing on a number of counts. The bulk of the study describes all the major federal programs, grouped into what the authors call “super-goals,” and indicates the trends in spending of each. The writing style bears a close resemblance to that of the principal source of their data. Occasionally, the authors suggest some of the common criticisms of the programs, but space limitations prevent serious discussion of these. The almost total absence of any commentary or opinions makes for rather unexciting reading. Their findings will hardly be surprising to anyone who follows the ups and downs of federal programming. Because their data only cover actual expenditures through the Johnson administration, one gets little insight into changes in priorities effected during Mr. Nixon’s first term. The way the data are presented offers little opportunity to judge who are the principal beneficiaries of federal spending. And, they do not distinguish direct federal spending from grants to cities and states, an exercise which would have been useful.

To sum up their conclusions, the authors find declines during the decade in the percentage of total outlays in their “supergoal” categories of “national security,” “space,” and “commerce, transportation, and communications,” while “income security and welfare,” “education, knowledge, and manpower,” and “health” showed significant increases.

Their final chapter, entitled “The Likely Future,” is based on past data trends and “judgments about the possibility of breaks in these trends due to already apparent changes in priorities . . .” (p. 183). The authors predict continued growth in the areas that grew in the 1960s.

FRANK C. COLCORD, JR.

Tufts University

City Managers in Legislative Politics. By Ronald O. Loveridge. (Indianapolis: Bobbs-Merrill, 1971. Pp. 224. \$8.50.)

Students of local government are well aware of the traditional controversies surrounding the role of the city manager: Should managers make policy, or should they merely confine themselves to administration? Are managers motivated solely by a concern for efficiency, or do they have broader social goals?

In his useful and readable study, Ronald O. Loveridge avoids the exhortative, debating approach which has characterized much of the literature in this field. Instead, the author provides a systematic analysis of the ways in which city managers define their policy roles and participate in the political process.

Loveridge’s analysis is based on interviews with city managers, mailed questionnaires to city managers, and interviews and questionnaires with city councilmen—all in the San Francisco Bay Area. The study is part of the City Council Research Project of the Institute of Political Studies at Stanford University.

In examining city managers’ views of their proper policy roles, Loveridge found “overwhelming agreement that the city manager should be a policy innovator and policy advocate” (pp. 49–50). Eighty-eight per cent of the managers studied agreed with the statement that “A city manager should assume leadership in shaping municipal policies” (p. 49). When the author turned to city councilmen, however, he found a very different conception of the proper role of the city manager. Eighty-seven per cent of the councilmen in the sample agreed that “The city manager should act as an administrator and leave policy matters to the council” (p. 86). Councilmen consistently voiced the opinion that the city manager should serve as a staff aide and adviser to the council.

Given this policy role conflict, city managers were found to utilize a number of strategies in dealing with councilmen. Loveridge discovered that the manager might consult informally with individual councilmen, authorize staff reports to prove certain points, hire outside consulting firms to make recommendations, prepare the agenda in ways which would influence policy decisions, and so forth. By using these strategies, managers could influence policy without unduly increasing councilmanic anxieties.

Although there was a substantial area of agreement in city managers’ definitions of their role, Loveridge does identify different levels of support among managers for the view that they should make policy. And the author found that these differences in normative policy orienta-

tions were reflected in styles of political behavior. Those who endorsed a strong leadership role for managers tended to advocate major policies and to work actively with community groups, while most of those who defined the manager's proper role in a more limited way revealed themselves as avoiding contentious policy questions.

One of the most interesting parts of Loveridge's study is the identification of sources of differences in orientation among managers themselves. Loveridge presents a number of background factors which are found more frequently among aggressive political managers than among their more constrained counterparts: college education; social science major; graduate work; outside appointment (not on city staff); apprenticeship in city management; short period of service (15 years or less) in municipal government. The author also argues persuasively that professional identification and participation in the International City Managers' Association can be forces in the development of an ambitious definition of the manager's role.

Another strong section of the book is that in which the author documents the widely differing opinions of city managers and councilmen about the proper role of managers in the policy process. But the identification of this conflict in role perception raises some questions which are not answered in the study. Although Loveridge does describe strategies by which a manager can further his goals in the city council, he does not deal with the question of open hostility between manager and council. Given the differences in role perception, one would expect serious confrontations to take place. And if such is not the case, what is the explanation? Loveridge's findings show that city managers do participate in policy initiation and leadership. Are they so adept at masking such activity that councilmen are not aware of what is going on? Or are councilmen able to invent face-saving rationalizations for managers' active involvement in the making of policy? It has often been said that councilmen hold a trump card in their power to fire the city manager, but it would be interesting to know how often—and under what conditions—this power has actually been used.

The weakest segment of the book is the last chapter, "City Manager in Community Politics," in which a number of broad assertions are made without any supporting evidence. Dealing with the question of city managers and democratic values, Loveridge makes the following statements: "Probably no member of the local elite is more open to diverse

public influences than the city manager" (p. 160). "The manager often represents those whose interests are not represented in the elite. Negroes provide a good example" (p. 161). "Unlike . . . most elite members, the city manager champions the general welfare of the community as opposed, for example, to the special interests of business, labor or civic groups" (p. 161). Such claims demand considerably more explication and support than this volume provides.

In fairness, it must be noted that the last chapter is somewhat speculative in tone and, at any rate, not crucial to the study. The rest of the book is a carefully argued, well-written contribution to our understanding of the perceptions and behavior of city managers.

JEFFREY L. PRESSMAN

Massachusetts Institute of Technology

With All Deliberate Speed. Edited by John H. McCord. (Urbana: The University of Illinois Press, 1969. Pp. vii, 205. \$7.50.)

This frequently interesting set of essays says a great deal more about the practice than the theory of civil rights. As Professor McCord writes in his foreword, the authors here are not detached academicians, but "crusaders and public teachers for civil rights" (p. vi). Following a useful general summary of the development of civil rights from the Civil War to the present (written by the student editors of the *University of Illinois Law Forum*, where the symposium first appeared), there are articles on education, public accommodations, and housing, which discuss prospects for future progress toward equality in the context of analysis of the current legal situation. The article on employment opportunities is a detailed case study of a successful action by the federal government intended to serve as a model for the future.

As a book which captures the state of civil rights at a particular point in time and makes proposals for action in the immediate future, *With All Deliberate Speed*, is in some respects already outdated. A good deal has happened in the four years since it was written, and the advice it offers on tactics is therefore no longer entirely relevant. In the intervening period, for example, the decisions in the *Mecklenberg* and *Holmes* county cases have dramatically accelerated the pace of school desegregation in the South, and the Philadelphia plan and other federal affirmative action programs have changed the picture in employment.

But there is another and more subtle sense in which the passage of time tends to undermine the book. All of the authors base their analyses

and proposals on the assumption that full equality can and should be achieved by governmental action. Increasingly, however, doubts grow as to whether this is indeed possible or desirable. This is by no means to suggest that the authors here are wrong in their assumptions, but that they fail to take account of the objections which are more and more being raised against them.

This is clearly true of the piece on equal educational opportunity by Robert L. Carter, former NAACP General Counsel (1956-68). Its lucid and informative analysis of the developments since *Brown v. Board of Education* rests on the assumption that "... segregation, in and of itself, denies equal educational opportunity" (p. 62). But this assumption must now be confronted with Christopher Jencks's carefully documented judgment that "... the case for or against desegregation should not be argued in terms of academic achievement" (Jencks et al., *Inequality* [New York: Basic Books, 1972], p. 106). Carter also falls short in devoting only a brief and rather cryptic paragraph to what he admits to be the "growing clamor for something called community control or decentralization" (p. 83). It seems especially true here that the determination to have governmentally imposed integration, and therefore presumably equality of educational experience, blinds him to other very real values to be taken into account on the other side.

The same sort of point can be made even more forcefully about the liveliest and most intellectually stimulating piece in the volume, Meiklejohn Library President Ann Fagan Ginger's article on housing. Ms. Ginger looks upon housing as a public utility, a necessity to be provided to each of us by the most efficient possible means. Seen from this perspective, the private housing market is naturally found to be unfair and inefficient. In contrast to the current state of affairs we are given a nostalgic picture of those systems, from feudalism to the company town, which once provided housing for the worker. Then we are offered a model for the future in which a single government agency would control the provision of housing for each of us, bringing back the benefits of feudal stability and eliminating discrimination "... on the basis of race or financial condition" (p. 177). Of course this would mean that housing is no longer private property. But most of us don't really own our own houses anyway (the bank does), and we already get most of the vital services which make our houses habitable from the government. Why not recognize the fact that private property has ceased to exist and adopt the proposed model?

The answer lies in those considerations which Ms. Ginger chooses to disregard. Admitting that we are already far too dependent on banks and governments to claim that our homes are really our castles, it is still essential that we maintain and extend our right to choose, shape, and live in our homes as freely as if they were our castles. What would happen to what remains of our freedom to order our private lives as we choose, if we were faced with a centralized bureaucracy every time we wanted to make a major change in our housing, or the way in which we live in it?

But setting aside the problem of the desirability of such a system, growing experience with welfare bureaucracies should lead us to question the assumption that it would work. At least it is clear that the effectiveness of such a bureaucracy can no longer be taken for granted.

This is a book which may be most important, then, not as a blueprint for the future, but as a monument to a period of singleminded confidence in the government's capacity to solve the problems of equality, if only everyone would try hard enough: a period which is now rapidly coming to an end.

ROBERT S. GERSTEIN

University of California, Los Angeles

This Sacred Trust: American Nationality, 1798-1898. By Paul C. Nagel. (New York: Oxford University Press, 1971. Pp. 376. \$9.50.)

Historians have long been intrigued with the problem of defining the distinctive attributes of American nationality. Differing in their conceptualizations of the problem and in their methods of attacking it, they have produced a rich and varied literature on this baffling theme, to which Dr. Paul C. Nagel's impressive study must be added.

Nagel, who previously explored attitudes toward Union in his *One Nation Indivisible*, now examines "what being an American implied to the citizens of the Republic." He is concerned with the sense of nationality, rather than of nationalism, that found expression in representative statements by political leaders, publicists, and intellectuals. Thus he focuses on self-conscious interpretations of the nation's perception of its "calling" or "trust." Instead of the consistent, almost static ideological commitments identified by such scholars as Louis Hartz, Daniel Boorstin, and David Potter, Nagel finds ambiguity, agonizing doubts, and intolerable anxiety.

Beginning in 1798, as the young republic became aware of the full range of foreign and domestic threats that imperiled its existence, Na-

gel details the prevalent despair about the survival of the national experiment. After 1815, he tells us, the concept of a trust emerged, and although its nature was variously defined, there was constant fear that innovation, disunity, and man's evil nature would result in betrayal of the trust. The ante-bellum decade brought conflict between those who called for a stewardship of preservation and those "restorationists" who urged a dynamic quest for liberty, even at the price of Union. Then came the "atonement" of the Civil War, and with it the anticipation of a new birth of freedom.

But, in Nagel's view, the hoped-for regeneration proved illusory. Although both the Union and republicanism has been maintained, the sordid influence of Mammon threatened to pervert the high calling which had to be realized if nationality was to be achieved. Escape from the agony of an intolerable trust was accomplished in 1898, when the nation turned abruptly away from preoccupation with its shortcomings to embrace a new trust, that of pursuing a disinterested stewardship of moral betterment throughout the world.

So intricate and shaded is Nagel's treatment of his central theme that any *précis* can merely be suggestive. His study is important for the confirmation that it gives to the accepted notion that America has been singularly concerned with its calling, for the new emphasis it places on the extreme anxieties that such concerns produced, and for his suggestion, which I do not find quite plausible, that an escape from the old trust was achieved after 1898.

Among the weaknesses of the study I would cite the idiosyncratic conceptualization of "trust," the extreme reticence of the author to interpret his findings in general terms, and a scheme of organization that is often baffling. Others may share my bewilderment at Nagel's enigmatic conclusion that the republic can only fulfill its trust through humility. Students of American nationality will find the work more useful as a well-selected record of the varied perceptions of national goals than as a contribution to their theoretical understanding of the problem.

RICHARD P. MCCORMICK

Rutgers University

Mr. Republican: A Biography of Robert A. Taft. By James T. Patterson. (Boston: Houghton-Mifflin, 1972. Pp. xvi, 749. \$12.50.)

On a sloping lawn near the U.S. Capitol there stands an imposing hundred-foot tower, a memorial to Senator Robert A. Taft, Republican of Ohio, placed there by unanimous act of

Congress two years after Taft's death in 1953. No other Member of Congress, past or present, is so honored. In 1957, a special bipartisan committee of the Senate selected Taft as one of the five most significant figures ever to sit in the Senate. Both these signal honors attest to the special esteem in which Taft was held by his colleagues. Because he was the son of a President of the United States, several times himself a presidential hopeful, and leader of his party in Congress, it was inevitable that Taft would attract the attention of a biographer.

It was by no means inevitable, however, that Taft would receive a fair or sympathetic hearing at a biographer's hands. Most of his colleagues respected Taft for his intelligence, his mastery of facts, and his analytical power. Few of them liked him, however, or even knew him well, and many found him stubborn, narrowly-mindedly ideological, and cold. "Taft has the best mind in the Senate—until he makes it up," was a famous remark. He is remembered today for the Taft-Hartley bill, a law regulating labor-management relations enacted over President Truman's veto and widely regarded by union people as antilabor. Taft denounced the Nuremberg trials as ex-post-facto, and encouraged Joseph McCarthy in his assaults on the reputations of persons linked with the State Department. He was the foremost conservative politician of his time, yet he regularly sponsored bills for federally subsidized housing and aid to education. He opposed the excessive interventionism of the Marshall Plan but supported General MacArthur in Korea. As a member of the Yale Corporation he deplored the employment of left-wing professors but demanded that they be left strictly alone in the pursuit of their work. He was strongly partisan, a party regular, and highly conventional in his address to most questions, yet fiercely self-reliant and independent in his drive to master issues and present them in his own way. He wrote all his own speeches. And for all his conventionality he was never a religious man. He was for years an undisputed leader in an institution known far and wide as the world's most exclusive gentleman's club, yet in his personal relations he was relentlessly unclubbable—awkward, shy, and brusque.

James T. Patterson has done even-handed justice to this remarkable mélange of characteristics, neither forcing an interpretation upon recalcitrant contradictions in his subject's character, nor permitting an undue fascination with warts to obscure the main features of Taft's portrait. Patterson, whose previous work was on the conservative coalition in Congress, en-

joyed the full cooperation of the Taft family, and exclusive access to a formidable treasure-trove of family documents. In addition he seems to have combed through or interviewed every other plausible source for letters, diaries, and recollections. Although he has written an authorized biography, it is nevertheless an independent work of scholarship.

Two features in particular distinguish the book: Patterson shows a remarkably thorough grasp of the dynamics of congressional politics and of national politics more generally throughout the entire 60-year period of his narrative. And he is absolutely fair-minded in discussing the activities of Taft and his contemporaries. It is now fashionable in historical writing to commit the sin of anachronism, in which the historian parades his knowledge of how things turned out—plus his presumably superior moral insight—so as retrospectively to weigh and judge the acts of his subjects. By scrupulously refraining from any such nonsense Patterson has immeasurably enhanced the value of his book. He has also produced a well-informed and illuminating portrait of a significant American and the politics of his times.

NELSON W. POLSBY

University of California, Berkeley

The Long Struggle for Black Power. By Edward Peeks. (New York: Scribner's Sons, 1971. Pp. 448. \$7.95.)

The Black Man and the American Dream: Negro Aspirations in America, 1900-1930. Edited by June Sochen. (Chicago: Quadrangle Books, 1971. Pp. 372. \$2.95.)

For Blacks Only: Black Strategies for Change in America. By Sterling Tucker. (Grand Rapids, Michigan: William B. Eerdmans Publishing Co., 1971. Pp. 211. \$4.95.)

Differing in format, these books have similar concerns and common subject matter: the black experience in America. Each is timely and useful and has its particular merits. *The Long Struggle for Black Power* is a brief, well-documented history of major ideas, movements, and personalities in the saga of black Americans. *The Black Man and the American Dream* is a selection of articles on the American race problem, primarily by Negro authors, from periodical literature appearing in the first three decades of the twentieth century. *For Blacks Only* is a terse, personal, thoughtful analysis of the present state of the black revolution and pragmatic proposals for meaningful change.

The essays in Professor Sochen's book were taken from the *Nation*, *New Republic*, *Crisis* (NAACP), *Opportunity* (Urban League), and *Southern Workman* (Hampton Institute). They examine major aspects of Negro life: education, health, employment, migration, progress, discrimination, disfranchisement, etc., in the context of American values and goals.

Sochen's analysis of five dozen articles by Negro authors leads her to conclude that black writers in this era generally shared the values of white America and "used the rhetoric, methodology, and goals of the American Dream in their struggle for equality" (p. 3). Though aware of views of black radicals such as Monroe Trotter, Chandler Owen, and A. Philip Randolph, she finds little intense social ferment. She is amazed that black intellectual thought reflected faith in the democratic process and the possibilities of substantial progress within the American system.

Most reform movements in twentieth-century America, however, have been mounted on a similar faith. Thus, in view of the dynamic potential of many of America's professed ideals, the tortuous history of the black experience, and the realities of the Negro's struggle to survive in a hostile environment, this reviewer finds Professor Sochen's amazement difficult to understand. Most of these essays are well written, interesting, worth reading, especially Alain Locke's brilliant interpretation of the "New Negro," and Abram Harris's erudite analysis of one sector of the American Left. Harris reflects the intellectual potential that made him an outstanding scholar and teacher in economics (not sociology, as Sochen mistakenly claims).

These writings address many of the race issues still confronting—and dividing—black and white America. In these days of the "cult of blackness," when the legitimate quest for black identity and respect sometimes leads to counterproductive self-delusion, it can be helpful to consider the caustic comments of George Schuyler and the rebuttal of Langston Hughes. Michael Gold's presumptuous piece is another reminder of some white men's readiness to proffer unsolicited advice to the black brother.

Peeks's *Long Struggle* is a solidly written book, reflecting the author's graduate study in journalism. Based primarily on secondary sources, the volume is profusely documented by much of the best literature on Afro-Americans; and Peeks draws effectively upon his personal contact with the civil rights movement as a reporter covering historic events of the 1950s-'60s.

Peeks organizes his narrative around the

theme of self-help, which he perceives as a major strategy in the struggle for black survival and progress in America. Viewing organization of the Free African Society in 1787 by Richard Allen and Absalom Jones as the beginning of organized Negro self-help, Peeks traces this phenomenon in the creation of various types of voluntary associations. Enforced segregation and pervasive oppression forced black Americans to establish organizations to abolish slavery, aid the sick, bury the dead, educate youth, acquire land, establish businesses, fight discrimination, and so on. The establishment of churches, fraternal orders, banks, etc., reflected the Negro's recognition of the importance of group efforts. Peeks relates self-help to the antilynching movement, the fight against disfranchisement, segregation in public accommodations, discrimination in employment, the courts, and housing.

Peeks draws vivid portraits of black leaders who moved boldly upon the American scene and had massive impact on public opinion and race relations. He discusses black nationalism, boycotts, integration versus separatism, nonviolence versus violence, electoral politics, protest politics, and many other ideas, actions, and movements evolving out of the black man's struggle to find strategies for liberation. He presents in sharp, balanced perspective the cross-currents in Negro life, and the conflicts which have divided and weakened black leadership in the fight against exploitation and institutionalized racism.

Though it covers ground familiar to the social historian, particularly black scholars, nevertheless, most readers—especially students and the general public—can profit from reading this book. It should be required reading for those cocksure militants who lack understanding of the essential continuity of black protest and who tend to think that the fight against oppression began in the 1960s.

Tucker's *For Blacks Only* is not an exercise in scholarly documentation. It is a brief, stimulating statement, distilled from the active mind and rich experience of a man long and actively involved in the black struggle. The author has held responsible positions in the civil rights movement as national coordinator for the Solidarity Day March in support of the Poor People's Campaign and Vice-chairman of the 1963 March on Washington.

Whereas Peeks believes the civil rights movement has entered a new phase, Tucker feels it is dead. But despite America's retreat to conservatism, despite the deliberate abrasiveness of some embittered blacks, despite the polariza-

tion of the races, and despite white repression and black rage, Tucker believes that there are strategies which can be used to effect significant change within the system. He feels that the anger and energy of blacks can be channeled into constructive work. On the basis of his experience as activist and Urban League executive, Tucker shows how coalitions with other groups can successfully attack crucial problems in education, housing, employment, crime, political power, and representation.

Like Peeks, he defines and dissects black power and identifies its positive and useful aspects. Both Tucker and Peeks celebrate black pride and racial solidarity but reject black separatism. Tucker recognizes the uses and the strong appeal of separatism, especially to the powerless. He notes how overemphasis on cultural uniqueness and "the current fashionable focus on black identity serve white racism" (p. 99). Both Tucker and Peeks agree that Negroes cannot effect their liberation by their efforts alone; they must have help of white allies. Tucker understands how disillusionment with the slowing of civil rights progress led to expulsion of whites from some interracial organizations like the Student Nonviolent Coordinating Committee and the Congress of Racial Equality. He also realizes, however, that "it cannot be a long-term strategy" (p. 105).

Tucker and Peeks, like the black writers in Sochen's volume, reject revolution and violence as counterproductive. Tucker is aware that, unlike white radicals, "those who grew up in the ghetto and know the realities of police repression find little purpose or catharsis in provoking violence, in engineering confrontations, in baiting cops" (p. 145).

Tucker and Peeks know that there is no single or simple solution to the complicated, deep-seated problems facing black Americans. Both authors recognize that the black minority must develop and implement a wide variety of flexible strategies in order to achieve liberation. They see strong political organization and wise use of the ballot as a vital avenue to real power. And both agree that blacks, operating from a position of organized strength, must form coalitions with other minorities and elements of the white population.

ROBERT E. MARTIN

Howard University

The Politics of Economic and Human Resource Development. By Randall B. Ripley. (Indianapolis/New York: The Bobbs-Merrill Company, Inc., 1972. Pp. 184. \$8.00; \$3.75, paper.)

The Politics of Economic and Human Resource Development is the fifth book in the Bobbs-Merrill policy analysis series edited by Thomas R. Dye. These volumes all claim to offer original theoretical formulations in the emerging field of public policy. Ripley, professor of Political Science at Ohio State University, erects his own "analytical framework" on the premises of "systems theory" as elaborated by David Easton and by other scholars who have favored its constructs and metaphors. This framework he applies to four brief case studies of programs in the area of economic and human resource development: the Economic Development Administration, the Job Corps, Model Cities, and Appalachian redevelopment (through the Area Redevelopment Administration and the Appalachian Regional Commission).

Its chief merit lies in its assembling materials on the legislative politics, the implementation, and the social effects of each of the four program packages. Its corresponding defects, however, are that the legislative histories are dull (we are spared, it seems, the account of not even one roll-call vote), that the accounts of program implementation are superficial, and that program evaluations are unsophisticated—and occasionally credulous.

It is to Ripley's credit that the analytical framework is explicitly developed and consistently applied to all four case studies. Unfortunately, the propositions entailed by the framework are so abstract as to be either trivial or meaningless (e.g., "Policy process variables figure importantly in the production of policy statements" [p. 14]). Their application to the empirical materials is therefore arbitrary and unilluminating. For example, one of the four "policy process elements [which] emerge as particularly important" regarding the Job Corps is that "Job Corps officials agreed: that their primary goal was to help enrollees become employable in decent-paying jobs" and also that "their mission involved not simply subsidy but the manipulation of job opportunities and the redistribution of useful skills" (p. 90). Even if it were clear what exactly is meant by "the redistribution of useful skills," it would still not be obvious why these facts are "policy process elements." Nor is there any explanation why they rank among the four "particularly important" such elements in the history of the Job Corps.

Another peculiarity of Ripley's framework is that "policy results" include *statements* as well as actions and outcomes. Hence we learn that one of the three Job Corps "results" was that

"societal policy statements were expansionist throughout the Johnson administration" but more restrained under Nixon.

Ripley's lack of success with the development and application of analytical frameworks puts him in company with most other scholars who have essayed the same task. What is perhaps unique to the effort under review here is that its shortcomings are especially evident owing to Professor Ripley's commendable wish to take his own framework seriously enough to use it explicitly and consistently.

EUGENE BARDACH

University of California, Berkeley

Federal Service and the Constitution. By David H. Rosenbloom. (Ithaca: Cornell University Press, 1971. Pp. 267. \$8.00.)

Government employment has increased sharply in the last decade: from 8.4 million in 1960 to 12.9 million in 1970. This growth, coupled with persistent and vocal demands for the provision of steadier and better government service, and the increased activity and militancy of public employee unions, has brought many of the problems of federal, state, and local employment into the public eye. Mr. Rosenbloom's book, therefore, is welcome. It is a well-written and easy to read sketch of the federal employment relationship from the beginning of the Republic. It is, however, no more than this, and accordingly, it will be of little value to the political, historical, or legal scholar.

Rosenbloom traces the growth of the federal service from 1776, through the spoils system, the reform movement and the development of the modern civil service. His is a straightforward and accurate presentation, but it lacks the rich texture of good historical writing. The facts fail to sparkle, for they are not mounted in their social or political setting. Nor is there the narrative drama that fixes events in the reader's mind.

The author's principal concern, however, is not with history. He is interested in "the constitutional relationship between the citizen and the state in public employment." His approach to this relationship is set forth in the book's introduction.

The relationship is most usefully thought of as the degree of differentiation between the constitutional rights of citizens when in or in application for, and when out of or not in application for, public employment. The concept of such a relationship is intended to comprehend the distinction between citizens in public employment and others with respect to the ways in which the state may legitimately

act upon the constitutional rights of one or the other, but not both. Therefore, the relationship can take two general forms. It is nondiscriminatory when the roles of citizen and public employee are thought to be entirely compatible and there is no differentiation between the constitutional rights of citizens which is dependent upon public employment. The relationship is discriminatory when the roles of public employee and citizen are not believed to be wholly compatible and differentiation of constitutional rights is dependent upon the fact of public employment. The relationship is negatively discriminatory when the constitutional rights of public employees are subject to greater restriction and it is positively discriminatory when they are afforded additional constitutional rights. It is logically possible for the relationship to have positive and negative elements at the same time and for those elements to balance each other to cause a neutrally discriminatory relationship (pp. 1-4).

Armed with this analytic tool, Rosenbloom examines the federal service and finds that, during most of its history, the public employee has suffered negative discrimination. He has, as a condition of his employment, been forced to surrender some of the freedoms—speech, privacy, association, etc.—that he otherwise would have enjoyed. The trouble with this analysis is not that it is wrong, but rather that it is incomplete. It tells the reader nothing about the comparative intrusions on individual freedom suffered by employees of private employers. Of course, the Bill of Rights and the Fourteenth Amendment do not apply to the private employer, but that is not a sufficient excuse for the book's incompleteness. Consider, for example, the editorial writer who loses his job because his views differ from those held by his employer. Is he not in the same unhappy position—i.e., unemployed as a result of the opinions he holds—whether his employer is the government or the *New York Times*? I do not mean to suggest that the law should treat the private and public employee the same (that was Holmes's mistake in the policeman case). I do mean to suggest that neither the law nor its critics are free to ignore the comparison.

Moreover, Rosenbloom's examination of the case law in such areas as loyalty/security or employee political activity is pedestrian. All of the constitutional and statutory decisions he addresses have been better analyzed elsewhere; yet this analysis is not satisfactory. There remains important work to be done. Take, for example, the question whether political neutrality can constitutionally be imposed upon a public employee. The issue in part is whether such an imposition violates the employee's right of free association. Surprisingly, we know little about

that right. Is it mainly instrumental as Tocqueville argued, "a necessary guarantee against the tyranny of the majority"? If it is, then its reach must be judged in terms of the goal it seeks to serve. And that goal—preventing the tyranny of the majority—may well be furthered by the political neutrality of public employees rather than by their active involvement in the political process. If, on the other hand, the right of free association is seen as desirable in itself (and quite apart from whether it furthers a separate societal goal), the power of Congress to restrict the political activities of federal employees may, indeed, be meager. I suspect Tocqueville's vision is correct, but I am far from sure. I am sure, however, that Rosenbloom has not illuminated this or the many other hard legal problems touched upon in *Federal Service and the Constitution*.

HARRY H. WELLINGTON

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Prophet in Politics: Henry A. Wallace and the War Years, 1940-1965. By Edward L. Schapsmeier and Frederick H. Schapsmeier. (Ames, Iowa: The Iowa State University Press, 1970. Pp. 268. \$8.95.)

Prophet in Politics is the second half of a two-volume biography of Henry Wallace by Edward and Frederick Schapsmeier, professors of history. The first work covered the agrarian years, 1910-1940, and this effort completes the project. Beginning with the period shortly before World War II and concluding with Wallace's death in 1965, the authors examine Wallace with affectionate accuracy in his roles as wartime administrator, diplomat, cabinet officer, presidential candidate, and agricultural scientist. With less objectivity and frequent apology, they recount his activities as critic, maverick, and polemicist.

The Schapsmeiers have combed most of the relevant manuscript collections and have also corresponded with or interviewed scores of people who had political relations with the former Vice-President. Thus their book is likely to be the standard source on Wallace for some time. It is thoroughly documented (more than 500 footnotes) but written in a somewhat labored and frequently didactic style. In addition, the Schapsmeiers are uncritically fond of their subject. They picture him as a fervent idealist, totally dedicated to God, world cooperation, and humanity. If Wallace had the driving ambition, selfish thoughts, or partisan motives most men need to reach the threshold of the presidency, it is well concealed from the reader. His miscalculations and political errors often are blamed

on innocence, candor, noble objectives, or on others who misled him. The authors seem determined to protect Wallace—even from himself, and even when he doesn't need it.

Wallace was in many ways an admirable though perplexed man. He was an extraordinarily religious person who endorsed World War II, but found the Cold War irrational and unconscionable. He was a trusting, almost naive, leader in a world of pragmatic heads of state determined to maintain and aggrandize their power. He was a propagandist for peace oblivious to the need for a base from which to operate, and a leader of the moderate Left without quite comprehending the significance and responsibilities of his position. As a politician, he was evidently insensitive to the machinations of the men around him. As an agricultural scientist, he was not always perceptive of the complex relations between science and religion, science and war, and science and world affairs.

For the most part, the authors stick closely to Wallace's activities with little analysis of the larger political environment in which he moved. The rift with Truman over foreign policy is well described, but the discussion of the election of 1948 is an egregious example of the lack of extended treatment one would hope to find in a definitive work. Too much of the brief discussion of Wallace's presidential candidacy is spent defending his inconsistencies and explaining how he was duped by the Communists and fellow travelers. Surely Wallace was a victim of his own compulsions and ideals as well as of those with whom he was associated in the late 'forties and early 'fifties. Old liberal friends deserted him in his third-party candidacy, and his long and distinguished service to the nation was depreciated. Yet, in perspective, many of his principles were sound. His positions on Russia, the Cold War, and world cooperation hardly need exoneration today.

Henry Wallace was a deeply spiritual public figure who crusaded for his conception of what was good for humanity. As a politician, philosopher, and scientist, he was only partially successful, but this informative and compassionate biography, more descriptive than analytical, fails to completely explain why this was so.

DONALD BRUCE JOHNSON

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Budget Innovation in the States. By Allen Schick. (Washington, D.C.: The Brookings Institution, 1971. Pp. 223. \$6.95.)

There have been several efforts to reform budget procedures during this century. Ranging

in time from President Taft's Commission on Economy and Efficiency to President Johnson's efforts to spread Planning-Programming-Budgeting throughout the federal establishment, the reformers have sought to stretch the concern of budgeters beyond the narrow realm of financial control. Most proposals would recast budgeting to emphasize information about the details of programs and the goals of policy. Unfortunately, few of the reformers or their critics have tried to maintain consistency of terminology. The contemporary student of budgeting who would try to place present efforts in the perspective of the past must first wrestle with several meanings of "performance budgeting," "program budgeting," "fiscal control," "management," and "planning."

Allen Schick of Brookings does a creditable job of sorting out the history of budget reform in the United States and offers a standard vocabulary that seems to have a proper place for each major thrust: into would-be uses of the budget that stress "control," "management," or "planning." "Line-item" budgets serve control purposes, while "performance" budgets represent the demand for improved management of resources. "PPB" is a mixture of several orientations, with policy-planning dominant. Dr. Schick describes the complexity of PPB as a source of some problems. He argues that it is more a plastering together of different components than tightly integrated system. Its components—and their labels—promise different things to different people, who then work at cross-purposes while they seek to implement their views of PPB. Throughout the book, the author relates his tale of budgeting to the larger political process: what perspectives characterize various participants in budgeting (administrator, central budget office, chief executive, legislative), and how each component of a reform has brought support, opposition, or apathy from various actors.

It is no fault of the author that the book came out only shortly before the Nixon Administration announced the end of PPB and its replacement with yet another scheme. We must wait for the next summary treatment of budget reforms—or perhaps an article by Allen Schick—to understand how much form or substance lies within this recent change; and what it tells us about the preferences or capacities of government budgeters.

This book attempts to integrate description and analysis of budget reforms in the national government with descriptions of budget reforms in the states. Yet the allocation of space in the seven chapters does not match the prom-

ise of the title. The first two and the last two chapters deal with budget reforms in general—almost exclusively with events in the national government—while most of the middle three chapters deal with the states. Yet while Chapter 4 is labeled “The Status of PPB in the States,” it offers little information specific to the states; most of it could pass as commentary on the problems in adopting PPB by federal agencies.

The states are slighted not only in terms of the quantity but also of the quality of the attention devoted to them. Budget reforms in the national government are broadly and convincingly analyzed in as complete and attractive a discussion of budget reforms as any I have seen. Schick takes to task both the most optimistic of the reformers and the most caustic of the critics; he identifies some goals that seem feasible as well as some features in the structure of budget making that discourage certain greater hopes. In his conclusion Schick suggests some tantalizing—although briefly described—alternatives that might appear in the next rounds of reform.

The material on the states is disappointing. There is little effort to relate material on changes in budgeting to the different modes of state politics and policy making. Implicitly and occasionally explicitly we read that budget reform begins in Washington and comes later and piecemeal to the states. Yet we read that at least three states adopted components of PPB before many of the federal agencies did. We do not learn why some states adopt the reforms more quickly or thoroughly than others; we find no detailed explanation or assessment of PPB couched in the context of state politics or economics. Schick offers simplistic descriptions of changing budget procedures in a few of the states, sandwiched between cogent analysis of budgeting in the national government. There is scarcely a word about the states in the final two chapters that offer some theorizing about budgeting. His descriptive chapters on the states do not seem to inform his analysis of what happened in the recent flurry of budget reform, or what changes in procedure may hold on after the flurry subsides.

The combination of meaty treatment of national affairs and lackluster treatment of the states is confusing and unfortunate. Schick knows enough about budgeting to deliver better on his promise to write about budget innovation in the states. He buries much good material on budgeting in the national government in a book that is made unsatisfactory by its alleged focus on the states. This reviewer's advice is to scratch the second half of the title, and to

read Chapters 1, 2, 6 and 7. The result is a good book.

IRA SHARKANSKY

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American Communism in Crisis, 1943–1957.

By Joseph R. Starobin. (Cambridge, Mass.: Harvard University Press, 1972. Pp. 331. \$12.95.)

Except for the pre-World War I Socialist Party, the American Communist Party was between 1938 and 1948 the largest and most important radical or revolutionary movement in American history. One might even make a case for giving first place to the Communists on the basis of their greater discipline and internal cohesion as well as their more extensive field of influence. Individual Communist party members spent more time and energy for the party than did individual Socialist party members; many Communist sympathizers and fellow-travelers were the equivalent of Socialist party members in their service to the party.

In any case, scholarly interest in the Communist movement has been far less than its relative importance would warrant. Much more attention was paid in the nineteen-sixties to the I.W.W. than to the Communists, no doubt because the radical mood found the I.W.W. more sympathetic politically or emotionally. Serious study of the Communists in their “middle period” in the nineteen-thirties has hardly begun and is now long overdue.

From 1930 to 1939, the Communist Party's membership rose from about 7,500 to at least 75,000 and possibly closer to 100,000. In 1948, the Party still claimed almost 70,000. It was able to overcome the two crises of the previous decade—brought on by the German-Soviet pact and the expulsion of its most successful leader, Earl Browder—without serious losses. But from 1948 on, it headed downwards; and as a result of the internal crisis which tore it apart in 1956–57, it became little more than another radical sect. It has succeeded in making a modest recovery in the past few years, but it is barely back where it was about 1932, without the same élan and momentum.

The questions that beg for answers are: Why and how did it rise in its period of growth? Why and how did it fall in its period of decline?

We now have the answer to the second question before getting one for the first. Roughly the same ground was covered in a previous book by Professor David A. Shannon, published in 1959. But Professor Starobin has had a dozen more years in which to gather material

and ponder it; he has also had the inestimable advantage of greater intimacy with the party's inner workings. In this field, it is invaluable to know where the political bodies are buried, and Professor Starobin, without writing an autobiography, has made the most of his personal experiences. His book supersedes everything that has been written on this period; it belongs with the few indispensable works on American radicalism in general and American Communism in particular.

The main fascination of Professor Starobin's story is that it shows in fine detail how a radical or revolutionary movement can try to commit suicide at the same time that an attempt is being made to murder it. Without its own suicidal tendencies, the victimization suffered by the Communists in the McCarthy period would probably have hurt the party to a considerable extent, but it could never have done as much damage as the party did to itself. There was far more official violence against Communists during the depths of the depression, but it did not stop them from tripling their membership between 1930 and 1933.

Professor Starobin has written the definitive account of the Communists' self-inflicted wounds. On the assumption that a cyclical economic crisis, on the order of the depression of the early 'thirties, together with a Soviet-American war were imminent, the Communists began to make preparations to go "underground" as early as 1947, some time before Senator Joseph R. McCarthy began to do his dirty work. These apocalyptic expectations did not prevent them from taking part in—and taking apart—the Wallace movement the following year, an intermezzo of opportunism for which they paid dearly by being forced to break with the larger labor movement. The Communist aspect of the Wallace movement has never been dealt with more clearly and subtly than by Professor Starobin. Nevertheless, the party was apparently able to hold on to most of its *cadres* and a large part of its postwar membership until 1951, the crucial year in Professor Starobin's account. The most striking section of his book tells how the party leaders decided to go "underground," to conduct a witch-hunt inside the party that would have made Senator McCarthy suffer torments of envy, to shatter far more Communist lives and dismantle far more of the party's organizational machinery than all the congressional committees and volunteer Communist-hunters were capable of doing. Nikita Khrushchev's "secret speech," the Polish uprising, and the infinitely tragic Hungarian revolt, all in 1956, came as shock waves hitting a party that was

already diseased and demoralized. Those who think that McCarthyism was mainly responsible for the American Communist Party's undoing in the nineteen-fifties are especially advised to read this book.

Professor Starobin's conclusions are in part somewhat less satisfactory. Throughout his book, he shows how the American Communist leaders looked to Moscow for inspiration and even tactical guidance, despite the official dissolution of the Comintern in 1943. The following year, Browder sent to Moscow exactly those party documents which Jacques Duclos used against him in 1945. Starobin himself went on a mission to Moscow in 1951 to find out what the Soviet leaders thought about matters that struck him as "almost entirely technical" and "insignificant." Steps were taken to insure continuing contacts. Nevertheless, Professor Starobin writes: "All the evidence suggests that the C.P. leaders never really *knew* what either the Soviet Union or the international movement wanted of them—except for them to keep in step at every turn" (p. 231). This is a most peculiar exception. Was it not enough for the American leaders to know that much in order to know what the Soviet Union or the international movement wanted of them?

One need not agree with every nuance or interpretation in this book to consider it, as I do, a major contribution in this field. No one who wishes to understand what happened to American Communism can afford to ignore it.

THEODORE DRAPER

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The Institutionalized Presidency. Edited by Norman C. Thomas and Hans W. Baade. (Dobbs Ferry, N.Y.: Oceana Publications, Inc., 1972. Pp. 239. \$8.50.)

The American presidency is in "a constant state of flux," and it is therefore necessary "to reappraise the presidential office with a view toward developing a new set of perspectives for the 1970's" (p. 1). With reappraisal as their theme, Thomas and Baade have collected nine articles whose unity lies in their examination of the presidency as an ongoing institution rather than as the instrument of any one incumbent. The collection includes a number of worthy articles: Joseph H. Kallenbach on the hidden implications and unforeseen results of constitutional amendments relating to the presidency; Harvey C. Mansfield on different approaches to reorganization of the executive branch and their reflection of the differing political objectives of the president and Congress; Marver H.

Bernstein's history of the failure of attempts to improve management of the executive branch; Allen Schick's history of the failure of the Bureau of the Budget and a tentative assessment of the Office of Management and Budget's potential; Norman C. Thomas's overview of the sources of information available to the president and the limitations of each; and Hans W. Baade on West Germany's examination of its own executive, with possible implications for the American presidency. Three articles, however, are of particular interest.

The first, by David L. Paletz, presents an overview of the literature on the presidency and classifies it according to the different perspectives of the authors. This in itself is useful; far more significant is the way Paletz utilizes his material to show that trends in political scientists' approaches to the presidency mirror changes in the ideology of the political activists with whom the writers identify. Paletz suggests that writers tend to react to the most recent incumbent in formulating their theories, and that the theory accorded orthodoxy at any given moment is directly dependent on the dominant political values of the time. If he is correct, the next spate of theories should mix post-Johnsonian reaction (the president has too much unchecked power in foreign affairs and should consult with Congress before acting) with post-Nixonian reaction (the president has a chance to score great unilateral coups in foreign affairs and should pay more attention to the domestic needs of the people). The inevitable contradictions in the reactions should inspire commentators to ask whether they might not eschew formulating their theories backwards (i.e., deciding what they disliked about the last president and adjuring all future Chief Executives to do otherwise) and turn instead to a more abstract consideration of the extent and kinds of power any president and any Congress should have if the constitutional system is to function properly. A first step, of course, is deciding what "properly" means, which brings us to the next article of significance.

Senator Walter F. Mondale's article is a defense of his proposed bill to establish a Council of Social Advisers in the Executive Office. Its purpose would be to maximize full social opportunity for all Americans through improved social policy making. Mondale's dual assumptions, therefore, are that the ultimate purpose of the constitutional system is equality of opportunity, and that further institutionalization of social scientists within the executive branch would rationalize the social policy-making process. The merit of Mondale's first assumption is

matched by the naivete of his second. There are social scientists and there are social scientists, and one might wonder in what way the contributions of those like Schlesinger, Moynihan, and Kissinger—not to mention the warriors of the abortive War on Poverty—have extended equality of opportunity. It is interesting to note, however, Mondale's third implicit assumption, which is that overall planning for social justice must of necessity originate in the executive branch. Thus, along with the political scientists represented in the volume, Mondale appears to believe that the presidency is destined to ever greater size and power and that the only questions remaining are those of organizational design. (President Nixon's pre-Watergate personnel shifts, which were in effect an attempt at reorganization, indicate that he, too, accepts this premise.) Mondale here reflects the majority of his colleagues in Congress, who can usually be counted upon to give their all in the fight to render their institution as impotent as possible.

The most impressive essay, destined to become a classic in the field, is Thomas E. Cronin's "Everybody Believes in Democracy Until He Gets to the White House . . .": An Examination of White House-Departmental Relations." Cronin is concerned with the "gap" (p. 147) between the President's function of formulating policy to meet the nation's needs and that of running the bureaucracy in the implementation of such policies. The particular problem he examines here is the relationship between the bureaucracies of the White House and the Cabinet departments, and the technique he utilizes is the summation of interviews with forty-three recent White House Staff members. Cronin explores the extent and nature of the built-in conflict between the two bureaucracies; the consequences of the blurring, by Kennedy and Johnson, of the distinction between staff and line functions; the tendency of activist presidents to try to do too much too fast (which leads the reader to wonder whether the presidential term should be extended or whether the functions of the presidency should or can be more limited); the different time perspectives of the departments and the White House staffers; and the results of retaining the implementation function by the department bureaucrats even while they share the policy-making function with the staff. (It should be noted that President Nixon apparently attempted to solve the problem of the conflicting bureaucracies by substituting his own loyal appointees for the traditional career bureaucrats in the jobs of Assistant Secretary for Administration

in the several Cabinet departments.) Finally, in an excellent section on the Cabinet, Cronin supplements and updates Fenno (*The President's Cabinet*, 1959), and suggests a discernible difference in power between the "inner" and "outer" Cabinets.

The wealth of material contained in the essay is extraordinarily useful, but even more impressive is Cronin's blending of empiricism with analysis. He has never fallen prey to the fallacy that an empiricist must not think and evaluate both quantitatively and normatively, and the conclusions he draws about the implications of his material are of the utmost value. He notes that the conflict between the bureaucracies is a fluctuating and highly differentiated entity, varying with such factors as the personnel in each, the policies which are important to the president at any given moment, the kind of function performed by each aide (domestic policy, budget problems, congressional relations, etc.), and the styles of the various presidents. He concludes, in part, that the conflicts between the bureaucracies should be categorized according to their appropriateness in a democratic system and that certain kinds of conflict should be encouraged. This applies particularly to those conflicts which will generate information and alternative policy choices for the president.

This is a volume devoted to the question of how the presidency should operate rather than what the president should do. Since, as indicated by Watergate, organization tends to define the parameters of possible action, the question is an important one. The value of the book is that it will undoubtedly stimulate consideration of the various possibilities for organizational reform, and, one hopes, the potential effect of reform on policy.

PHILIPPA STRUM

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The Third House: Conference Committees in the United States Congress. By David J. Vogler. (Evanston: Northwestern University Press, 1971. Pp. 133. \$6.50.)

It would be hard to overestimate the importance of congressional conference committees, and for that matter, the difficulties involved in researching this crucial stage in the legislative process. One reason for both the importance and the difficulty is that the conference committee is, in the late Congressman Clem Miller's terms, "the ultimate flowering of the power of seniority." The senators and representatives who meet to reconcile and sometimes

go beyond differences on legislation between the two houses are almost inevitably the ranking committee and subcommittee members, majority and minority. By definition these are busy men, hard to gain access to, difficult to get them talking about what happens. Some other phases of the legislative process, like marking-up a bill, also take place behind closed doors, but most of the give-and-take of conference committee decisions seldom becomes public—the reports, themselves, and the debates which follow may cover up more than they reveal. For all of these reasons as well as the sheer range and variation of activity (something like ten per cent of all legislation passed goes to conference, including nearly all major bills) this "end game" remains one of the most fascinating but least understood phases of the legislative cycle.

Thus, it was with some anticipation that I looked forward to reviewing Professor Vogler's tight little monograph on *The Third House*. By and large, he builds well on the thin literature focusing upon conference committees: the "classics" by McCown (1927) and Steiner (1950), isolated findings from legislative case studies from Bailey (1950) to Paletz (1970), and generalizations from the committee-centered research of Fenno (1966), Manley (1970) and others. One intriguing question—why is it that almost all of this literature, with the exception of Fenno, grows out of dissertation research?

Vogler opts for aggregate analysis of many cases (all conference decisions taking place over five Congresses, 79th, 80th, 83rd, 88th and 89th, $N = 596$) rather than an intensive case study approach. Thus, he bypasses the problem of interviewer access altogether. Indeed, there is no explicit evidence that he interviewed any congressmen or staff about the process. Still, his careful qualitative analysis does provide a number of useful generalizations beyond Wilson's earlier cryptic dismissal of conference committee activity as "at best a haphazard method of compounding legislation, liable to suffer many singular accidents, and impossible for a busy people to understand when they occasionally look on with unwonted attention" (*Constitutional Government in the United States*, 1908, p. 107).

The author is interested in a number of questions: Who goes to conference, to what extent is the majority party over- or under-represented, is it the committee seniority or the subcommittee ranking which is likely to control, who wins as between the two houses and their related pairs of committees, and what form

does conference bargaining take? By the nature of the research mode he has adopted, Vogler tends to provide more satisfactory answers to the first three questions, gets into greater difficulty with the latter two. His analysis is primarily based upon public documents: the language of conference reports, debates in the *Congressional Record*, committee membership and rankings from sources like the *Congressional Directory* and *Congressional Quarterly Weekly Reports*.

For answers to the question of "who wins," he turns to descriptions from *Congressional Quarterly Almanacs*: "in most cases the House prevailed"; Senate conferees conceded to the House version . . . ;" and so on. It is on the basis of 297 such judgments favorable to one house or the other that Vogler makes his case. Regardless of whether or not one shares Richard Fenno's observation that "the central question of conference committee decision making is 'Who wins?'," it remains quite unsettling to rely on judgments about the dependent variable at least two steps removed from the language of the conference committee reports, let alone research into how the participants perceived the outcomes. The question of "who wins, who loses" is never easily answered, even when it is a matter of reconciling differing dollar amounts. All too often a majority of Senate conferees may yield on a number of amendments in order to obtain House conferee approval for one section which they deem critical. In fairness to Vogler this kind of detailed evaluation can seldom be extended beyond three or four pieces of legislation in any one Congress. Still, if one believes that the questions of "how" and "why" and "with what consequences for whom" are at least as important as "who wins," then he is likely to come away from Vogler's analysis feeling more than a little troubled.

Vogler's next to last chapter is an attempt to apply theories of formal organization and bargaining to conference committee activity based on overviews of 34 of the more conflictful cases scattered over the five Congresses. Although the theoretical refinements are suggestive, their integration with data is, at best, marginal—an occasional quote from the *Congressional Record* or the *Congressional Quarterly Almanac*. Outside influences on conference committees—the executive branch, interest groups, and constituency influences—get all of a page and one-half! Despite this monograph's limitations, and they are sometimes severe, the author has made several contributions. He has ordered the literature, outlined some the parameters for the study of conference commit-

tees, and developed some useful generalizations about who participates, if not necessarily who wins. As the author himself concludes: "What we have learned about conference interaction palls in comparison to what must be learned . . ." (p. 113).

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AKEL: The Communist Party of Cyprus. By T. W. Adams. (Stanford: Hoover Institution Press, 1971. Pp. 284. \$6.00.)

This is the first detailed work published in English on the Communist party of Cyprus. Professor Adams's study grows out of his long and persistent interest in the political affairs of Cyprus, as indicated by his two previous publications on Cyprus: *U.S. Army Handbook for Cyprus* (1964), and (as co-author) *Cyprus Between East and West* (1968). In the introduction to his work on AKEL, the author expresses his concern that "much of what is written about the party comes from polemicists and journalists who are strongly biased" (p. 1). Although Adams shows that he has undertaken long and arduous research in order to complete this volume, it cannot be considered a definitive work on this subject.

The general tone of the book tends to be polemical. From the very beginning, specifically in his preface (p. x), Adams engages in a polemic against Mr. Papaioannou, the leader of AKEL, in which Adams justifies the objectivity of his two previous works on Cyprus. The book is replete with sweeping generalizations about the character of AKEL's leadership, many of which are unfounded. For example, Adams states "AKEL is still a sleepy party ruled by an insecure and unimaginative group of old men" (p. 9). In another place, he describes the leadership of the party as "AKEL careerists" (p. 201), by which the author implies motives of opportunism that he does not satisfactorily substantiate. If one accepts Adams's characterizations about AKEL and its leadership, one finds it difficult to understand how this very party gained the highest proportion of votes cast in the 1970 Cypriot Parliamentary Elections (40 per cent), a fact which the author reports in the book's postscript (p. 205). One would have to admit that the Communist leadership in Cyprus must have been doing its homework very well, indeed.

I agree with Professor Adams that AKEL does not presently exhibit the dynamism it showed during its formative years, especially during the 1940s and early 1950s. This, however, has not minimized its influence. AKEL did en-

counter difficulties for its neutrality during the E.O.K.A. armed struggle in 1955. On this point, concerning AKEL's miscalculations at that time, the author handles the analysis well, especially in his historical presentation in Chapter 1 and in later references (pp. 59, 117). AKEL's miscalculations notwithstanding, I believe that the author misses an important point regarding AKEL's influence among the Cypriots (Greek-Cypriots). He fails to account for, as well as analyze, the fundamental fact that AKEL represents an ingrained and stable social force in Cyprus. The party's popular appeal among the Cypriot masses has to be seen in connection with its major contributions in the labor movement, and its impact thereafter in the elevation of the Cypriot working class. One cannot be expected to understand AKEL's influence in Cyprus without analyzing it as a distinct social force.

In some areas of the book, a number of points deserve a more detailed discussion. For example in chapter 4, entitled "AKEL's Conflict and Integration with the National Environment," Adams gives an inconclusive analysis of the character of the other political forces in Cyprus. A good in-depth discussion on EDMA and its successor, The Patriotic Front, would have shed light on the understanding of the "New Political Parties in Cyprus" (p. 147). In another instance, one would have to go a bit far to call EDEK a Socialist Party—a better characterization would describe it as having a program with strong "socialist" tendencies.

The chapter on "The National Environment" (Chapter 3) is rather weak, specifically the presentation on the "Greek-Cypriot Ruling Structure," the "Esoteric Tendencies of the Cypriots," "Socio-Economic Structure," and "Greek Social Structure." The analysis of these topics is too general.

Two final strictures: A number of weaknesses could have been avoided if the author had not relied so heavily on translations; and an opinion survey of the rank-and-file membership of AKEL and PEO would probably have yielded a more critical understanding of the subject matter of this book.

STANLEY KYRIAKIDES

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Documents of the History of the Communist Party of India, Vol. One, 1917-1922. Edited by G. Adhikari. (New Delhi: People's Publishing House, 1971—distributed by South Asia Books, Columbia, Mo. Pp. xxiv, 640. \$6.50.)

Communism and Nationalism in India: M. N. Roy and Comintern Policy, 1920-1939.

By John Patrick Haithcox. (Princeton: Princeton University Press, 1971. Pp. xiv, 389. \$12.50.)

During the past few years the Communist movement in India has attained a degree of respectability and influence that was noticeably lacking earlier. At the state level, leaders of the two major communist parties (the Communist Party of India, or CPI, and the Communist Party of India-Marxist, or CPM) have become Ministers and patron-wielders. At the national level the CPI has gone into alliance with the Congress party, in such a way that its leadership is now being consulted fairly frequently by both state and central governments. Moreover, Prime Minister Indira Gandhi apparently decided after the Bangladesh crisis of 1971 that India's north wind must prevail over both its east and west winds, with the result that she has surrounded herself with a number of former and present pro-Moscow communists as advisors, and they are presently engaged in a collective attempt to lessen India's dependence on the West while countering those Chinese thrusts in Asia that do not parallel their perceptions of the interests of India.

Having gained respectability and influence, both of India's communist parties (CP's) have begun to use their fairly elaborate research and publishing wings to write party histories. Aside from the ego-satisfaction that can be derived from such an endeavor, there are also practical reasons for this phenomenon. Since the split in the united CPI in 1964, both the CP's have found it necessary to rewrite history according to their own current tactical and strategic lines. Then too, both CP's have an aging leadership (the average age of the top committees in both parties is mid- to late sixties), with the result that the "old-timers" feel some compulsion to have things written down *their way* before they pass from the scene.

Dr. G. M. (known to his party comrades as "Doc") Adhikari's collection is the first in a series of sourcebooks, officially approved by the CPI, and it is intended as "basic documentary material" for "an authoritative history of the party" (p. vii). Although the book was originally designed to cover the period 1917-1925, the author found that nine years of Indian communist documents would be too much for one volume, so the present effort is restricted to only six years (1917-1922). In addition to the documents reproduced, more than a hundred pages of introductory and explanatory notes by party historian Adhikari tell the reader what he

(Adhikari) thinks should be learned from them.

There are several problems with the documents: (1) letters and communications to and from leaders of the present CPM—some of whom (such as Muzaffar Ahmad) were especially important in the formation of the united CPI—are missing; (2) many of the “documents” are extracts or excerpts of longer letters, newspaper articles, or pamphlets, edited by Dr. Adhikari on the basis of what he thought was most important; (3) the volume by no means contains a complete listing of even the most important documents of the period (Adhikari complains that he could not get the full cooperation of the Government of India’s central intelligence department, which has copies of many of the documents this volume lacks); and (4) many of the documents—such as the excerpts from M. N. Roy’s writings—are available in more complete and accurate form elsewhere. To be sure, there are some interesting things here, but one is struck by the fact that they were all written originally in English, which means that many of the indigenous Indian sources on communism during this period are completely neglected.

Dr. Adhikari’s introductory notes may be valuable to someone who is trying to fathom the minds and tactics of pro-Moscow Communist-wallahs in India, but they are of little use for anyone trying to understand the position of the Communist movement in its Indian environment. Adhikari describes the period 1936–1947 as the time when “the party develops into a mass political organization on an all-India basis” (even though at present his CPI gets about 5 per cent of the national vote by appealing to certain regions and communities, but is essentially without a “class” base). During the period 1954–1964, Adhikari argues, “the party emerges as the leading opposition party,” a rather hollow claim in a situation where the CPI has been outpolled by other opposition parties in almost every state and national election since India’s Independence in 1947.

Both Dr. Adhikari and Professor Haithcox do point up the way in which pro-Moscow Communists have historically been concerned with attempts to penetrate the ranks of India’s nationalist leadership. Indeed, Haithcox justifies another in a long series of books on M. N. Roy by pointing to the “new material” he has collected on Roy’s relationships with Lenin, with Congress party leaders in India, and with the Congress trade union movement. Through the eyes of Haithcox, Roy is seen hobnobbing with

Lenin and other international Marxists, walking the villages with Nehru, being defended by the Congress “High Command” when the British arrested him, and, through his writings and activities, influencing the course of nationalist politics particularly in the 1930s. There is indeed some new and valuable material here, and Professor Haithcox has emphasized facets of Roy’s life that usually receive less emphasis.

Professor Haithcox quotes without comment from a British intelligence estimate in the 1930s (reported by Roy himself, in a letter to Louise Geissler) to the effect that Roy was “the only man who could make communism a real danger in India” (p. 197), and that Roy “stands out heads and shoulders above all other Indian communist leaders with the possible exception of Dr. G. M. Adhikari.” There is little in Haithcox that would confirm such an estimate, indeed, there is a good deal that would make one seriously question it. Haithcox rejects the argument that Roy’s failure to rally India’s left wing behind him in unity during the 1930s could be attributed to “inordinate ambition,” instead arguing that Jayaprakash Narayan “came closer to understanding the reason for failure” when Jayaprakash pointed to “the ridiculous idea held by every miserable little party [including Roy’s] that it alone was the real Marxist Party, and that every other party had therefore to be exploited, captured or destroyed” (p. 237).

If Roy was a factionalist first—and at various times in his life a nationalist, Communist, socialist, or radical humanist second—then Haithcox has certainly captured the spirit of “Royism.” On the basis of a wide variety of primary and secondary sources Haithcox traces out many of Roy’s factional maneuverings and modifications in his political philosophy, through which, in the judgment of Professor Haithcox, “runs one constant theme—the quest for human freedom and dignity—to which Roy devoted his entire life” (p. 299).

Two irritations make the Haithcox book unsettling to read. First, since it is primarily intended as a work that will fill certain gaps, it does not fit together in very systematic fashion, has no major themes that flow consistently through its pages, and sometimes shifts abruptly from subject to subject. Second, Princeton University Press not only resorted to the increasingly frequent publisher’s cop-out of putting footnotes at the end of the book, but it then tried to correct for the resulting clumsiness by scattering some of the more informative footnotes at the bottoms of pages. The result intensifies the reader’s feeling that he or she is sometimes being subjected to a field re-

searcher's unfinished notebook.

M. N. Roy was undoubtedly an enchanting character, and he perhaps wrote (while his followers provided for the preservation of) as much material as any man that ever lived. Although each of his comrades in the various Indian leftist movements has written less, the combined literary output of the Indian left is formidable. If only because Indian Communists are frequently so fascinating, have gained a degree of influence and respectability, and have so many of their letters, pamphlets, articles, books, novels, short stories, and other documents tucked away somewhere, we can rest assured that there will be many, many books like both of these.

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Student Alienation: A Venezuelan Case Study.

By Robert F. Arno. (New York: Praeger, 1971. Pp. xxiv, 209. \$15.00.)

This case study of student alienation at the University of Oriente in Venezuela is very good. Some might argue that it is more social psychology or educational sociology than political science, but for the scholar interested in political socialization, political psychology, or political development in more general terms, this work has real merit. Unlike most case studies, the material presented here has general application, and the careful discussion of methodology employed is a real contribution for those who seek greater sophistication in carrying on this sort of research, in developed as well as emerging areas. Professor Arno has developed a sensitive instrument for measuring attitudes in an institutional setting, in this case testing for alienation in a university.

After an Introduction in which he discusses his general rationale and research plan, the author offers seven substantive chapters and a concluding summary. The substantive sections discuss such matters as the impact of university social structure on professional efficacy and upon political efficacy, the relationship between professional efficacy and political activism, student political involvement, and the concept of the efficacious student as modern man. In toto, the book offers a comprehensive picture of political activists in a provincial university whose students are recruited mainly from the poorer sectors of the population, comparing them with the alienated *universitarios*. Contrary to popular wisdom on the matter, Professor Arno finds that the Latin American student activist usually is academically successful, while the

more violence-prone alienated student tends to be fatalistic and politically passive, and somewhat less successful in his intellectual pursuits.

In terms of physical presentation this book probably is overpriced. It is short and has no index. The 146 pages of typed-offset text and 46 pages making up ten methodological appendices contain few maps or illustrations, and the tables also are typed. Even in this era of fantastic costs for book making, \$15.00 seems excessive. Considering the quality of the substantive contents, it is a shame that Professor Arno's work may be priced out of the general academic market.

ROBERT E. SCOTT

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The United States and the Trujillo Regime. By

G. Pope Atkins and Larman C. Wilson. (New Brunswick, N.J.: Rutgers University Press, 1972. Pp. 245. \$10.00.)

The Trujillo regime was one of the most cruel and harsh dictatorships ever to exist and is a perfect choice for a case study to demonstrate the dilemmas faced by the United States in dealing with nondemocratic governments. The policy of the United States toward Latin America is frequently criticized very severely, and Atkins and Wilson attempt to evaluate that criticism through careful analysis of the intricacies of United States relations with the Trujillo regime.

In an effort to make the study more than a historical-descriptive survey, the authors employ a "policy framework" model analyzing policy in terms of "policy objectives, policy instruments and techniques, and capability analysis" (p. 4). While the authors make an elaborate presentation of their analytical framework in the first chapter, the framework is not very rigidly applied in their analysis. Their analysis of United States policy toward Trujillo hinges on the conflicts between the nonintervention doctrine and promotion of democracy and stability in Latin America and particularly the Caribbean.

Despite the failure to apply the framework consistently, Atkins and Wilson present a very useful and interesting study. As they point out, it is the first study to cover the entire span of United States-Trujillo relations. Indeed, they review pre- and post-Trujillo relations with the Dominican Republic as well. The chapter on the post-Trujillo era is an excellent analysis of the problems created by the political vacuum which follows the overthrow of a well-established dictatorship.

The recurring theme of the analysis is that

the United States must recognize that its policy has to be adapted to differing cultures and experiences. The Latin American development experience is different from that of the United States, yet United States policy makers are often unwilling to accept the fact that political development can take forms different from ours. The authors' definition of political development seems to be "representative democracy," an indication that they may follow the same tendency for which they criticize the policy makers.

In attempting to evaluate the policy of the United States toward Trujillo the authors constantly ask whether it could have been any different, and if it had been different would the Dominican Republic have fared differently. They answer their questions by noting that the United States has to deal with political realities, and although it might want to promote democracy in Latin America, there are very real practical limitations to what it can accomplish. In effect, critics of United States policy are usually answered with a justification of the policy on the basis of political realities. As the authors point out, one of the political realities in their case study is that Trujillo was extremely shrewd in recognizing where his best interests lay—usually in strong alignment with the United States, at least until the late 1950s when the United States cooled toward him. Although the authors do raise some criticisms of their own, the major emphasis of the work seems to be on justifying United States policy rather than analyzing it. Nevertheless, the study does represent a great deal of in-depth analysis not previously available and the authors' technique of working in fascinating detail makes it a very interesting one as well.

The authors conclude that the United States has been largely unsuccessful in its policy of fostering democracy but that it has had other impacts on the area. They are particularly critical of the United States' tendency to view political development in terms of economic growth. The frequent Latin American obsession with economic development resulting in neglect of the development of political institutions may be attributed to the United States' unbalanced emphasis on economic growth.

The volume will be particularly useful to the student who wants a good review of the major United States doctrines in its Latin American policy. Particularly instructive is an analysis of the inconsistency in and the problems of defining the various policies. Also very helpful is the thirty-nine page bibliography which is the most complete available on the Dominican Republic.

The authors are successful in making this

case study what a case study should be—a lesson about some larger concern. They have made the Trujillo regime a good example of the dilemmas and embarrassment of the United States in dealing with dictatorial regimes while espousing democratic ideals. In addition they have demonstrated that there are many political limitations on the power of the United States to influence its neighbors, even though it would appear to have an imbalanced advantage.

N. JOSEPH CAYER

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Bureaucracy and Politics in India. By C. P. Bambhri. (Delhi: Vikas Publications, 1971. Pp. 349. Rs. 36.)

Dr. Bambhri's thesis is that Indian institutional structures are insufficient to control the bureaucracy and that unless the political parties establish legitimacy and hegemony over the bureaucracy, it will continue to arrogate power to itself. His analysis is divided into two parts. The first is devoted to an exposition of the party system, the institutional controls of the Indian Parliament and the Prime Minister over the bureaucracy and the organization of the bureaucracy itself. The second half seeks to illustrate these theoretical relationships by case studies of T. T. Krishnamachari, M. H. Patel; Krishna Menon as Defense Minister; the Voice of America Agreement; Lal Bhadur Shastri's relationship to the bureaucracy; and the conflict between Gulzari Lal Nanda and L. P. Singh.

Indian political development has been characterized by the conversion of a nationalist political movement to a polarized party system. In the transition, the disappearance of symbolic leaders of the Independence movement, the re-emergence of local and regional political aspirations, the absence of a parity relationship between electorate and political leaders, and the slackening of momentum of developmental efforts, have pushed the crucial decision making upon the bureaucracy.

The author's characterization of the Indian Civil Service (ICS) as generalist, unresponsive, elitist, middle-class, and urban oriented does not fairly assess its contribution to nation building in the post-Independence era. A one-page analysis of the other classes of Indian civil service hardly does justice to their accomplishments in railway, transportation, health, and hydroelectric power development.

Students of Indian politics should carefully read the chapter on institutional control in order to understand the technical parliamentary maneuvering in dealing with the various issues which faced the nation.

Indecisive and second-rate political leadership factionalism, political defections, communal groups, the lack of meaningful social dialogue leading to national consensus—all result in the conclusion that the “bureaucracy is left free to accomplish whatever it sees fit” (p. 111).

The five cases studies offered display the various patterns of relationship which may emerge. They illustrate the following patterns:

(1) Strong supporting bureaucracy, strong political leaders, strong supporting party, strong opposition party attack (T. T. Krishnamachari—H. M. Patel).

(2) Strong opposing bureaucracy, strong political leader, strong supporting party, strong opposition within supporting party (Krisna Menon as Defense Minister).

(3) Strong bureaucracy, weak political leader, strong opposition party activity (agreement with Voice of America).

(4) Strong bureaucracy, weak political leadership, divided support within ruling party (Lal Bahadur Shastri as Prime Minister).

(5) Strong bureaucracy, weak political leadership, strong opposition from segments of ruling party and opposition groups (Gulzari Lal Nanda—L. P. Singh).

What disturbs this reviewer is the subtle innuendo that an administrative bureaucratic conspiracy against political leadership exists in India.

It must be stated here that bureaucracy is never indefensible. It operates the levers of power in a very subtle and anonymous manner. The fraternity of the ICS, when a brother ICS is attacked, tends to combine together and pulls the string in a subtle manner and reaches the highest echelons of political decision-makers (p. 251).

Ultimately a more evenhanded evaluation may be reached which will give the ICS greater credit for maintaining higher ethical values in government than would prevail if the English tradition of the bureaucracy did not persist. Innuendoes of conspiracy will be tempered by the realization that the power vacuum left by the British was filled in part by those who had experience in governing under well-defined standards, even though they may have also been elitist, western in cultural outlook, and generalist in orientation.

BENJAMIN N. SCHOENFELD
Temple University

Education and Politics 1900–1951: A Study of the Labour Party. By Rodney Barker. (Oxford: Oxford University Press, 1972. Pp. 173. \$10.25.)

Social critics from Matthew Arnold to Ray-

mond Williams have recognized the influence of education in determining class stratification in Great Britain. While the aristocracy and the newly rich segregated their children in public (i.e., boarding) schools, a state system of elementary education for the mass of the people did not develop until after 1870. If literacy was more widespread in England than in many parts of Europe, it was not universal until the end of the Victorian era. One might, therefore, expect the Labour Party to be preoccupied from its inception with educational reform as an essential instrument of social change. Yet, as Dr. Barker demonstrates in his study of Labour's attitude towards education between 1900 and 1951, the party consistently accorded the issue a low priority among its professed goals. Rarely in the vanguard of educational reformers, Labour politicians were content to adopt the remedies of other parties, endorsing those changes which promised to disseminate benefits more widely, but eschewing innovation. However much it aspired to a more egalitarian social order, in practice Labour adhered to prevailing elitist values, including the notion of education as a competitive contest in which only the deserving attained the prizes.

Labour's educational sights, the author contends, “were fixed on the top of the nearest summit” (p. 101). Thus in the early years of the twentieth century it aimed at nothing more than an extension of the opportunities provided by the elementary school and the concession of free places in secondary schools for working class children. Only after the introduction of universal secondary education in 1944 did the party begin to consider the social implications of private schools. Throughout the period Labour concerned itself not with the content or even the structure of education, but with its accessibility. Like wages or housing, education was regarded as a commodity whose distribution might be rendered more equitable, but whose basic premises were not in dispute. For Socialists, Barker suggests, “the problem was not to abolish the peaks of inequality, but rather to obliterate its troughs by massive improvements in the condition of the people” (p. 101). Such conflict as existed in Labour thinking was between those who wanted equality of opportunity, associated with unhindered access to the academically prestigious grammar schools, and those who sought equality of provision, attainable only through a more uniform educational structure that avoided the pitfalls of early selection. The majority of Labour politicians clearly favored a system which retained the grammar school as an exclusive institution

but facilitated working class entry. Many of the early Labour leaders who had been denied educational advantages saw their own success as a model of mobility. They objected not to class distinctions, but rather to those obstacles to advancement which fortitude and fair competition could not overcome. If Labour generally supported legislation that extended opportunity, many of its spokesmen judged educational questions in terms of immediate benefit for the wage earner. Thus during the debates over the Fisher Act of 1918, trade unionists opposed provisions for continuing education that might deprive families of the wages of adolescents. Conversely, during the 1920s Labour ministers proposed the raising of the school-leaving age as a means of reducing unemployment.

Barker interprets this indifference to the broader educational issues as reflecting a lack of Socialist principle, presumably on the grounds that Socialists genuinely committed to social change would have adopted a radical educational policy. Instead of reconstruction, Labour settled for fair shares and tended to emulate the pragmatism of the older parties. None of this is very new to students of British politics, but the author seems to regard his analysis as strongly revisionist. In his view other historians have egregiously exaggerated the Socialist ingredient, ignoring the fact that Labour was firmly in the "tradition represented by liberal collectivism and social radicalism" (p. 11). Barker announces, as though it were a startling discovery, that Clause IV of the party constitution avoids the term Socialism, and he proposes "Labourism" as an appropriate designation for party ideology. But surely it would be more sensible to accept the Socialist self-description at face value and to assess its significance, than to reject it for lacking specificity and for failing to measure up to some objective criterion. What is distinctive about British Socialism—its strength and its weakness—is its very eclecticism, its strong roots in a democratic, liberal tradition, its disregard for the finer points of the class struggle. If Labour was indifferent to educational matters, it was because Socialist politicians, probably unwisely, regarded them as less important than common ownership and full employment.

As a survey of educational policy, the work is competent, but the larger questions are all too often lost in a morass of conference resolutions and committee reports. The focus tends to be on politics at the center, to the neglect of any intellectual dialogue carried on in pamphlets and periodicals. Even the writings of R. H. Tawney, cited as the architect of Labour

views, receive only cursory examination. Adult education is briefly sketched, but no attempt is made to explore the content of W.E.A. classes or weekend schools. Nor is there any effort to explain the shift in Labour's attitude after 1951 in favor of the comprehensive school and toward greater concern "with the shape of society, and even with the quality of life" (p. 97). Although the author offers a useful elucidation of Labour's views on education, his contribution to a deeper understanding of the party is slight.

F. M. LEVENTHAL

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Unions, Parties and Political Development: A Study of Mineworkers in Zambia. By Robert H. Bates. (New Haven and London: Yale University Press, 1971. Pp. xi, 201. \$12.50.)

Demonstrating the crucial importance of copper for Zambian well-being—in 1966, copper represented 95.3 per cent of the £236 million in Zambian exports—Professor Bates calls our attention to an important problem facing the Zambian government, as well as other governments confronting the dilemmas of rising demands on scarce resources. The problem is how to transform an organization designed for making demands—in the Zambian case this is the African Mine Workers Union—into an instrument of government for the limiting of demands and for the increasing of production.

Despite vast increases in capital investment since Zambian independence, average productivity has decreased by 12 per cent, while wages have gone up, and industrial discipline has faltered. Bates shows that in its attempts to rectify the situation by exerting political control over the organization of the AMWU, the government has largely failed.

Though the government has been successful in convincing the national leadership of the AMWU, it has failed to convince the local branch leaders, who time and again have rejected government orders to curtail wage demands and to promote industrial peace. In some of the strongest chapters of the book, Bates shows convincingly that branch leaders cannot follow government directives because their position rests not on the good will of the government, but on the militancy with which they are able to articulate their workers demands to the expatriate managers. In the few cases where the United National Independence Party (UNIP), the ruling party, has tried to substitute its own men for labor leaders, it has been rebuffed by the rank and file. For the rank and file does not seem to want to take a "na-

tional" view of Zambian development. The workers are jealous of their rights and prerogatives, and their reference groups are not the poor peasants in the villages, but seem to be the expatriate managers and supervisory staff whose wages have grown from precolonial levels. This places UNIP in a difficult bind: Should it side with the workers against the foreign companies it would hurt the Zambian development program; should it side with the companies against the workers it would look as if it defended expatriates against Africans. It would seem that one solution might be nationalization. But even after August 1969, when nationalization of the copper industry took place, the rank and file refused to sanction government imposed union leadership.

Bates's book deals in an original way with a significant issue of political and economic development too long neglected by the literature on the "third world." It is rich with insights and detail, and it is written with great clarity. It may be suggested however, that by viewing the conflict between government and the union in organizational terms only, Bates may be neglecting two other problems of interest to political scientists, the first pertains to the survival of industrial democracy in Zambia, and the second to the problem of class formation in Africa. Turning to the first, the question may be posed: how is it possible that the principle of industrial democracy—the free and fair elections of branch union officials—has not been subverted in Zambia when it has been politically abrogated in so many other of the third world countries? Is the ruling elite so committed to democratic procedure that it refuses to exert force on the AMWU? Or contrarywise is it that the AMWU has the power to insist on democratic procedure? If it does, where does this power stem from? Bates does not tell us.

Nor does he address himself to the issue of class formation. Though he brings out with some force the structurally induced conflict between African workers and expatriate managers, by referring to the political class as "government," Bates overlooks the possibility that the rank and file does not accept government directives because it finds illegitimate the economic and status positions of the governing elite.

We have evidence from other parts of Africa such as the Nigerian General Strike of 1964, and the Sekondi-Takoradi strike in Ghana under Nkrumah, that African workers are responding not only to the immediate context of industrial relations, but to the broader context of class formation. In postcolonial African so-

cieties class formation is proceeding from the top down, from the pinnacles of the political class made up of successful politicians and senior civil servants down to the unmobilized peasantry. Somewhere in the middle, nearer the political class than to the peasantry lies the industrial working class. But it is a class which has not as yet learned to accept its position below that of the political class. Why should an industrial worker with ten years on the job make less than a young parliamentary secretary straight out of university? The answer is obvious only in societies where criteria for class formation such as educational level have become widely legitimate. In African societies, the legitimacy of class differences is still very much in question, and as long as such questions persist, there will be conflict between industrial workers and governing elites.

ROBERT MELSON

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The Nyoro State. By John Beattie. (Oxford: Oxford University Press, 1971. Pp. 280. \$9.75.)

The great merit of this work is that it makes Beattie's important studies of this major Ugandan kingdom easily available to specialist and newcomer alike. Basically it is an expansion of the first half of Beattie's earlier student text, *Bunyoro, an African Kingdom* (Holt, Rinehart & Winston, 1960), and as is to be expected from its author, it is a humane book, written in clear, uncongested English, with a minimum of the foreign language terms with which anthropologists sometimes protect the sacred inviolability of their message. Three main sections deal with myth and history, with the traditional kingship, and with changing political relationships and roles in the colonial period. The book ends with an essay comparing the traditional Nyoro kingdom with its more highly centralized neighbor and rival, Buganda. In taking up these topics, the book provides a useful synthesis of its author's earlier writings on the kingship, chiefs, chiefship, land tenure, and "democratization." New material and analysis bring the history of Bunyoro, in general terms, up to the abolition of the kingdom in 1967 and treat more freely and fully than before the major internal political dispute at the time of Beattie's fieldwork in the early 1950s—the attempt to wrest from the king the control of appointment to chiefships which he had obtained under the 1933 Bunyoro Agreement.

Much is therefore provided by this book, but the reader, particularly the reader to whom a good deal of the material is already familiar, repeatedly finds himself in the ungrateful posi-

tion of asking for more. This is partly because Beattie's reputation runs before him. He has always been concerned with the political in general and with the effects of European overrule on indigenous politics in particular, and he has contributed most valuably to the understanding of African kingdoms under colonial overrule and in precolonial days. His work on the limits of despotism in traditional African settings and on the paradoxical results of Indirect Rule policies, leading in the Nyoro case to a more than traditional measure of autocracy, are important instances of this concern, raising expectations of a major new work. High expectations are also aroused by developments in the disciplines concerned since the 1950s, the growth of African history as a specialized field of research, the eruption of political scientists into Africa, and particularly the movement of political anthropology from its early preoccupation with administration and its functional alternatives to its current awareness of competitive processes as a major theme.

And finally, Beattie's text itself constantly raises expectations by stimulating the reader's imagination and suggesting questions beyond the kinds of analysis and explanation actually offered. The reader is told, for instance, of the myths and traditional histories of Bunyoro and their congruence with social and political values. He is reminded that such stories have no necessary, unchanging form so long as a written version has not become the sole authority: names and generations in the royal genealogies, for example, have multiplied prodigiously since they began to be written down. He is likely therefore to ask how these stories and their possibly varying forms actually related to the political process: Who knew them, and who told them? In what contexts were they told? What was their importance to their hearers? Were there rival versions? And so on. The reader has also been reminded that new media for the dissemination of such material were already becoming available: some of the earliest publications in the indigenous language were of this kind. He is therefore certainly likely to be interested in the changing situation and its relevance to the political importance of the old stories, and possibly to the creation of new ones.

In a rather similar way, the chapter on "The Modern Kingship," to take a further example, offers tantalizing glimpses of the king, his chiefs, and his people in action. A senior chief spears the king's special bull as it roams the capital one night, and he is dismissed from his post and prosecuted for it. Through the recounting of such incidents the reader is left in

no doubt that there was a bitter contest between the king and his opponents, but this does not lead on, as he hopes it will, to a systematic analysis of the parties and the interests involved in the dispute, of the resources they could bring to bear and the strategies they pursued. It is clear in this context, incidentally, that Protectorate Government officials were very much a part of the relevant political field, and it is a pity that the book's "Nyoro State" frame of reference tends to leave them as peripheral "voices off stage." Again, in the crucial area of appointment to chiefships, Beattie provides fascinating material on the kinds of people who became chiefs, the procedures prescribed, and the way in which, according to rumor, things actually occurred. The reader's appetite is whetted for a final direct discussion of how some individuals succeeded in obtaining preferment, how others failed, and how preferment was related to the larger political process. The discussion does not come and he is disappointed.

The book conveys strongly a sense of what it was like in Bunyoro in the 1950s, and through its basic categories of traditional and modern, it places its observations within a general framework of change. The ungrateful reader may ask for more but he has been given much already. It is valuable to have Beattie's political material concerted into a single volume; and it is a volume which will provide as useful an introduction to the more detailed study of African kingdoms as his earlier volumes have provided for other and more general fields of anthropology.

S. R. CHARSLEY

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Origins of the Chinese Revolution, 1915-1949.

By Lucien Bianco, translated from the French by Muriel Bell. (Stanford: Stanford University Press, 1971. Pp. xiii, 223. \$8.50.)

Dozens of fine monographs on modern China have appeared during the past two decades, and all provide important insights into various aspects of Chinese history and institutions. And now Professor Bianco, with an unerring eye for the essential and germane, has tied all these strands together and has produced the best introduction to Chinese Communism ever published. (This is one case in which the blurb is absolutely true.) I know of no other work on modern China that can match its cogency of analysis or its lucidity and succinctness of presentation. Bianco's work is much more than another display of the Gallic talent for grand synthesis. His expertise on the peasant problem showed him where to look for the real motive

force in the rise of Chinese Communism.

Why did the Communists succeed in winning the support of the peasants? Bianco agrees that the Japanese invasion of 1937 gave Mao the opportunity to exploit the rising tide of nationalist sentiments. He then goes on to show that nationalism was not the only ingredient in the Maoist formula for revolution. Nationalism was in fact the common platform of all Chinese politicians and leaders in the 20th Century, but as Bianco points out, "It was through Communism that nationalism triumphed" (p. 166). The difference between the Communist and other brands of nationalism was that "in addition to being authentic patriots they [the Communists] were genuine revolutionaries, men who understood the needs of the people, knew what changes had to be made, and set about making them" (p. 159). By providing the peasants with better government, by allowing them to participate in the governing process, and by affording them greater social justice, the Communists won mass support during the Second World War. That support not only enabled them to resist the Japanese, but it gave them power to win the postwar civil conflict. When the war broke out in 1937 there were 80,000 men in the Red Army. In 1945 there were 900,000, plus a militia force of 2.2 million men. Yet as Bianco reminds us, this was not the whole story. "It was during the Second World War that the Kuomintang *lost* the civil war" (p. 159). Kuomintang leadership was incapable of ameliorating rural distress during peacetime. The war aggravated this social misery and created even greater challenges which further exposed the government's inadequacy.

In explaining the Communist success Bianco focuses on the essentials. A turbulent peasantry and a pervasive nationalism—fired by a significant measure of xenophobia—added up to a revolutionary situation that could not be relieved through gradualist methods. (Here the author is particularly instructive in showing the inefficacy of well-intentioned rural reformers who attacked the symptoms rather than the causes of peasant poverty. He tells us of idealistic students who tried to promote hygiene by distributing pamphlets on the use of toothbrushes to the peasants when these people did not have the price of one toothbrush for an entire family.) The Communists created a disciplined party and effective army using this social and emotional force. Bianco furthermore makes it quite clear that it was not just that the Communist leadership was more efficient or capable in exploiting this objective situation, though as a good historian he notes that the

presence or absence of effective leadership is in itself an important historical datum. The entire history of China since the Opium War of the last century shows that China could not stand up to the West until it fully mobilized its resources, both human and material, and that such a mobilization was impossible as long as the socioeconomic structure was dominated by the gentry-landlord stratum. The social revolution was therefore an indispensable condition for realization of nationalist goals. The Kuomintang, however, became even more conservative as the war progressed. "Installed in the back country and cut off from the merchant bourgeoisie of the eastern ports and great cities, its social base consisted almost exclusively of that most conservative of classes, the large landowners" (p. 160). Real agrarian reform would have threatened to destroy the social base upon which the Kuomintang was built.

As Americans digest the implications of Bianco's account for United States policies toward Asia, they should be reminded that many of his important conclusions were being reported to Washington by official American observers more than 25 years ago, when the Communists were first emerging from obscurity. In September-October, 1944, for example, John S. Service, after several months of close observation of the Communists, reported that "The peasants support, join and fight with the Communist Armies because they have been convinced that the Communists are fighting for their interests, and because the Communists have created this conviction by producing tangible benefits for the peasants. . . . The common people, for the first time, have been given something to fight for" (see Service's *Amerasia Papers: Some Problems in the History of U.S.-China Relations*, Berkeley: Center for Chinese Studies, 1971, pp. 152, 161). As for the Kuomintang war effort, the most devastating criticisms can be found in General Wedemeyer's reports of 1944-45, reproduced in the official U.S. Army history (see *Time Runs Out in CBI*, by Charles F. Romanus and Riley Sunderland, Washington, D.C.: Office of the Chief of Military History, U.S. Army, 1959). If Washington subsequently failed to understand the reasons for the victory of Communism in China, it was not due to lack of information.

In proving once and for all that Chinese Communism is an indigenous phenomenon with deep roots in Chinese society and history, Bianco also brings Mao Tse-tung's role into proper perspective. He sees Mao as a "mediocre theoretician" whose "strategy seems to rep-

resent the triumph of common sense" (pp. 79–80). But in questioning the myth of Mao's infallibility and profundity as a thinker, Bianco makes Mao's greatness seem much more credible. More than any other modern Chinese leader Mao was aware of peasant bitterness, and no one worked more persistently to turn this bitterness into a political and military force. Bianco sees Mao's greatness in terms of his heroism, toughness, audacity, and the "courage to free himself from dogma" (p. 80). The author nevertheless reminds us that given the backwardness of a country like China, the real test of this heroic leadership begins only after power is won.

Bianco's perceptive and well-balanced study must be read by everyone who wants to understand what the Chinese revolution was all about.

HAROLD Z. SCHIFFRIN

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Constitutions of the Countries of the World.

Compiled and annotated by Albert P. Blaustein and Gisbert Flanz. (Dobbs Ferry, New York: Oceana Publications, 1971–. Twelve volumes, loose-leaf, \$69.00 each.)

In addition to the first volume of this compilation, supplied by the publisher for review purposes, I have been able to examine three of the five further volumes which were issued in 1972. All four conform to a basic design which is to provide, for each country, (1) a chronology of constitutional development, (2) the text of the current constitution in English, and (3) an annotated bibliography of selected primary and secondary materials relating to the country's constitutional life.

The over-all plan calls for approximately 10,000 (!) loose-leaf pages of text, to be brought up to date by annual supplements. The editors commit themselves, in an introductory statement (which is of bibliographical interest itself as a review and assessment of previous efforts along the same lines), to currentness of the collection as their first priority. If they succeed in this, they will have created a uniquely valuable research resource. This will be especially true if the updating includes not only constitutional documents but also the chronologies and bibliographies.

Examination of the first four volumes indicates that the chronologies are almost uniformly excellent and the bibliographies soundly representative. (I missed under "Uruguay" references to the work of Ernst-J. Kerbusch, and I suppose that others will spot comparable omissions, but generally the scope of the included

literature is international and current.)

The only comparable collection in the English language is Amos Peaslee's *Constitutions of Nations*, now in the third edition. It, however, lacks the flexibility of the loose-leaf format, its chronological summaries are much briefer and often not current, and the bibliographical material of the latest edition is quite limited. "Blaustein and Flanz" will undoubtedly replace Peaslee as the prime reference work for information on the constitutions of the nations.

FRANCIS H. HELLER

University of Kansas

The West German Legislative Process: A Case Study of Two Transportation Bills. By Gerard Braunthal. (Ithaca: Cornell University Press, 1972. Pp. xviii, 290. \$11.50.)

In tracing the stormy course of two transportation bills in the mid-1950s, Professor Braunthal usefully contributes to what, one hopes, will be an increasing number of empirical studies of the West German legislative system at work. The author, privileged to receive access to confidential ministry files, provides a revealing glimpse of the executive role in legislation and of interest-group interaction with the administration in the Federal Republic. This perspective lends depth to his discussion of the more public phases of the legislative process in the West German parliament. Students of German politics and of comparative politics will welcome this brief but searching study.

Underlying the Transportation Finance Act of 1955 and the ill-starred Highway Relief Bill was the growing competition which the burgeoning trucking industry gave to the railroad system of West Germany. By 1952–1953, the Bundesbahn faced a financial crisis, and the road network showed signs of strain. The two transportation measures powerfully affected major productive enterprises of the country, and evoked strong responses from a considerable variety of interest groups. The ensuing controversy divided the coalition cabinet, notably the Ministries of Transportation, Finance, and Economic Affairs, and cut across party lines. It sparked a CDU/CSU rebellion on the floor of the Bundestag. The Highway Relief Bill was permitted to die in committee. The tax provisions of the Transportation Finance Act then became the main stakes in the battle of the interests. In its final form, the Act fell considerably short of providing a blueprint for future transportation development. Braunthal's step-by-step account of the two bills is located in the context of both earlier and later transportation policy in Germany.

Regrettably, Braunthal chooses not to consider in any more than a handful of cursory footnotes Gerhard Loewenberg's masterful volume, *Parliament in the German Political System* (1967). He does not comment on Loewenberg's scheme of case selection, which prefaces, and is exemplified in, more than one hundred pages on lawmaking in West Germany: "The examples chosen have been selected, not so much as representative, but as limiting cases of the kinds of treatment which legislation receives in the Bundestag. They indicate the perimeter within which most cases occur" (Loewenberg, p. 282). By contrast, Braunthal does not make clear the grounds for choosing the two transportation bills to illuminate the West German legislative process.

Braunthal professes an interest in testing some general hypotheses:

The more political actors involved in the decision-making process, the more difficult it becomes to measure their power and responsibility in any one decision; the more opposition develops to a bill, the more concessions the executive makes to the political actors of the other subsystems (political parties, legislature, and so forth) to assure its passage, and vice versa; and the greater the adverse economic impact of a bill on interest groups, the greater and more successful their resistance (p. xi).

Nowhere, however, does he provide operational definitions of the key terms of these hypotheses or place conditions on the evidence he will accept. Nor does he indicate how it is possible for the two transportation bills alone to serve as a test for the hypotheses.

Apart from these conceptual difficulties, the book is a useful supplement to constitutional descriptions of the West German legislative process. In what it says of the functioning of the ministries, the cabinet, and major interest groups in the controversy over the two transportation bills, the book casts light on aspects of the legislative process not normally open to public scrutiny.

ARTHUR BRUCE BOENAU

Gettysburg College

Police State. By Brian Chapman. (New York: Praeger Publishers, 1970. Pp. 150. \$5.00.)

Since Hitler's Germany, the term "police state" has come into everyday usage. It has assumed a negative connotation, projecting visions of arbitrary and draconian police actions, executed with total disregard for human rights, freedom, and dignity. Consequently, the term is often used (or misused) by those opposed to police generally as they attempt to sway public feeling. The author posits an important ques-

tion: "Why is it that the idea of police rule is so much more offensive than the idea of, say, military government?" (p. 94).

Although primarily descriptive of European police states, Brian Chapman's book impressively clarifies the term and puts the concept of a unitary police organization into proper perspective. It does so by tracing the history of the term "police state" from its origins to its entry into the English language and presents collateral information in a logical, orderly fashion. Chapman identifies and analyzes three different classes of police states: (1) traditional; (2) modern; and (3) totalitarian.

Although the descriptive chapters are not as fast-moving as his later material, they are necessary for a thorough understanding of the comparative forms of centralized police. Each class is examined in detail, using a comparative approach that allows easier identification of the similarities and differences among classes.

Of special interest to American readers is Chapman's analysis of certain European police methods. He argues that spying, brutality, and the arbitrary use of power are certainly concomitants of any police system, but that their incidence cannot be taken as *prima facie* evidence of a police state. In fact, these methods result naturally when any police agency is regarded with indifference by an apathetic public.

Brian Chapman sees alienation from the public as a breeding ground for overt abuses and usurpation of police powers. Taking his argument a step further, the author reasons that it is the social reformers, through their insistence upon immediate social change, who actually catalyze the emergence of a police state. So the police state stems from the regime's virtually unbridled authority to use coercion to maintain order, i.e., the status quo. This virtually forces the police, as agents of the state, to resist change and engage in periodic violence.

Chapman's analysis of Germany's political evolution from a modern to a totalitarian police state under Hitler's rule should prove enlightening to students of public and comparative administration. In this context he examines and explains the administrative intricacies involved in the development of a police state noting that police apparatus must become superior to political administrative officers, the judiciary, and the military before a true police state can exist.

Although the book deals with the concept of the police state as a political phenomenon, Chapman's chapter on police psychology is highly relevant to practitioners and academicians interested in the administration of criminal justice. In this chapter, the author cuts to

the very heart of police work and examines the "whys" of such police issues as role, violence, corruption, loyalty and morality. While based on European experience, Chapman's observations apply to the contemporary American scene. Moreover, the analogy of the police profession to a combination of soldier, priest, and artist is especially provocative, though not new. (See: August Vollmer, *The Police and Modern Society*. Berkeley: University of California, Bureau of Public Administration, 1936, p. 22.)

In summary, *Police State* is short, readable, timely, well documented (74 citations), and based on substantial research (56 authors are listed in the bibliography). It should be useful to students of political science, particularly those whose interest is comparative government. The book will have little value to American police practitioners, however, for it deals only with European police states. Brian Chapman has achieved his goal of enabling students and others to understand and apply a key concept in political science.

SAMUEL G. CHAPMAN

University of Oklahoma

The Indian Army; Its Contribution to the Development of a Nation. By Stephen P. Cohen. (Berkeley and Los Angeles: University of California Press, 1971. Pp. 200. \$7.50.)

No aspect of contemporary India's national political life has been as ignored by scholars as its military system. This is in part due to the fact that Nehru's grand designs in foreign policy minimized the visibility of Indian military power. When that veil was torn in the Sino-Indian border war of 1962, most analysts began concerning themselves with new Indian strategies and ephemeral matters like nuclear options. Stephen Cohen chose instead to pursue a much more mundane but possibly more promising traditional approach to the history and structure of the military machine in the subcontinent. He managed to overcome some of New Delhi's historic and notorious sensitivity to such research, but at the price of analytical depth and descriptive candor.

The book is essentially a brief and clear historical primer on the Indian army with only one of the seven chapters on postindependence developments. (The book's subtitle should be read as a publisher's afterthought, not an author's intent.) The major questions examined by the author all concern the political control of the armed forces; civil-military relations, the nature and ethos of the officer corps, the re-

cruitment and socialization of troops and the adaptation of military management to the three great crises of the twentieth century—World Wars I and II and partition/independence.

The most interesting section of the book explores the "Punjabization" of the army, a process that involved long-term strategic choices on recruitment, regimental organization, ethno-communal balances in the forces and the relationships between officers holding different commissions. The chosen option left India and Pakistan with armies from a small and territorially discrete region of the subcontinent, with a great imbalance in communal representation (Sikhs and Muslims were greatly over-represented; high caste Hindus practically absent), and with regiments organized on ethnic and regionally distinct loyalties. This system failed to provide an adequately broad recruitment base in the World Wars, but it was an optimum approach to civil control by the imperial power.

The structure of the army system perhaps explains why Indian nationalism did not have a greater effect within the military forces. The chapter that deals with this matter focuses on the Indian National Army—a force of prisoners of war organized by the Japanese to aid them in the conquest of South Asia. Dr. Cohen believes that the political loyalty of Indian officers to the British was much more tenuous than has been generally thought, and that its political management must therefore have been a more important matter for imperial policy than is generally supposed. The INA trials after the war symbolized the dilemmas of officers caught between the values of organizational loyalty, behavior fitting and proper as a prisoner of war, and loyalty to the Indian national cause. The author sets forth the problem, but only hints at the way in which these questions were resolved, noting that "when full documentation is available it may well be seen that Nehru and Auchinleck played key roles in the relevant decisions" (p. 163).

The last chapter of the book deals all too briefly with the postindependence army and its management. There are no statistics on the numbers of officers, the sources of their commissions, the caste and locality of their recruitment, their training curriculum and specialization options, inter-service relationships or the changing social context of the profession of arms in India. Surely these factors shape self-image as much as an awareness of history. At the purely descriptive level, it would have been valuable to have the author's expert opinion on the Thimayya-Krishna Menon affair

and on the charges and countercharges of military mismanagement in the Sino-Indian war. These comments are perhaps prime examples of the reviewer's sins—noting the book that wasn't written—but one is tantalized by what Professor Cohen appears to know but doesn't say.

In his next book, perhaps the author will address himself to many of these questions, and perhaps explain such statements as "Modernization will come to South Asia, but only at a pace tolerated by the military" (p. 2). Does the Indian cabinet *have* to consider army predilections on basic development choices? Is defense industry now an imperative dictated by the forces? Has the expansion of the military budget brought an equivalent rise in the voice of the army in national politics and in foreign policy?

There is something of the modesty of a dissertation in the book, and the scholar's painstaking appreciation of historical chance is also evident. Given the paucity and the sensitivity of information on the Indian Army, Dr. Cohen has done a service to all scholars in gathering and assessing basic structural and historical evidence. His knowledge, commitment, access, and experience will doubtless lead to a more empirical and contemporary study of Indian military organization, itself rapidly changing, in the future. Political scientists can look forward to his next book, to which this one will stand as the historical preface.

WAYNE WILCOX

Columbia University

Rulers of Empire: The French Colonial Service in Africa. By William B. Cohen. (Hoover Institution Press, 1971. Pp. 279. \$9.50.)

As colonialism is being superseded by neocolonialism—and as imperial archives are gradually made available to scholars—the colonial period of Africa has been attracting an increasing amount of attention on the part of historically-minded social scientists. Some of the resulting work (like Ralph Austen's "Northwestern Tanzania under German and British Rule" or J. Gus Libenow's work on the Makonde) is mainly concerned with the impact of alien rule on indigenous African cultures; others, following Robert Heussler's study of "Yesterday's Rulers" (1963) have concentrated on the men and doctrines that made colonial policy what it was—sometimes, as in the case of Gann and Duignan, with perceptible sympathy and even nostalgia.

William B. Cohen's remarkable study of the

French colonial service in Africa (*West Africa*, really) falls in the latter category. With a felicitous combination of scholarship and writing skill, he details for us the growth of a tradition of overseas service in what may well be the most homebound nation on earth. Tocqueville suggested that French colonial administration was a caricature of the metropolitan bureaucracy. The British colonial service, one might add, could be viewed, by contrast, as a caricature of English squirearchy. But the problems faced by any overseas bureaucracy were so unique as to make all such parallels rather pointless. The French colonial service numbered some 4,000 functionaries during the seventy-three years of its existence. A body of such size, insulated from day-to-day supervision and made up of members who had, for the most part, selected overseas service as a means of escaping the confining norms of French society, was bound to develop its own traditions of independence and esprit de corps, like some sort of white-collar Foreign Legion. What is surprising, in fact, is that it did so to such a limited extent.

Cohen rightly points out that the paper centralization of the French colonial administration was largely formal. Physical distance from Paris (or even from Dakar and Brazzaville, the regional administrative headquarters of AOF and AEF), the instability of French cabinets, and the resulting incompetence of the successive incumbents at the Rue Oudinot combined to leave the Commandants de Cercle in unchallenged control of their districts as the true *Rois de la brousse*. Nor did "Colo"—the famed *École Coloniale*—instill in French colonial service the rigid, uniform, centralized sense of values which the other *grandes écoles* of Jacobin extraction provided for almost every aspect of French public life.

In what probably represents the most original portion of his book, Cohen outlines the slow development of the *École Coloniale*, and its persistent struggle to achieve an excellence and prestige that could match those of *Saint-Cyr*, *Polytechnique*, *Normale Supérieure* and other such breeding grounds of the French meritocracy. It was not until the 1930s, however, that the graduates of the *École Coloniale* were able to fill a majority of the appointments to the corps of colonial administrators. Slight wonder, therefore, that "in their methods of administration, the colonial functionaries seem to have been relatively unaffected by the *École Coloniale*" (p. 121). What the author may have overlooked, however, is the extent to

which French colonial administrators, whether or not they realized it, mirrored in their official behavior the norms of French political culture and thus tended to reproduce metropolitan routines rather than to improvise new methods, as their de facto autonomy might easily have permitted.

Bureaucracies are seldom at their best in an era of rapid political change. The French colonial service was no exception; and whatever sense of its purpose and "mission" it might have developed during the age of "classic" colonialism was largely diluted after World War II by considerations which it was not prepared to entertain. With his subject matter thus gradually fading away, Cohen falls back, during the last third of his book, on a description of French decolonization policy (or the lack of it). He does so competently—and, on occasion, brilliantly—but it is understandably not the most original portion of his work. It is at this point, however, that the reader is likely to become aware of a strange void, namely the almost complete absence of the Africans from the pages of Cohen's book. Houphouët-Boigny appears only once; Sekou Touré not at all. The dismantling of the chieftaincy system in Guinea—Touré's political masterstroke—is presented only through the rather anticlimactic comments of the French administrators whom he convened to legitimize his brilliant coup and who, whether or not they realized it, were being thoroughly manipulated by the Guinean leader. Methodologically, of course, there is nothing wrong with Cohen's treatment of his chosen subject: he is, after all, dealing with the colonizers and not with the colonized, but the absence of the colonized deprives his study of a dimension of reality which could have made it better rounded, if not more scholarly. Similarly, the author's laudable desire to maintain a value-free attitude toward his subject leads him to avoid dealing with the human quality of colonial rule. All French administrators, we are led to believe, were basically honorable men. Their professional shortcomings (documented by a skillful use of personnel files) are amply substantiated, but owing to the nature of the author's source material, these are appraised in purely bureaucratic terms. A good number of French administrators are reported to have been poorly trained, bungling, incompetent, and occasionally dishonest, but we learn little or nothing of what it may have been like for an African to live under their authority. Such is the price of objectivity. Despite such limitations, however, Cohen's book will be useful to anyone interested in the history and politics of

colonialism, as well as to those who seek to understand the distinctive political culture of French-speaking Africa.

EDOUARD BUSTIN

Boston University

Les Républicains Indépendants: Valérie Giscard d'Estaing. By Jean Claude Colliard. (Paris: Presses Universitaires de France, 1971. Pp. 352. 25F.)

French political parties as institutions have evoked a burst of scholarly interest among French students of politics in the last several years. Since 1966, serious sociological studies by Frenchmen have been published in book form on the S.F.I.O. (by André Philip), the P.S.U. (by Guy Nania and by Michel Rocard), the P.C.F. (by Annie Kriegel and by Frédéric Bon et al.) and the U.N.R. (by Jean Charlot). The Democratic Center and the Radical-Socialists are the only national parties that have escaped such scholarly scrutiny, though the studies have varied greatly in thoroughness. This useful volume by M. Colliard falls squarely in this stream of work.

In large part, Colliard follows Charlot's *L'U.N.R.: Etude du pouvoir au sein d'un parti politique*, both in compass and in structure. Their books are the most extensive, thorough, and scholarly of the lot. Both consist of a long first section (204 pp. for Colliard, 111 pp. for Charlot) presenting the development of the party chronologically, followed by another long section (129 pp. for Colliard, 190 pp. for Charlot) of topical analysis. Finally, each has a short documentary appendix (7 pp. and 37 pp.).

Colliard's greater attention to chronology results largely from the longer time span he covers (1958–70 compared to 1958–62). He labels his chronological section "*Les déchirements d'une révision*" and develops the theme that the movement was constantly torn between the past attachments of its leaders and members to traditional conservatism and their current conviction that the welfare of France (and of their electoral prospects?) required de Gaulle's leadership and, after his resignation, Gaullist unity. The author documents his thesis thoroughly and persuasively. The incohesiveness of the movement emerges clearly, and the crisis points and issues are quite visible, although their factional basis remains confused. We see Raymond Marcellin at the tip of the "*oui*" end of the spectrum and Giscard d'Estaing similarly extreme on the "*mais*" end, but the distribution in between is not clarified. Whether this confusion results from the situation in the party or

from the author's method of presentation is unclear also.

The second half of the book is divided into two parts. The first analyzes the parliamentary elections of 1962, 1967, and 1968 and the caucuses of the *Républicains Indépendants* in the National Assembly and the *Union Parlementaire Républicaine et Rurale* in the Senate. The latter part looks at the extraparliamentary structure of the party, its doctrine and program, its influence, and its image.

His treatment of elections and caucuses is quite conventional. He performs the usual analysis of both voters and M.P.s by geographic, occupational, and demographic characteristics. He also studies the parliamentary activities of the caucuses. The section on the extraparliamentary aspects of the party is the shortest (31 pp.), quite in keeping with the party's character as primarily parliamentary. Under structure, he covers the *Fédération nationale des Républicains Indépendants*, and the finances and press of the party. The most striking impression to emerge from this section is the dominance of Giscard d'Estaing in this area of party activity. Under doctrine, he considers four main points: centrism, liberalism, European integration, and Gaullism. Unfortunately, this part of the study is especially skimpy (6 pp.). Surely, a comprehensive survey of R.I. speeches in parliament and election campaigns, party resolutions, and party publications could have yielded a more substantial review of its positions.

On the whole, M. Colliard has produced a very useful study on a political party that is especially difficult to understand. The *Républicains Indépendants* are peculiarly beset by ambiguity. They are Fourth Republic parliamentarians dedicated to Fifth Republic presidentialism; local "notables" *par excellence* riding the coattails of passionately anti-notable Gaullism; organized nationally by Giscard d'Estaing, yet fiercely independent of him in their constituencies; terrified of separation from orthodox Gaullism, yet equally wary of absorption by it. M. Colliard's book provides all the information and explanation necessary for an understanding of the movement, although his style lacks the punch to make that understanding vivid.

More seriously, he has passed up a number of opportunities to flesh out spots that are especially weak. For instance, he mentions no attempt to conduct special surveys for information on and opinions of R.I. voters, yet because of the small number of R.I. voters who appear in general voter surveys, their indications are notoriously unreliable. Even less understand-

able is his failure to survey R.I. M.P.s for his profile of them. Unlike the Charlot study, we find no long list of party leaders whom the author has interviewed. In short, the study is a valuable addition to our literature on French political parties, filling a major gap, but it lacks the stylistic sparkle and the imagination to make it a truly outstanding contribution.

WILLIAM G. ANDREWS

SUNY at Brockport

Egypt Under Nasir: A Study in Political Dynamics. By R. Hrair Dekmejian. (Albany: State University of New York Press, 1971. Pp. 368. \$10.00.)

The significance of this book lies in its attempt to present Egyptian politics in a systematic framework, using as its analytical key the idea of charismatic leadership and its routinization. Having acquired his magical reputation through his diplomatic successes from 1954 to 1958, Nasser (or Nasir) sought to use it as a springboard for initiating changes in the outlook of Egyptians and the organization of their affairs, preaching the gospel of radical "Arab socialism," redistributing wealth, and launching ambitious development programs. Routinization was to come through the improvement of administration under a technically qualified elite, and through the newly introduced democratic institutions of parliament and the single mass party, the Arab Socialist Union. The author very competently describes these efforts in detail—ideological elaboration, reform measures, and institutional change—and explains why at best the routinization was only marginally successful. In an extensive and interesting discussion of the impact of the 1967 defeat on domestic Egyptian politics he shows how Nasser sought to deepen the impact of the revolution and particularly to breathe new life into the listless Arab Socialist Union.

The theme of charisma is certainly a useful and valid one in Nasser's case, and readers will appreciate Professor Dekmejian's systematic, authoritative, painstakingly documented, and level-headed analysis. Clearly he knows a good deal about his subject, and has thought his points through. He provides more comprehensive information on certain matters than is available elsewhere: the evolution of Nasser's ideology, for instance, and data on the social and career background of the 131 individuals who held cabinet office from September 1952 through 1968. Moreover, he writes intelligently and intelligibly. Overall, the book is well worth the attention of political scientists who want to read one work on the Nasser regime.

I do feel, however, that the concepts and tools of contemporary American political science not only have their limitations in enabling us to learn about such subjects as this, but may in some respects actually inhibit understanding. While Professor Dekmejian's work clearly commands respect, I am not so sure that he has managed to shine as much light on Nasser and Egypt as some others writing from a journalistic or diplomatic background, such as Lacouture, Stephens, Copeland, and Nutting, or others proceeding from an avowed Marxist framework, e.g., Anouar Abdel-Malek and Mahmoud Hussein.

The charisma-routinization formula, for instance, seems to have encouraged Professor Dekmejian to give inordinate attention to Nasser's ideological preachments, and to give the impression that the doctrines he enunciated, superficial as they often were, substantially reflected a determination on his part to translate his prestige into national improvements. The question here is not merely whether Nasser really thought the way he talked (Professor Dekmejian expresses healthy skepticism on this score), but whether his words were even intended to induce thought amongst his listeners. And is there any more reason to assume that Nasser sought to parlay his charisma into social change than that he sought to parlay his championing of social change into greater charisma?

Again, the significance of survey data about the elite can easily be exaggerated: the more we learn about the numbers of civilians with Ph.D.'s in high office in proportion to army officers, the more we may forget to ask what the role of each of them was in relation to the man at the top—who reputedly manipulated them all, but some with the help of others. The conspiratorial atmosphere that never disappeared from the regime and that surely stemmed not only from its origins in the 1952 coup d'état but equally from Nasser's own highly suspicious mentality, is not easily reflected by survey data nor by a systemic mode of analysis. It is mentioned (p. 191) that eight of the 131 in the elite survey had once served in military intelligence, but this statistic does not begin to indicate the importance of their role. Elsewhere (pp. 261–2, 268, 306–7) the author refers to the oppressive ways of the security police and to Nasser's "conspiratorial approach," as unfortunate by-products of military rule. Of course, this is a matter of judgment, but a good case might be made to the effect that conspiratorialism and repression were central to the very character of the regime, negating in advance whatever steps (such as the celebrated "March

30 Program" of 1968) might be taken to liberalize the political process.

To my mind the greatest gap in Professor Dekmejian's analysis has to do with the impact of Nasser's socialist programs on Egyptian society. The programs themselves are outlined in 13 brief pages of Chapter 9, "The Nasirite Theory and Practice of Arab Socialism," followed by a passage of equal length on the accompanying ideological pronouncements. But with minor exceptions the author does not follow up by telling us what difference Arab Socialism really made to the Egyptians; and this is a pity, since ultimately we must surely gauge Nasser's legacy by his success or failure in lifting Egypt's face out of the mud of poverty and backwardness. "Given the predominantly middle-class and upper-middle-class background of the leadership and its middle-class revolutionary aims," writes the author, "the potential for class conflict in Egyptian society remains real" (p. 294). If so, we need to know much more about the emerging shape of social affiliations and interests than the book provides. Professor Dekmejian would doubtless be among the first to agree that this sort of information is important, but I suspect that his adherence to the ideologically antiseptic tradition of the American science of politics has tended to inhibit him, like others, from devoting more enthusiastic attention to the dynamics of class relations.

MALCOLM H. KERR

University of California, Los Angeles

Die KPD von 1933 bis 1945. By Horst Duhnke. (Cologne: Kiepenheuer & Witsch, 1972. Pp. 605. DM 72, or \$23.00.)

Professor Duhnke's account of the Communist Party of Germany in the years of its impotence is a valuable scholarly work in several ways. As a sequel to the works of Flechtheim, Angress, and others on the KPD in the 1920s, it carries the history of the party forward to 1945. Designed on a far more ample scale than the works of Hermann Weber, this book is, first of all, an impressively complete history of the KPD in this era. As far as this reviewer can tell, Duhnke has read every German-language source relevant to his topic. The book also provides massive evidence for the process by which the remnants of the once proud movement of Rosa Luxemburg became a dependent instrument of Soviet policy.

Duhnke amply documents the emergence of the Pieck-Ulbricht leadership that was to dominate the postwar party, and he does so in a wonderfully clear exposition of very tangled circumstances. The author shows how the very

weaknesses of the KPD, both practical and intellectual, in coping with the power of Hitler, worked to the benefit within the party of those leaders most responsive to Soviet demands.

Aside from the internal history of the KPD, this work also provides valuable material for the history of the German aspects of the emergence, fitful fulfillment, and decline of the Popular Front line. While here Duhnke's account will supplement a number of extant works (Buber-Neumann, Gross, Koestler, and Edinger, among others), it gives the most complete version of the KPD's difficult relationship with other German exile groups. One might have wanted a fuller treatment of the Trotskyist challenge to the KPD, as well as details of the fate of German Communists in Stalin's purges, if only for completeness in an otherwise unstinting work.

A theme running through this volume is the growing incoherence of the Communists' analysis of the reality confronting them. This fact, to an extent not sufficiently brought out by Duhnke, disarmed the KPD in the face of Soviet leaders who, while no better at understanding, had the power to avoid (until June 1941) the consequences of their errors. From the deadly foolishness of "social fascism," through the graveyard optimism of the first months of the Third Reich, during the superficial revision of the KPD line in the Popular Front phase, and in the hopeless "analyses" of the later years by a party increasingly out of touch with German life—Duhnke shows how at every step the German Communists found no adequate tools in their theoretical armory to understand the development of German life that had deprived them of their opportunity for power. Of course, some leaders (and followers as well) took comfort in dogmatic formulas, but that only increased their helplessness. The Communists were far from alone in this predicament, but they had staked the most—in lives, ambitions, and effort—on the correctness of their analyses.

This theoretical inadequacy of the KPD was matched by its practical lack of success. As Duhnke shows, perseverance, courage, and faith in ultimate success were insufficient to prevent the Gestapo from nullifying the Communists' efforts at building a KPD-led German resistance movement. Although Communist groups continued to appear to the very end of this era—the Leipzig-based group formed around Georg Schumann in 1943–44 was one of the most vigorous and interesting (although ultimately unsuccessful)—at no time was the Hitler regime in any measurable danger from

all of this heroic activity. Indeed, if one reads this part of Duhnke's book while bearing in mind Edinger's study of the exile-SPD, one is left with the rather surprising conclusion that, even though the KPD had an acknowledged party leadership functioning in Moscow, the personal and intellectual dispersion of German Communism after 1939 was not much less than that of its Social Democratic rival.

It is a measure of Duhnke's achievement that his book raises a number of interesting and important questions in the reader's mind. One such question concerns the nature of a modern mass-party and the relationship of the leadership of such a movement to the mass of supporters. One of the Communists' more plausible claims at the time of Hitler's accession to power was that a party with millions of members and millions more supporters, which could elect eighty-one deputies to the last Reichstag despite Nazi terror, was a party which could not simply be swept away. Yet swept away it was—almost as completely as the Nazi party was after 1945. This study describes some of the tactical reasons for this decline, for example, the lack of credibility on the national question so evident in the Saar plebiscite of 1935. More broadly, this study suggests that modern mass movements owe more to power and authority than to ideological appeals or social situation.

In this context, the KPD's Leninist "organizational weapon," appears less important. It certainly enabled the party to maintain contact with small and scattered bands of loyalists, and it allowed the KPD to dominate the prisoners' organizations of many concentration camps, but it had little consequence when it could not be deployed in the political process.

Duhnke shows in detail how the KPD grappled ineffectually with the agonizing problems of patriotism in the face of a despised regime in its homeland. Ultimately, the Communists took refuge in following the Soviet line. They denounced other foes of Hitler who hoped for an Allied victory during the early phase of the war, then attacked these rivals bitterly for demanding justice for the Germans when Soviet policy favored territorial and population shifts.

The chief problem with this book is that the voluminous record is allowed to obscure these and other larger issues raised sometimes in only tantalizingly marginal ways by the author. When this German-language version of a University of California, Berkeley, dissertation finds its deserved American publication, Duhnke might do some massive pruning (say, of the long discussion of underground and Red Army

espionage activity in wartime Germany) and close the work with a discussion of the significance of KPD's twelve years in exile, rather than, as now, simply breaking off the discussion arbitrarily.

HENRY KRISCH

University of Connecticut

Political Attitudes of Indian Industry: A Case Study of the Baroda Industrial Elite. By Howard L. Erdman. (London: The Athlone Press, 1971. Pp. 62. 22 shillings.)

Recently, the drift of American social science scholarship on India has been away from the village and into the towns and cities. As usual, such research has not devoted itself to the secondary analysis of materials already collected as it might in the United States, but has involved the collection of primary descriptive data, with a greater or lesser attempt to proceed systematically with analytic concepts leading back into the discipline and with an eye on comparable work done elsewhere.

Erdman's slim monograph reports on research he carried out in the Western Indian city of Baroda in 1966-67 on the political attitudes of business leaders. It is essentially descriptive, perhaps necessarily so in the absence of solid materials ready at hand, and makes few self-conscious attempts at systematization or conceptualization. To the extent that it is oriented to other literature, it attempts to correct earlier simplistic conceptions of business leaders as totally for or against the existing government, and to represent them along a continuum. Its primary focus is on political attitudes and not activity, and a basic economic determinism is invoked in explaining the origin of those attitudes.

Baroda emerges as a secondary business center, in the shadow of, but not completely dominated by Ahmedabad. Its leaders are primarily home-grown, although somewhat cosmopolitan in experience. Its industries are spread across that limited range that characterizes Indian manufacturers. And the social structure of its leadership is familial and hierarchical, not unlike the American business class as represented in Lynd's *Middletown*. In political attitudes, the Baroda industrialists approve of government actions in making land acquisition easier for them and the provision of cheap and dependable power. They disapprove the government's support of labor's position in industrial disputes because of the populist imperatives of contesting elections; an overgrown thicket of government rules, permits, quotas and licenses; and the government's habit of publicly breathing

the fire of theoretical Marxism, while finding pragmatic, capitalist solutions to specific problems. Overall, Erdman sees the viewpoint of the industrialists as close to the conservative Swatantra Party view, on which he is, incidentally, a leading expert; and he hints that they would welcome the creation of an authoritarian government less tied to the need for securing popular approval. In view of the current move toward a left-oriented mobilization state in India today, prayers may be growing more fervent.

I have only two small disappointments to register as criticism. First, it would have been helpful if Erdman had made a more deliberate attempt to place his findings in a slightly broader context of similar studies in other countries. Second, although Erdman interviewed a substantial number of business leaders, one looks in vain for even the most general indications of the degree of consensus on particular issues and some notion of what characteristics of leaders went with what attitudes. I am not trying to suggest an uncalled-for numerical precision, but the notion that cases must be many and measurement precise before general quantitative description is useful is one of the banes of case study research.

Erdman's work is a contribution to a still small, but growing literature concerned with Indian business elites, and as such it is very valuable.

RICHARD D. LAMBERT

University of Pennsylvania

From Crisis to Crisis: Pakistan 1962-1969. By Herbert Feldman. (London: Oxford University Press, 1972. Pp. 340. \$18.75.)

The present volume is the second in a projected trilogy by an Englishman long resident in Pakistan. Herbert Feldman has been writing about the Pakistan scene for almost the lifetime of the nation, and his work has enlightened a generation of students interested in South Asian politics and government. His earlier book, *Revolution in Pakistan: A Study of the Martial Law Administration* (London: Oxford University Press, 1967), is judged by Mr. Feldman to be a prerequisite to this "sequel." I would concur with this assessment, for the author's opening chapter reads almost like a continuation of that volume. Moreover, readers are already put on notice that the third volume in this series will begin where this one leaves off. We are introduced to Yahya Khan, Ayub's successor, in a brief epilogue, thus preparing the way for the ultimate sequence of Mr. Feldman's narration of the Pakistan tragedy.

Mr. Feldman writes as a close-in observer of the daily happenings in Pakistan. He informs his readers that he has kept a detailed diary of what appear to him to be significant events. His publications, like the one under review, reflect this concern for day-to-day detail, but his analysis is somewhat thin. The author's strength, therefore, lies in his long and abiding interest in Pakistan, and for this reason his latest contribution is worthwhile reading.

If there is a theme in *Crisis to Crisis* it is the transformation of Ayub Khan from a professional soldier with a flair for administration into a power-grabbing dictator. Ayub, asserts Mr. Feldman, insisted on surrounding himself with personalities who, while assisting in the building of his personality "cult," also isolated him from the realities of his society. Although I cannot quarrel with the observation that Ayub was poorly or erroneously advised, Mr. Feldman's suggestion that Ayub was obsessed with power appears overstated. The author tends to focus on Ayub's "personality" and the conflict over the legal system, not the administrative state over which Ayub officiated but clearly did not subordinate to his will. I believe that greater attention should have been given to Ayub's dependence on the Army and higher bureaucracy. Mr. Feldman neither analyzes their more than supporting roles, nor for that matter, explains the constraints which the colonial bureaucratic legacy imposed upon Ayub. Ayub is a beneficiary of the preindependence system, hardly its master. Moreover, the perpetuation of the agrarian power structure and the traditional norms reinforcing it are overlooked in Mr. Feldman's treatment. Ayub Khan was a dominant actor, but despite repeated references to his imposing a dictatorship on Pakistan, the evidence thus far assembled seems to have been stretched to satisfy popular perceptions.

No one expects Mr. Feldman to be an apologist for Ayub. The failures of Pakistan's erstwhile President are too obvious and too gross for any objective scholar to sing his praises. Nevertheless, Mr. Feldman does appear to apologize for Ayub's actions in his *Revolution*, whereas, in this volume, he seems only too willing and ready to pillory him. Moreover, the author reads special significance into the amending of the 1962 constitution. While recognizing that this was Ayub's constitution, he seems to suggest that had Ayub not tampered with it, he would have been a far more successful governor. Notes Feldman: "... the longer-term effect of all this [amending] upon his political position was a decisive factor in his ultimate fall" (p. 23). It is difficult to believe that the

constitution would have been any more acceptable to the intelligentsia had Ayub not interfered. The constitution which the attentive public clamored for was not granted, and that which Ayub's administration imposed upon the country was condemned from the start. Furthermore, despite all the demands for a written constitution in Pakistan there is little evidence that one would be revered over another. In Pakistan, the use of arbitrary power tends to be limited more by conditions than by constitutional principles, and even that which passes for the "rule of law" survives because it is in tune with the reality of power.

Mr. Feldman writes with clarity and labors to omit little from his narrative of these crucial years in Pakistan's history. The book is divided into fifteen chapters, each of which highlights a significant period or event. In the main the author's treatment is chronological; however, he also attempts to be topical, as when he devotes separate chapters to West and East Pakistan. In the latter there is ground for criticism. Given subsequent events and the publication date of this volume, the treatment of East Pakistan must be judged skimpy, and it is not fully incorporated in the text. Twenty-two pages in a text of 299 hardly does justice to the subject. The author's treatment of Pakistan's foreign relations and especially those with India is welcome, but there are questionable passages such as the prediction of little chance for an early renewal of Pakistan-Indian hostilities after the 1965 conflict. Mr. Feldman's belief that "India is too deeply concerned with internal political troubles which threaten its integrity as a single, sovereign state" (p. 118) is falsified by the events of 1971. Perhaps it can be argued that the 1971 drama was unpredictable. Nevertheless, a more thorough examination of the East Pakistan problem and greater awareness of the east wing's relationship, even if only indirect, to the turbulence in Indian Bengal, Assam, and the tribal territories might have prompted a different conclusion.

Hence we arrive at the principal criticism of the book: Mr. Feldman could have produced a more important and timely work had he included the Yahya Khan period in *this* volume. This would have necessitated a more coherent analytical framework (and perhaps further delays in publication), but it also would have brought forth a more notable contribution. As is, the author set his sights on condemning Ayub Khan as a bumbling dictator. This determination, unfortunately, has prevented Mr. Feldman from giving his readers the larger benefits of his long and informed experience but he

has provided us with an important chronology of the decade of Ayub.

LAWRENCE ZIRING

Western Michigan University

A Guide to Parliamentary Papers, 3rd ed. By P. Ford and G. Ford. (Totowa, N. J.: Rowman and Littlefield, 1972. Pp. xiv, 87. \$8.50.)

Varied and rich as they are as sources for the analysis of policy making, British parliamentary papers in their many modes are apt to confuse the unfamiliar user. One's best guide is still a well-informed documents librarian. Lacking that, those new to these mysteries should consult the latest edition of the Fords' guided tour of parliamentary documents, appropriately subtitled "What They are; How to Find Them; How to Use Them." It updates the Fords' earlier work by including references to the changes in parliamentary structure and publication procedures made in the 1960s, and contains some new bibliographical aids. A somewhat musty work, it is nevertheless a must for reference libraries (though at the price few individuals will want or need to acquire it for themselves).

JAMES B. CHRISTOPH

Indiana University

Soviet Prison Camp Speech: A Survivor's Glossary. Compiled by Meyer Galler and Harlan E. Marquess. (Madison, Wis.: University of Wisconsin Press, 1972. Pp. 216. \$10.00.)

By now millions of Soviet citizens have had either direct or indirect exposure to prison dialect, and it appears frequently in work by dissident writers circulating in underground (*samizdat*) copies. Those interested in repression and opposition, political culture (especially the symbolic aspects of political communication), and Soviet politics in general will therefore find this book useful for several purposes. The six hundred and fifty-six items in the glossary detail physical conditions, norms, and values of prison and camp life. A useful forty-two page introduction gives background on the development of the dialect, some of its functions in the system of repression, a short content analysis of glossary terms, and a list of useful references. Compiled jointly by a former camp inmate and a scholar of Russian language, the book is limited in scope to those terms Galler encountered during his years (1942-1957) in Central Asian camps supplemented by some from the writings of Solzhenitsyn who served time almost simultaneously (1943-53) in roughly the same place.

A special vocabulary which can be adopted

by anyone speaking Russian, camp slang is described as a "... substandard variety of Russian ... 'tainted' by legal and administrative argot, obscenities, and frequently, by elements of uneducated peasant speech" (p. 15). Proverbs and sayings are most revealing of the basic concerns of prisoners and everyday conditions, particularly the morality of camp life and dealings with authorities. Profound alienation and a cynical despair is implied in such expressions as "Provided we've got the man, we can find a charge for him," and "Moscow does not heed tears." "He who can will swallow you" pertains more generally to the quality of life within the camps, and "Having nine lives, like a cat" refers to the remarkable resistance of female prisoners to camp life. Acronyms show political categorization and, therefore, help to delineate the legitimate boundaries of Soviet political culture. ZhVN, standing for "Wife of an Enemy of the People," betrays what Inkeles has termed the "virus theory of social pathology" ("The Totalitarian Mystique" in his *Social Change in Soviet Russia* [Cambridge: Harvard University Press, 1968]), providing the basic justification for the purge: once the social organism is contaminated, the absolute elimination of all carriers (the potential "enemies") is required. "Guilt by association," another common feature of the purge mentality, dictates that marriage to an "enemy" constitutes a state of contagion as well. Obscenities constitute a rather prominent feature of camp speech and are important from two points of view: For prisoners they serve as a mild form of open rebellion, while for authorities they are an instrument for downgrading and brutalizing their charges. "Call me a chamber pot but feed me" is the prisoners' pathetic expression of resignation to their impotence. A residual category of terms include specific references to the grim conditions of camp life; *výshka*, a death sentence or a watchtower; *vozhátiy*, a guard in charge of watchdogs; *podkidysh*, a "plant" or informer; and *samorúb*, one who maims himself in order to avoid hard labor and possible death from exhaustion. Still more of the grim tone of life "inside" is reflected in the examples of how words are used: for "stukách" (squealer or informer), "last night a squealer in our barracks had his throat cut."

To be sure, the facts and flavor of camp life reflected in this glossary and its long introduction can also be gleaned from fictional and autobiographical accounts; but there is special merit in the more systematic list accompanied by specific explication. Particularly interesting is the attempt to estimate the relative numerical

strength of various content categories in camp terminology. Here the most important categories seem to be: names for fellow prisoners and authorities, 23 per cent of the total; terms referring to locale of prisons, 16 per cent; survival-oriented terms, 16 per cent; expressions relating to arrest and investigation, 14 per cent; and terms of abuse and derision, 11 per cent (pp. 36-37).

As the authors note, the gallows humor which repeatedly crops up in proverbs, sayings, and terms of decision must help "to reconcile man to the painful, unacceptable, and tragic aspects of his condition" (p. 37). Particularly expressive in this regard are the mutations of official slogans which constantly haunt both prisoners and everyday Soviet citizens as well. For prisoners "Being (*bytiyé*) determines consciousness" becomes "Beating (*byt'yó*) determines consciousness" and "Socialist vigilance (*bditel'nost'*)" becomes "Socialist farting (*bzditel'nost'*)." Of course, such safety valves serve not only the prisoners but the regime as well, by slowing down the accumulation of collective frustrations which might hinder the smooth functioning of the coercive apparatus.

While Galler and Marquess make tantalizing beginnings at discussing some of the important theoretical considerations the content of their book raises, they do not carry through with much systematic speculation. This omission, however, seems to result more from their own backgrounds and the original conception of the project than from its faulty execution.

GAYLE DURHAM HOLLANDER

Hampshire College

L'An 40: La Belgique occupée. By J. Gérard-Libois and José Gotovitch. (Brussels: Centre de Recherche et d'Information Socio-politiques, 1971. Pp. 517. \$10.75, paper.)

Scholars interested in the political development of Western Europe often surrender the World War II period to military historians or memoir-writing notables. The connection between the war years, 1939-1945, and their sequel has not received much attention, possibly because of the belief that wartime disruption destroyed most patterns of political behavior. A major contribution of the lengthy and detailed study under review, *L'An 40*, is the explicit recognition that awareness of the events of World War II, specifically those of the crucial year, 1940, within Belgium, significantly extends our understanding of contemporary Belgian political structures and processes. There is no attempt to argue this case for other countries, which would at least entail a choice of different

time frames, but the book itself is a convincing example for other specialists.

L'An 40 is the most recent of the full-length studies published by the Centre de Recherche et d'Information Socio-politiques (CRISP), well known among African specialists in the United States for its exhaustive dossiers on the Congo (some published by Princeton University Press) but recognized among only a few scholars for its distinguished analysis of nearly all aspects of Belgian politics. Jules Gérard-Libois, the leading figure in the establishment of CRISP in 1958 and presently its Director-General and his younger associate, José Gotovitch, bring to the study of the German occupation of Belgium a thorough review of the relevant secondary literature, a ground-breaking examination of the pertinent German archives, and a quantity of published and solicited memoir material. The description of Belgium's reactions to occupation in 1940 reflects several of the customary theoretical concerns of CRISP such as the mechanics of decision making, leadership recruitment, the structure of public opinion, and the morphology of groups. As a result, the perspective on the German occupation sharply diverges from the few other available accounts.

There is a rich treasure of information in this volume for those fascinated by Belgium or any other small European democracy and also for the more numerous students and scholars concerned with German politics, especially Germany during the Hitler period. Viewing the German occupation from the Belgian side makes it easier to detect the army/party/government conflicts concealed by some popular writings on the Nazi period. Unlike Holland or France, Belgium was placed under a military administration and ruled indirectly, the cause of much quarreling among the untidy chains-of-command of the German hierarchy. The book is equally informative in tracing the evolution of opinion toward the occupying administration in all sectors of Belgium's pluralist polity. It is true, of course, that collaboration existed alongside the Resistance in Belgium. Nonetheless, in roughly a five-month period, especially during the harsh winter famine that occurred in the latter months of 1940, the preponderance of public opinion swung from collaborationist tendencies to general hostility, a basis for diverse forms of clandestine activity in the rapidly organizing Resistance groups. Tracing this shift, scarcely affected by the activity of the exile government in London headed by Hubert Pierlot, is one of the major achievements of the study. The discussion of "collaboration" (a term almost entirely confined to polemical usage in the United

States) fairly takes account of the Nazi effort to capitalize on the permanent tension between Flemish and Walloons in Belgium.

L'An 40 displays an admirable freedom from partisanship in the treatment of events and personalities whose memory can still evoke a full range of emotional response among Belgians. The only explicit point of view adopted by the authors is revealed in the governing hypothesis that the "occupation brought into play the same political, economic, social, and cultural forces which fashion Belgian society in normal times" (p. 9). But a crucial difference exists in observing those forces at play in 1940 and today because war and occupation exacerbate "the natural tensions in society and furnish a privileged source for socio-political observation" (p. 9). The entire book makes a carefully documented and persuasive case for this view. The immediacy and influence of the events of 1940 on present-day Belgium are largely matters for speculation. It should be recorded, however, that *L'An 40* has developed into a publishing event in Belgium. Within a year of its appearance, it had entered its fifth printing and led to a brief sequel also published by CRISP, *Commentaires et Réactions*, a collection of reviews and other communications commenting on the book.

An omission of *L'An 40*, recognized by the authors, is any extended discussion of the impact of military developments on the political situation. For many political scientists, this gap is not as disconcerting as the failure to discuss directly the consequences of the availability of and access to interview sources and correspondents. There is also an occasional confusion between the opinions of sources and the analytic interpretations by the authors. These criticisms do not weigh heavily when placed in the context of the author's three-year effort in researching and writing the book, the need to maintain the confidentiality of some interview respondents and, particularly in the case of the senior author, a remarkable knowledge of Belgian personalities, groups, and political issues. The scope of this account of occupied Belgium during the first crucial year of World War II, the excellence of the historical and political analysis in each chapter, and the opening of important research vistas contribute to a work that merits a wide readership.

HOWARD BLISS

Vassar College

Manpower for Development: Perspectives on Five Continents. By Eli Ginzberg. (New York: Praeger Publishers, 1971. Pp. 331. \$10.00.)

Eli Ginzberg, of Columbia University, is one of the central figures in manpower analysis in the United States. In this book he skillfully analyzes the economic, political, and manpower realities of his subject matter and how they interrelate. The book deals with manpower and related problems and prospects in twenty-seven countries as varied as Ethiopia and Sweden and as diverse in population as India and the Bahamas. It is based on the author's field studies since 1964 on five continents, and all except the first and the last chapters are based on his earlier publications.

The first chapter proffers as the focus for the book "the ways in which the leaders of those several countries have shaped their manpower policies with the aim of realizing their development goals" (p. 3). It then sets out as the "analytical schema" four categories of forces which determine a nation's human resources: the value system, the political structure, the economic organization, and the component parts of the manpower system itself. Unfortunately, the promise of a unifying focus is seldom attained. Ginzberg paints with a very broad brush but with quite different hues for the different countries with which he deals in any depth. The particular hue seems to reflect more often the author's impressionistic evidence than his concern for analysis under the schema he set forth. Nowhere does he draw explicitly on the work of others.

On many important matters, such as the nature and quality of Asian universities, which is the sole subject of Chapter 7, he offers very little substance on which to base his conclusions. His discussion of the universities of five countries covers a mere ten pages, of which less than two deal with India. Surely even Ginzberg's keen insight was sorely taxed to produce valid generalizations on such an important subject on the basis of his brief impressions and anecdotal experiences.

Such a work is valuable for its breadth of coverage rather than for its analytical depth. It is reassuring that the broad brush is wielded by so accomplished a painter. At times, however, his brush becomes so wide in covering broad economic and political development and prospects that the manpower content is relegated to insignificance. Such abandonment of his theme is particularly evident in Chapter 14, "The Eastern Block," wherein Ginzberg devotes twelve pages to Yugoslavia, Bulgaria, Czechoslovakia, Hungary, and Rumania, mainly in praise of market-oriented economic systems over the directed economics of those countries. What little he does with manpower in this chapter is not well done; for example, after

acknowledging that these five countries are very different, he nevertheless attempts to characterize in one short paragraph "the educational system." In spite of Ginzberg's emphasis on the critical importance of institutions, his lengthy chapter on Israel merely mentions the Histadruth, despite the fact that this "government within a government" is that country's most significant and unique institution in manpower and in many related areas.

Ginzberg develops several themes which have often been neglected in discussions of manpower and economic development, e.g., the important role of the military in training leadership and manpower, the importance of management, the neglect of women as a manpower resource, and the negative as well as the positive aspects of foreign university training and of foreign firms in manpower development and utilization.

With few exceptions he finds that the higher educational systems of countries at every level of development are producing more high-level manpower than the countries can effectively employ, and that this increases discontent and political instability. Ginzberg gives scant recognition, however, to the fact that such general overproduction often disguises persistent and critical shortages of many high-level skills, particularly administrative skills for the public and private sectors, which continue to impede development in most countries.

Ginzberg correctly advocates the development of on-the-job training by public and private organizations as essential to the development of needed skills. He properly criticizes ineffective vocational education as a general problem. But in his chapter on Venezuela he misses the opportunity to offer as a contrast that country's innovative and generally successful system of vocational education (INCE). Ginzberg's one-paragraph discussion and analysis (p. 267) is not only superficial, but it also misleads the reader about INCE's role and effectiveness.

The author repeatedly stresses the importance of rural development as an essential alternative to urban overconcentration. Although many others have discussed this problem, Ginzberg's wide coverage of countries points up the generality of such a problem regardless of countries' vastly different characteristics and levels of development.

To this reviewer the main value of the book is its knowledgeable treatment of economic, political, and manpower circumstances, institutions, problems, and prospects as inextricably interrelated. What Ginzberg's treatment lacks in depth it more than makes up for by high-

lighting those important elements in the development and utilization of manpower which are common to very diverse countries.

JOHN C. SHEARER

Oklahoma State University

Naissance d'un Etat Noir: L'évolution politique et constitutionnelle du Dahomey, de la colonisation à nos jours. By Maurice A. Glele. (Paris: R. Pichon and R. Durand-Auzias, 1969. Pp. xii, 527. Approx. \$10.00.)

Since independence in 1960, Dahomey has manifested chronic political instability under seven different presidents and four constitutions. Its highly politicized and faction-ridden army, seething with personalist ambitions, has already staged six successful coups and has become the primary destabilizing force in Dahomey today. Civil and military authority has also had to cope with increasingly radicalized assaults by primary and secondary school students and by unionists whose salaries have been eroded by austerity measures aimed at reducing civil service expenditures from the level of 75 per cent of the country's budget.

Maurice Glele's book is the first detailed study of Dahomey since Robert Cornevin's historical work in 1962. The author is a Fon from the Abomey royal family; he studied at the Sorbonne and is currently with UNESCO. During his research he has had access to many of the private archives in Dahomey and writes with the lucidity of one personally familiar with the major participants in Dahomey's long history of political strife. The narrative prudently ends with the onset of Soglo's military administration in 1965. The work is hence quite dated, for Dahomey has since had six additional civil and military regimes. The study is nevertheless a solid contribution to our knowledge of the Dahomean political context.

In his study Glele traces the political and constitutional evolution of Dahomey and pinpoints factors that have contributed to the pattern of acute political instability in this small West African state. Among these factors he notes the existence of intense vertical cleavages reinforced by historic antagonisms in the South, grievances in the less developed North regarding its exploitation by the South as a whole; the coalescence of regional allegiances around three leaders—Ahomadegbe, Maga, and Apithy—who have maintained for two decades an electoral stranglehold over their respective fiefdoms; a weak economy with exports never covering more than 45 per cent of imports; recurrent deficits necessitating austerity measures which disturbed unionists; dependence upon French subsidies, alienating radicalized stu-

dents; government mismanagement of resources; personal ambitions in conditions of acute scarcity in the country that had produced the largest number of intellectuals and administrators in French Africa; and above all else, a failure of civilian leadership to coalesce and to move in the direction of national integration and economic development. Glele does not shy away from harshly indicting the Dahomean political elite. He is understandably biased in favor of Ahomadegbe (also of Abomey), because of Ahomadegbe's greater dedication to national unity and modernization. More annoying is the excessively strong language Glele employs in driving home his conclusions, rather than letting the facts speak for themselves.

The author's searing analysis of the failure of political leadership in Dahomey also challenges the meaning and utility of elections or plebiscites in that country, which merely serve to reiterate the links between the masses and their regional political "bosses." Yet curiously, when Glele turns to prescriptive advice, his proposals are somewhat out of touch with Dahomean social reality. It is doubtful if the executive system and the nontribal parties he pleads for could survive Dahomey's traditional pattern of voting, especially since they would go against the current three-cornered division of power in the center. The stability over time of the clientelist vote was indeed attested to in 1970 when the triumvirate obtained *exactly* the same percentage of the vote in their core districts as in the 1960 elections, while Zinsou, the nontribal "modern" candidate obtained only 3 per cent of the vote. The solution to Dahomey's political problems cannot be sought in structural changes that leave untouched the underlying cultural cleavages and do not take into account the conflicting political ambitions in the center of the political vortex. Indeed, Glele's analysis itself underscores the complexities involved in establishing public and political order in this African State.

SAMUEL DECALO

New School for Social Research

The Twelve-Year Reich: A Social History of Nazi Germany. By Richard Grunberger. (New York: Holt, Rinehart & Winston, 1971. Pp. viii, 834. \$10.00.)

As we recede from direct experience of the Third Reich, and direct emotional involvement in its existence, we find the generalizations with which we explained its nature and origins less and less satisfying. Those who voted Nazi, those who joined the party, those who benefited from the regime, those who opposed it do not

fall into simple categories. On many topics the Nazis had no policy: Had not Hitler made a virtue of being unencumbered with programs? On others their proclaimed ideals turned out to be sadly irrelevant: A woman's greatest joy might be to sew, cook and wear the Honour Cross of the German Mother, but who was going to make the munitions and drive the streetcars while the men were at the front? On others still, such as the churches, the army or capital-labor relations the party was divided and confused. The party itself never grew out of the habits acquired during the "time of struggle"; it remained a loose alliance of egotistical chieftains and mutually incompatible sectional interests, whom Hitler played off against each other. The Third Reich was less a system than a chaos. This does not mean that it is unanalyzable; David Schoenbaum's *Hitler's Social Revolution* and Martin Broszat's *Der Staat Hitlers* analyze society and political institutions brilliantly without losing sight of the improvisatory incoherence of their subject. It does mean that the chronicler of the Third Reich cannot be content with mere description, in the hope that the facts will speak for themselves.

It is the chief defect of Mr. Grunberger's massive and well-researched volume that it lacks a framework. His book is the sum of its parts, and though some of the parts are good, they do not really justify the 800 pages he has written. There are plenty of worthwhile insights. He warns us not "to discount the reserves of idealism tapped by the propagation of the idea of the folk-community" (p. 51). But the combined appeals of the gospels of collective selfishness and egalitarian sacrifice had curious effects: Evacuees from bombed cities complained that possessions stolen from the Jews had been unfairly distributed, and there was always an excess of volunteers for military firing squads, whereas the U.S. Army had the utmost difficulty in getting its most famous deserter, Private Slovik, executed.

Some of the chapters dealing with individual aspects of life in the Third Reich are excellent summaries, e.g., those on agriculture, the army, and consumption. Some are misleading, e.g., those on women and family. One would not gather from this volume that, given the conditions of total war, German women were rather sheltered and cosseted, as Speer and others have pointed out. The illegitimacy rates Grunberger cites seem to prove nothing in particular. And was the crowd who urged an unwilling woman to give herself to a threatening Russian soldier (p. 43) uniquely characteristic of the Third Reich? Mr. Grunberger

should read Maupassant's *Boule de Suif*.

There is a lot of imprecise language. Why call Imperial Germany "pseudo-constitutional" (p. 2)? Its brand of limited monarchy is precisely what many continental Europeans thought of as 'constitutional,' as opposed to parliamentary or democratic government. We are told that "the Party was co-terminous with the state" (p. 130), but that "by and large the Party and the civil service remained distinct" (p. 132). And the identification of workers with the Third Reich in its last years must be ascribed to something other than their *embourgeoisement* (p. 201).

Despite its wealth of information and shrewd comment, the book shows an opportunity lost. The author has read widely, even if not always in the best sources. It is not an original book, but that does not matter. That there is a vast demand for a work that will explain Nazi Germany to the non-specialist reader was shown by the sales of William Shirer's amateurish *The Rise and Fall of the Third Reich*. A good synopsis of how the Third Reich functioned is more important than reading another fifty microfilmed security reports. But synoptic is not the word that *The Twelve-Year Reich* suggests to the reader's mind.

PETER PULZER

Christ Church, Oxford University

The Social Foundations of German Unification, 1858-1871: Struggles and Accomplishments.

By Theodore S. Hamerow. (Princeton, N.J.: Princeton University Press, 1972. Pp. 456. \$15.50, \$7.95 paper.)

This book completes the two-volume work by Theodore Hamerow, professor of history at the University of Wisconsin at Madison, on the social foundations of German unification. The work covers the period from 1858, when Prince William became regent of Prussia, to the formation of the German Empire in 1871. The first volume, subtitled *Ideas and Institutions*, appeared in 1969; it dealt systematically with the social structure, ideologies, and political forces of pre-imperial Germany. The present volume is a chronological account of the arrays of forces on various issues whose resolution led to the formation of the Empire. Drawing on biographies and memoirs, official documents and statistics, the press, and the propaganda output of parties and interest groups, Mr. Hamerow has produced a technically sound, gracefully written history.

His main point is that the political and economic order that had emerged in Germany by 1871 to last for nearly half a century was a re-

sult of a compromise between old and new capital in which the new industrial bourgeoisie accepted economic liberty without political rights and the old landed aristocracy sacrificed its corporatist ideology but retained its power and property. The immediate cost of abolishing the legal structure of a corporate economy was borne by the handicraft workers. But even before the legal basis of *laissez faire* was established in North Germany during the 1860s, industrialization had made major inroads. Mr. Hamerow shows that the law merely speeded up an inevitable process.

The same bourgeoisie that required economic freedom at home desired freedom of movement for persons, goods, and capital abroad. This was as true of the South German as of the North German bourgeoisie. In the South, however, the bourgeois proponents of German economic integration encountered strong opposition from governmental officials justifiably wary of Prussian domination. Indeed, the author contends that it was Otto von Bismarck who saw how far economic integration could be pressed, and who created a political order in which it occurred under the control of Prussia.

Readers familiar with the contemporary movement for economic and political integration in Western Europe may be struck by the similarity between the Rome treaty and recent plans for economic and monetary union on the one hand, and Bismarck's aims, first for the German Confederation (which included Austria), then for the North German Confederation (formed in 1867 by the states north of the River Main). Though Bismarck went further, advocating a common navy and a common system of civil law, he shared with the proponents of West European integration a desire for uniformity in tariffs, money and banking, the conditions of transportation and the migration of workers, and similar matters.

That the regulation of such matters fell, in the North German Confederation, to a Reichstag elected by equal manhood suffrage was due not just to the fact that the bourgeoisie stood to benefit most immediately from economic integration. It was due rather more to Bismarck's desire for democratic legitimization. Mr. Hamerow shows that, by creating a popularly elected legislature and occupying it with economic matters, Bismarck could achieve the appearance of democracy without granting a bill of rights, ministerial responsibility, or a parliamentary voice in foreign and military affairs.

Strictly comparable data are of course un-

available, but the desire for economic and political integration seems to have a broader base in Europe today than it did in Germany a century ago. Moreover, there is an active European bureaucracy in Brussels, conscious of its duty to carry out the Rome treaty. But until the integration movement acquires the force of a Bismarck backed by expanding industrial capitalism, one might question the extent to which it will make further advances, particularly in the political sphere.

Parts of the book are excellent political science. One thinks of an analysis of the elections to the constituent assembly of the North German Confederation and of several extended efforts at generalization on the social bases of political forces. This is a minor point, but I wish *Stand* (estate) had not been rendered as social class, a translation that does credit neither to the niceties of social-scientific terminology nor to the nuances of corporatist thought.

GLENN N. SCHRAM

Marquette University

Scottish Nationalism. By H. J. Hanham. (Cambridge: Harvard University Press, 1969. Pp. 250. \$5.50.)

It is perhaps unfortunate that since the publication of H. J. Hanham's book the fortunes of the Scottish National Party have waned. At the time that it was completed, the S.N.P. had made broad electoral inroads into both major parties in Scotland and had captured a parliamentary seat at Hamilton. To many observers the apparent success of a political party claiming to represent the national patrimony of Scotland seemed perfectly logical at a moment when Britain's Ulster dilemma was intensifying. Sharing this optimism concerning a rebirth of nationalist sentiment in the peripheries of the United Kingdom, Hanham traces the historical and institutional roots of the movement in Scotland. The decline in S.N.P. performance during the past several years, however, seems to have confirmed what should have been discerned at the time—namely, the assumption that there was a nationalist revival and the further assumption that the S.N.P. represented such a movement were illusions. We must now conclude that at least one of the two assumptions was mistaken.

Hanham's theme is that Scotland has "all the characteristics of a distinct nation" (p. 15) and that "a new Scottish system of government has come into being which with little modification would be capable of providing the basis for an independent Scottish administration" (p. 50).

It is apparent early on that he has overlooked the very essential distinction between "state" and "nation" and has failed to show how the presence of either one must of necessity lead to the other.

The book describes faithfully those institutions and patterns of thought that are typically Scottish and illustrates their historical development. Yet the author fails to make a convincing argument that these institutional and intellectual tracings have been converted into a popular nationalist movement. "In Europe," he asserts, "the normal basis for nationalism is the historic nation" (p. 10). That such historical determinism permits him to declare that when independence comes the Scots will be prepared to govern themselves is debatable; that it permits him to infer that Scottish nationalism has become a major political force is dubious.

The first section of the book is intended to show the historical distinctiveness of Scottish institutions. Hanham's treatment of Scottish history before the twentieth century, although perhaps overemphasizing separatist currents of thought, is nonetheless a balanced one. The author recognizes that Scotland has never been characterized by the homogeneity that some romantic nationalists have ascribed to it but rather has been plagued by recurrent fissiparous tendencies rooted in ethnicity, geography, religion, and economics. It is when he seeks to find continuity between historical nationalism and contemporary electoral politics in Scotland that he is treading on softer ground. Nowhere does the confusion of dispassionate analysis and political commitment become clearer than when in discussing the "anglicization" (or is it "modernization") of Scotland, Hanham concludes that "what most troubled thinking Scots was that the ordinary Scottish man-in-the-street . . . seemed to welcome the fact that he could now get in Scotland the advantages his southern counterparts enjoyed in England" (p. 47). It is almost as though Hanham were a Marxist confronted with the failure of the "inevitable" revolution to materialize. At such points the reader is uncertain whether Hanham is *describing* a nationalist rebirth or is *calling* for one.

Elsewhere it is equally difficult to distinguish the author's empirical analysis from his own values. He argues, for example, that "what makes the demand for Scottish control of Scottish affairs so persuasive is that since 1918 the Scottish economy has gone through a very testing period" (p. 29). We are left uncertain as to whether economic deprivation is causing a resurgence of nationalism or whether it simply

justifies such a phenomenon. One could argue after all that the Scots have been subject to economic bias from Whitehall. Since the demise of the Empire and the impoverishment of nineteenth century industries like shipbuilding and coal mining, Scotland has fallen upon lean times. The relationship, however, between Scotland's economic and political conditions is unclear.

In recent years political support for the S.N.P. has varied with the severity of unemployment and inflation. Yet, much of this variance can be accounted for in heavily Labour areas (such as the urban Clydeside) during Harold Wilson's tenure of office. This phenomenon probably represents less a direct translation of nationalist sentiment into S.N.P. votes than a protest vote against Westminster among those Labour supporters ideologically unable to cast their votes for Tories. The more consistent sources of S.N.P. membership have been in wealthy and rural pockets of the country, notably in the Highlands and Borders. This trend, unseen in aggregate voting data, indicates increasing localist sentiment directed against Edinburgh and Glasgow as well as against London. These conclusions are buttressed by the appeal which the decentralized and parochially oriented S.N.P. branch organization has had in small towns and villages and by the relatively greater electoral success enjoyed by the party in municipal and parliamentary by-elections as opposed to General Elections.

As for the normative aspect of the economic argument, Gavin McCrone has shown that Scotland would not benefit from independence and that English money has prevented the gap in wealth between England and Scotland from growing larger than it is. But even were Hanham's inferences valid, the argument that relative economic deprivation leads to nationalist sentiment is by no means uncontroversial. Indeed, as the author's description of the "anglicization" of Scotland (pp. 33-49) suggests the reverse process may historically be more accurate.

Scottish Nationalism contributes to our understanding of Scotland's past and reminds us that constitutional and national issues are not confined to Africa and Asia. The book, however, lacks the theoretical underpinning that would permit us to assess Scotland's present and future. While emphasizing Anglo-Scottish differences, the author underestimates the very concrete integrative factors binding British society. Unless the reader shares Hanham's very real attraction to the notion of an independent

Scotland, he will find it difficult to accept the historical determinism which the argument implies.

RICHARD W. MANSBACH

Rutgers University, New Brunswick

Spatial Dimensions of Development Administration. Edited by James J. Heaphey. (Durham, North Carolina: Duke University Press, in cooperation with the Comparative Administration Group of the American Society for Public Administration, 1971. Pp. xii, 276. \$8.50.)

This is the sixth in the series of volumes in which the Comparative Administration Group has reported the work of its research seminars held during the 1960s. Like all the other volumes, it is the collaborative effort of a number of senior scholars (who assembled at the University of Pittsburgh in 1965), though each person's work is clearly identified in separate chapters. Trying to head off the reader who will proceed no further because this is just "another edited volume, held together only by the binding," let me report immediately that the present work is well worth one's attention. Its weaknesses with regard to focus and continuity, though surely visible, are no greater than in many works written supposedly by a single author. I admit that since I have contributed chapters to five different "edited volumes," my credibility might well be impeached. But I am also on record in this *Review* of having thrown, if not stones, at least some small pebbles at other people's glasshouses, i.e., "edited volumes."

In addition to the introductory and concluding chapters by the editor (pp. 3-31 and 260-271), the five substantive chapters of the volume span the spatial dimensions from the small confines of Indian and Pakistani villages with their unchanging faces (by Henry C. Hart, pp. 39-90), to the megalopoli of the post-industrial age (by Bertram Gross, pp. 216-259). In between we encounter a careful recounting of the American experience seen as "development" (by Emmette Redford, pp. 91-131), the spatial aspects of Soviet administration (by Jerry Hough, pp. 132-175), and an examination of the spatial patterns of human resources movements (by John C. Shearer, pp. 176-215).

In his opening chapter, Professor Heaphey develops a series of six themes which, he claims, are touched upon in some measure by the subsequent studies (p. 30). Only two of the five deal directly with "development"; the others suggest spatial aspects of political systems

as a whole ("How the nation state must be spatially unified while allowing for spatial innovation," and "How placement of people affects development"), or of elements of the political system such as political ideology, political elites, and political culture. If anything, Heaphey is too apologetic for the work of his group. To be sure, "space" and "development" were not specifically defined anywhere in the volume, but there is really not much doubt in anybody's mind that at least "space" has been treated as a "primitive," and that the general notion of development administration is close to that of Edward Weidner, as quoted by Bertram Gross in his essay: "the term suggests the problems associated with innovational programs that upset traditional social processes" (p. 217). One wishes that Heaphey had sought out what is implicit in these studies, instead of pursuing only the explicit themes displayed by the various participants.

The five substantive chapters have characteristics and qualities which please as well as annoy; in some instances the same qualities manage to do both. The essays are attractive because they are the work of specialists who are looking at things they know well and try to ask questions about the significance of the spatial aspects of the phenomena with which they are concerned. Obviously, they do not share a common framework, and they do not subscribe to a single Kuhnian paradigm; they don't even look too closely at development administration, in some instances. But because they know well what they are examining, they speak with insight and imagination.

But this very preoccupation with their "hobbies" becomes annoying because the reader is left to puzzle out for himself what is the relevance for development administration of each author's contribution to the volume. Fortunately, at least one of them (Henry Hart) has made a serious effort to codify his findings systematically. In one other case, the essay is simply a long narrative in which the systematization promised at the outset is never delivered; unfortunately, Emmette Redford must be cited here. The other three cases fall between these two extremes. I would rate Jerry Hough's study of the Soviet experience second only to Hart's chapter on village development in the Indian subcontinent. (The larger work from which this chapter is drawn has also been published as *The Soviet Prefects: The Local Party Organs in Industrial Decision Making* [Cambridge, Massachusetts: Harvard University Press, 1969]). Hough does not present his findings nicely packaged in a series of propositions as Hart

does, but he is sensitive throughout the study to his obligation to development administration as well as to possible "lessons" to be learned from the Soviet experience by the developing world. Though John Shearer's contribution is limited entirely to Latin American data and findings, he does attempt to suggest a number of conclusions which might be examined in other parts of the developing world, for he has found that the movement of high-level human resources is from the poor to the rich countries and constitutes, in effect, a subsidy of the rich by the poor. The relevance of these findings for development administration seems obvious, but Professor Shearer has not really explored them systematically. Finally, on turning to Bertram Gross's contribution, one leaves far behind the mud of Pakistani villages and the primitive cities of provincial Russia, to follow the author to whatever mountaintop he has picked to view the "birthpangs of a world society" (p. 247). To be sure, who would quarrel with the proposition that industrial man has conquered space—if we do no more than count the astronauts we have sent to and brought back from the moon since 1965, when these essays were written. But we are entitled to ask in 1973 in what way and to what degree we have advanced one inch towards the potentialities of the new individualism Bert Gross promised us (pp. 258–259).

Given the nearly free-form quality of this volume, it is no surprise that one of its central ideas, if not *the* central idea is so effectively (though unintentionally) concealed as to be passed over easily. Henry Hart, in his painstaking and imaginative examination of the possible avenues to development for the village societies of the Indian subcontinent, comes up against the inevitable conflict between the peasant society and culture at the village level on the one hand, and the development administrators and higher governmental officials on the other hand. Noting that this problem is generally looked at from the perspective of the center, he nevertheless suggests that we reverse the procedure and take "... a perspective which puts the village in the center and the government on the periphery..." (p. 75). (For a recent work on the center-periphery issue in developing countries see Richard Stryker's essays "A Local Perspective on Development Strategy in the Ivory Coast," in *The State of the Nations: Constraints on Development in Independent Africa*, ed. Michael F. Lofchie [Berkeley: University of California Press, 1971], pp. 119–139; and "Political and Administrative Linkage in the Ivory Coast," in *Ghana and the Ivory Coast*:

Perspectives on Modernization, ed. Philip Foster and Aristide R. Zolberg [Chicago: University of Chicago Press, 1971], pp. 73-102.) Is that not what he ought to have been doing all along? It is not the automatic assumption that the developed world is at the center and the underdeveloped world at the periphery that has led to so much failure in aid activities and development administration? Have the agonies of the Soviet regime not been due to the manner in which Moscow and the CPSU have seen themselves as the center and everything else as the periphery? Would this reverse perspective not have illuminated effectively the problem of the flow of high-level human resources? Is the customary center-periphery perspective not responsible for the failure to conceptualize U.S. development as anything but a centralizing dynamic, with Washington and the big corporations as the center and everything else as the periphery?

It would indeed, have been more gratifying and more useful if the members of the Pittsburgh seminar had been able to develop more self-consciously the true central themes of their common effort. But taking their work as a whole, we are enriched by their reporting, their theoretical formulations, and their insights. Perhaps others have better ground for the judgment that "... any regime in the Third World would, alas, be better off with the paperback *Handbook of Public Administration* of the United Nations than with a cordovan-bound set of the Duke CAG papers" (Warren F. Ilchman, "Comparative Public Administration and 'Conventional Wisdom,'" *Sage Professional Papers in Comparative Politics* 01-02 [Beverly Hills, California, 1971], p. 44). I am not sure that CAG papers were ever meant to be briefing materials; but they might, in the long run, turn out to have better staying power than the U.N. handbook. They are not engaged in "selling" patented solutions; they merely suggest what might be worth having, given certain specified conditions. But it would surely have helped all of us if we had not had to wait until 1972 to see in print the work completed during the summer of 1965.

ALFRED DIAMANT

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Czechoslovakia and the Absolute Monopoly of Power: A Study of Political Power in a Communist System. By Barbara Wolfe Jancar. (New York: Praeger Publishers, 1971. Pp. 331. \$17.50.)

Area specialists often hope that their research and findings will not only refine our un-

derstanding of their particular area but also contribute to the general theory and method of comparative politics. Their hope springs from a laudable scholarly ambition and from their painful discovery that a widely accepted model they had initially intended to use did not fit a particular set of national or international circumstances. Such a mixture of frustration and hope also characterizes Barbara Wolfe Jancar's carefully researched and well-documented study of the dramatic erosion and subsequent consolidation of authoritarian socialism in Czechoslovakia in the 1960s. In examining the nature and transformations of the communist system in small countries of East Europe, Professor Jancar seems to have found the Friedrich/Brzezinski comparative framework of totalitarianism wanting. Accordingly, she suggests a new framework and a new terminology.

Since totalitarianism implies *unqualified* control, the author suggests a new concept of "absolute power monopoly" which implies only (?) "*exclusive*, rather than total, control, by virtue of which no other groups or individuals can compete with the ruling group. Depending on the situation, the ruling group may choose to terrorize or liberalize" (p. 6). The author identifies eight monopolistic tools of absolute control which their parent source, the Party, manipulates as an octopus does its branches: (1) state power (which includes the police); (2) law and legislation; (3) nationalized economy; (4) ideology and culture; (5) mass communications; (6) education; (7) mass organizations; and (8) social security and medical care. It is doubtful that this new term will gain wide acceptance, despite some merits of Jancar's newly refined analysis which rightly stresses that no rule can really be total. One objection to the new term "absolute monopoly" is simply that it is redundant (can there be a "relative monopoly?"), and that *absolute* power monopoly is actually not more, but less qualified than totalitarianism. In fact, both terms give a somewhat ironic twist to a study that describes how very relative all eight absolute power monopolies may become unless they are protected against the corrosive acid of democratic pluralism by foreign intervention. In order to frighten the Czechs and the Slovaks back into an absolute power monopoly, the Soviet Union had to use 29 divisions, 7,500 tanks, and 1,000 planes.

The author's analysis of the forces that set out to liberalize Czechoslovakian communism from within reveals that they were either naively or boldly oblivious of the Soviet secu-

rity interests in the sensitive area west of Germany. However we may deplore the fact, the Soviet leaders rightly dreaded the contagious effects of the Czechoslovak "democratism" upon other East European Communist systems as well as the ultimate possibility that, in a true democracy, not only domestic but also foreign policy may be subject to discussion and revision. In the context of Soviet worries, the fall of Alexander Dubcek seems "irresistible" indeed—as was convincingly argued by Pavel Tigrid in his admirable study *La chute irrésistible de Dubcek* (Paris: Calmann-Levy, 1969).

As a country study, Jancar's book has many merits. The author's familiarity with the Czech and Slovak scene and its changes in the 1960s is evident. Most of her data have been collected in the field. Professor Jancar also displays a commendable sensitivity to Slovak nationalism and discontent directed at Prague centralism which had favored the Czech portion of the country for a very long time; while in 1968 the Czech intellectuals called for socialism with a human face (or, in Evzen Loeb's terms, "anthropocracy"), the Slovaks emphasized socialism with a human and a Slovak face. The result was a short-lived attempt at true Czecho-Slovak federalism.

This book also has some blemishes. The chapter on Czechoslovakia's history contains several debatable generalizations: the period of semi-federal Czechoslovakia between Munich (1938) and the Nazi occupation (March 15, 1939), for instance, seems to be telescoped into one event. As is often the case in volumes presenting area studies, many Czech and Slovak names are not adequately identified. On the other hand, this study merits serious attention because of its novel analysis of the intimate connection between the ups and downs of socialist economy and the cycles of liberalization and repression. Professor Jancar usefully challenges one wishful thought born in the Khrushchev era, namely, that "goulash" or "fat" communism (that is, a successful communist consumerism) would lead to political liberalization, if not to true socialist democracy. The Czechoslovak experience indicates that political liberalization, both promoted and circumscribed by the Party, resulted from a serious economic crisis and consumer discontent.

However depressing this may sound, the study also demonstrates how modern authoritarian socialism by the 1960s had perfected its controls over individual and group behavior: mass obedience and conformity are shown to result more effectively from the party monopoly over socialized economy and the access to

jobs and privileges than from terror à la Stalin or à la Hitler. In such a context, those who want to stay in their country and *live* tend to conclude that it is indeed better "to be red rather than dead," or, in more pragmatic terms, to be superficially red rather than to be deprived for the rest of their lives of better jobs, better salaries, weekend cottages (now prefabricated), cars, TV sets, or trips to a people's democratic sea resort. The all-permeating party control over jobs and privileges—and therefore over life itself—is based on a devastatingly simple assumption that among mortal men, on the whole, heroes and martyrs have always been less numerous than *bons vivants*—especially if they have to live in the proximity of the Soviet superpower. Jancar's study of the Prague Spring and the following Russian Winter is a valuable contribution to the growing field of comparative studies of communist systems; it is also an invitation to a further search for an analytical framework which would incorporate the penetrative radiation of a regional superpower into the inner-working of adjacent national systems.

IVO D. DUCHACEK

The City College of New York

The Emergence of Black Politics in Senegal: The Struggle for Power in the Four Communes, 1900–1920. By G. Wesley Johnson, Jr. (Stanford: Stanford University Press, 1971. Pp. x, 260. \$8.75.)

With this study Mr. Johnson, professor of History at Stanford University, expands our knowledge of African politics and society in two directions. First, he adds most importantly to our understanding of pre-World War II "nationalist" movements, work comparable to that done by James Coleman in Nigeria, David Kimble in the Gold Coast, Carl Rosberg and John Nottingham in Kenya. Second, he contributes to the already impressive cross-disciplinary monograph literature on Senegal by Nigerians, Senegalese, French, and young American scholars dealing with the Islamic Brotherhoods, traditional social structure and caste, cooperative organizations, community development, pre-nineteenth century Wolof Kingdoms, Toucouleur conquerors, French assimilation policies, local government, etc.—a literature that cries out for synthesis and has added considerably to our knowledge of change in a "developing" country.

In the four "communes" of Senegal, France's oldest African holdings, Western politics first took root in the 18th century. By the time of the French Revolution, Saint Louis and Gorée

had African mayors, and in 1848 the four communes elected a deputy to the French National Assembly. By 1879 with the addition of a General Council (colonial assembly), Professor Johnson claims the four communes possessed "effective institutions of local government controlled by the urban inhabitants" (p. vii). But this system remained undemocratic for decades because the great majority of the electorate, the indigenous black Africans, was dominated by "a small clique of French and Creole politicians until 1900" (p. vii). How these urban black Africans obtained political rights before 1900 and expanded them by 1920 is the subject of Johnson's study.

After the turn of the century, a suddenly toughened colonial policy of retrenchment that sought to take away indigenous political rights at the very moment of growing African involvement, precipitated a complex struggle between colonial officials, French Colonialists, Creoles and indigenous Senegalese. Under the leadership of Blaise Diagne (the first black African elected a deputy to Paris), Africans won a series of elections replacing the French and Creole elite as arbiters of local politics. Johnson demonstrates particularly effectively how Blaise Diagne's victory represented more than an isolated or personal conquest. Diagne brought with him into office a host of Africans at the local and municipal levels. His advent into the French National Assembly marked the coming into power of a local indigenous establishment that thenceforth dominated Senegalese politics. In describing this process Johnson fills a gap left by French and African scholars. He pushes the advent of black politics back several decades prior to 1945, "the generally accepted date for the beginning of African nationalistic movements" (p. viii). After highlighting and documenting the significance of this first African political awakening in 1900-1920, the author goes on to suggest that nationalism, however, was not necessarily the key factor in Senegal's political history and that other factors, such as "assimilation, urbanism, elitism, and religion" were equally important.

Mr. Johnson does not feel himself bound by the conventional categories of political scientists and Africanists, having thought of this study as a political history, rather than as an investigation into nationalism or Senegalese resistance to the French. By thus refusing to bring predetermined conclusions into this analytic apparatus, Mr. Johnson hopes to have the facts better speak for themselves. In this effort he succeeds partially. He would judge early Senegalese leaders in terms of the times, not the

standards of the 1970s, and finds these leaders "effective, audacious and ambitious" (p. ix). Johnson's clarity in exposition is impressive—whether it be a capsule history of Senegal as essential background or a description of the "traite" (the peanut trade involving multiple middlemen and complicated credit arrangements). His scholarship rests on an admirable perusal of the records in the archives of Senegal and the French Ministry of colonies as well as painstakingly collected interviews with Senegalese personalities and their descendants.

Johnson promises a second volume on the consolidation of African politics from 1920 to 1945. Possibly as a result, one major gap in this study is a curiously incomplete portrait of Diagne (especially Diagne's speeches in the French National Assembly) and hence of politics during this period. There is almost nothing of Diagne's counterplay with Garvey or Du Bois; little about his becoming a spokesman for the Bordeaux commercial interests that, Johnson points out, ran Senegal; and only a hint of Diagne's becoming an apologist for colonialism to the extent of an incredible defense of forced labor—shocking even in the context of the times.

Johnson also does not dwell on Diagne's connections with the traditional and religious leaders. From his statement that it isn't his *purpose* to establish a direct link between traditional politics and the politics of the communes, Johnson goes on to assert that "it is doubtful that traditional politics provided an adequate training ground for urban politics: traditional political roles were ascribed, whereas those of the urban area were achieved by personal merit" (p. 7). This conclusion is not based on a factual assessment but on a logical inference using Western "ideal type" constructs. Traditional rulers of the great caste-based Western Sudanic Empires always knew how to use men of ability. If instead of hypothesizing two distinct types of society we pictured a single society with a variety of competing and interacting interests, we might be led to ask different questions and come to different conclusions. For example, Johnson mentions only briefly that the leaders of the Mouride brotherhood donated funds and made possible much of Diagne's campaign for election, and that the first people Diagne visited on arrival in Senegal were Lebou elders. In fact, was not Diagne the traditional leaders' French connection?

What Johnson describes as resistance by the urban political elite to French administrative domination and economic exploitation by Bordeaux commerce and the Creole oligarchy may

also be viewed in historical perspective as the beginning of a "new class" of black bourgeoisie. The spread of political action into the countryside may also be viewed as incorporating the elite of provincial cities and traditional notables by this black bourgeoisie—not the advent of mass politics.

In this respect Johnson's study provides essential background for an understanding of contemporary Senegal, where independence did not come as a result of actual or threatened mass struggle and where politics is still essentially an elite affair.

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The Behavioral Revolution and Communist Studies. Edited by Roger E. Kanet. (New York: The Free Press, 1971. Pp. 376. \$9.95.)

In *The Behavioral Revolution and Communist Studies* Roger Kanet has placed under a single cover thirteen papers on Communist political elites, policy implementation and bureaucracy, and Soviet foreign policy which illustrate some of the steps that area specialists on Communist systems are taking to relate their research to that of their colleagues in the empirical social sciences. These steps stress the need to make research on Communist systems genuinely comparative by adapting and applying approaches, concepts, and techniques of analysis derived from research on non-Communist systems. The articles may be grouped into two categories: quantitative analyses and middle-range theorizing.

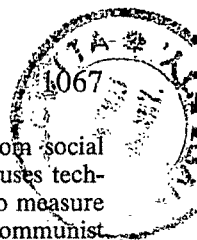
The quantitative analyses in the section on political elites include a study by Michael Gehlen and Michael McBride who have generated data on a number of background variables of members and candidate members of the CPSU Central Committee (*APSR*, 62 [1968], 1232–1247). Gehlen and McBride have constructed frequency distributions of the variables and employed a correlation matrix to determine patterns of interrelationships between pairs of variables. In the only article which employs East European data, Carl Beck, Gerard Johnson, and Thomas McKechnie examine changes in the composition of the Central Committee of the Bulgarian Communist Party at the time of the eighth Party Congress in November, 1962. They explore differences among those who were recruited, dismissed, and held over in terms of their careers within the Communist party by presenting statistics on discrete party positions and then using factor

analysis to locate systematic relationships among positions. The final quantitative piece in the section on elites is a study by Milton Lodge (*APSR*, 62 [1968], 827–839), who does content analyses of representative periodicals from 1952–1966 for five Soviet elites to determine trends in elite participatory attitudes in the post-Stalin period.

The findings of these three articles are of dubious value because of the authors' failure to deal successfully with a number of obstacles encountered when quantitative techniques are applied to the analysis of Communist systems. Because of the nature of the data available, assumptions are made which seriously distort reality (for example, Lodge's assumption of the group consciousness and solidarity of Soviet elites). Often the significance of the hypotheses tested is not dealt with adequately (for example, Gehlen and McBride's hypothesis that candidate members of the Central Committee are co-opted to provide a communications input into higher decision-making centers of the CPSU).

Another major obstacle to the application of quantitative techniques to Communist studies is pointed out in the conceptually oriented contribution of Joel Schwartz and William Keech (an expanded version of their earlier article which appeared in the *APSR*, 62 (1968), 840–851). On the basis of a case study of Khrushchev's educational reform of 1958, Schwartz and Keech conclude that "if one quantifies the number of articles which appeared during the debate, the oppositional point of view is clearly a minority. It is quite possible that a 'war of memoranda' may have been raging behind the scenes and that during this exchange the minority position was in fact the majority point of view" (p. 160).

A second conceptually oriented article in the section of elites is an attempt by Rita Mae Kelly and Frederic J. Fleron, Jr. to develop an approach to the long-standing question of the relationship between the official ideology and the actions and decisions of political leaders in Communist systems, by placing the question in the broader context of the motivational role of ideology (*Soviet Studies*, 21 [1970], 297–313). In another attempt to develop conceptual tools for the analysis of Communist systems which have cross-national significance Fleron sets up a classification of leadership systems and applies it to the Soviet Union (*Polity*, 2 [1969], 176–201). He hypothesizes that the co-optation process has been used by the political elite as an organizational response to legitimize its rule by providing a mechanism



for articulation and aggregation of various interests and by bringing capable, talented individuals into the political elite.

The three essays in the section on policy implementation and bureaucracy are attempts to adapt organizational theory, role theory, and development theory to Communist (and, in one case, Russian) studies. Each of these studies is concerned less with presenting substantive findings than it is with showing how a particular set of middle-range concepts can be adapted and applied to the study of Communist systems. Don Karl Rowney provides a research design for the study of the Ministry of Internal Affairs from 1904 to 1916 by drawing on the theoretical formulations of structural-functional analysis of formal organizations. Erik Hoffman's contribution shows how concepts developed in role theory can be used to study and understand the nature of role conflict and ambiguity in Soviet administration under Khrushchev; the regime's attempts to reduce role conflict and ambiguity under Brezhnev and Kosygin; and the way the regime has used the central press as an instrument for communicating role expectations to various audiences. Robert Sharlet focuses on the question of the role of law in political development. Using examples of Soviet economic, criminal, and family law, Sharlet demonstrates the proposition that various types of Soviet law can be viewed as performing a number of functions at different stages of development. On the basis of his overview he suggests a number of regulative hypotheses which can be used to orient research and narrow-gauge predictive hypotheses which can be operationalized and verified.

All the conceptually oriented studies lay the basis for potential contributions to Communist studies and help to bridge the gap between area specialists and political scientists by generating empirically verifiable hypotheses about the nature and operation of Communist systems derived from middle-range theory which has built upon the Western experience. An assessment of their actual contribution must await the time when the hypotheses are tested more fully against empirical data.

The final section of the book consists of three attempts to apply quantitative techniques of analysis to the study of the Soviet Union in the international arena. Charles Gati employs content analysis and correlation analysis of the foreign news items in *Pravda* and *Izvestiia* in order to ascertain "the relative weight the Soviet foreign policy elite assigns to various geographic regions or sub-systems of the world" (p. 282). P. Terrence Hopmann employs a

model of group processes derived from social psychology and communications and uses techniques of computer content analysis to measure the degree of cohesion within the Communist system during four major events of the postwar period. Finally, Dan C. Heldman presents a tentative and exploratory study of Soviet-African (developing nation) interaction with the aid of input-output models and correlation analysis. The utility of these three papers is placed in question because of their failure to generate relevant hypotheses and because of the glaring misuse of statistical analysis (for example, Gati's use of correlation analysis to determine "statistical causality" and Hopmann's measuring of intrasystem cohesion according to Communist states' perceptions of the West on the basis of content analysis of English language translations of single documents for each nation).

It is disappointing that the editor does not himself draw any conclusions about the potential contribution or obstacles in the use of behavioral approaches and techniques on the basis of the material he has chosen to include. In his introductory essay and in his introductions to each major section of the book, Professor Kanet does not indicate his criteria for selecting material, nor does he attempt to assess the utility or point to the pitfalls of the approaches and techniques used. He fails to suggest which are most appropriate in light of the data and types of political system under examination. And he fails to make any proposals about how the methods and concepts presented can be better adapted to the study of Communist political systems.

This failure of the editor to deal with his subject matter critically is partially compensated by Paul Shoup's introductory essay (*APSR*, 62 [1968], 185-204). The thrust of Shoup's argument is that there is no *a priori* reason why many comparative approaches cannot be applied successfully to Communist systems. However, on the basis of a survey of the literature on comparative Communism, he comes to two very sensible conclusions about the limits of behavioralism in Communist studies—limits which I find reflected in the contributions to this book. Regarding methodology, he suggests as a guideline for research that we focus our efforts on middle-range comparative studies at the subsystem level. Second, regarding methods of analysis, he concludes that "the field of comparative Communist studies is not yet in a position to become a quantitative discipline, in the sense that hypotheses will be routinely generated and then confirmed by meth-

ods of statistical probability, rank ordering, or other counting techniques" (p. 29).

Unfortunately, many of the contributors to this volume, especially those who have employed quantitative techniques, have not heeded Shoup's advice. One is left with the impression after reading most of the articles that very little has yet been added to our knowledge of the Soviet political system by applying "behavioral" techniques.

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Conflict in the Congo. By Thomas Kanza. (Baltimore: Penguin Books, 1972. Pp. 346. \$3.25.)

It seems scarcely credible that a mere two decades ago the author of this intriguing *témoignage* of the 1960 Zaire (then Congo) crisis created a small crisis of his own by seeking to attend a university in Belgium—the first publicized application. That such a banal event could have become a cause célèbre tells us much about why independence became a global trauma a few years later.

Thus set apart by destiny, Kanza has been at once deeply involved in the turbulent politics of his country, yet also strangely apart from them. Except for brief periods—the longest being a frustrating and unsuccessful year in 1957 following completion of his university degree at Louvain—he has lived outside his native land, in Belgium until 1960, mainly in London since that time. As a quondam ambassador of both official and dissident regimes, he has sat in the councils of world diplomacy, and came to know personally many of the key actors in global politics. Unlike most of his contemporaries, he never entered politics directly; he sought no elected office in 1960, and did not acquire a domestic power base in Zaire, although he was widely known among the new elite. His own assessment of his place in the Zaire firmament is poignant:

My fight against Belgian colonialism had been a subtle, bitter, and merciless torture. Since 1952 I had been completely alone, and understood by none of my compatriots. . . . This sense of isolation and solitude was something I continued to feel even after independence . . . I had no choice. Would I ever have? (p. 93).

The book unravels the tangled skein of Zaire's political history from the 1958 Brussels World Fair, which provided the first occasion for a large number of Zairians to visit Belgium, and the death of Patrice Lumumba in January 1961. It is a very personal account; autobiography blends with analysis. Yet it is much more

than just another *apologia pro vita sua*; Kanza was at once deeply involved, yet curiously detached. He is sufficiently Zairian to perceive the crisis of independence from within, yet far enough into the international diplomatic network to fully seize the image of the crisis as seen from overseas. He was dedicated to Lumumba, or more precisely to "Lumumbism"—the vision of Zaire as a strong, united nation totally in control of its own political, economic, and cultural destiny; yet he was painfully aware that Lumumba did not fully trust him, and gives us a candid assessment of the flawed aspects of his character, as well as what Kanza sees as his outstanding virtues.

The coals of the 1960 crisis have been so vigorously raked over by so many hands that it seems implausible that anything of note remained to be said. Not so; Kanza illuminates many events, and offers sensitive and informative vignettes of the main actors. In the fluidity of 1960, some of the personal relationships between key figures were quite pivotal—perhaps none more so than the incompatibility of Lumumba and Dag Hammarskjöld, which destroyed both men.

Kanza offers valuable detail on the tortuous dealings leading to the establishment of the Lumumba government in June 1960. To cite but one example, Lumumba's simultaneous promises of support for the presidency to Joseph Kasavubu and Jean Bolikango are elaborated in revealing fashion. Reneging on the Bolikango commitment created a bitter and costly enemy for Lumumba.

Lumumba emerges as a man wholly obsessed with the struggle against imperialism, affected by a prescient vision that he might not last long, failing to appreciate the fragility of his jerry-built government. Kanza captures the essence of the drama of independence in a single sentence: "Though we sat so comfortably in our sumptuous official cars, driven by uniformed military chauffeurs, and looked as though we were ruling this large and beautiful country, we were in fact ruling nothing and a prey to whatever might happen" (p. 151).

For this very reason, international participants soon assumed a key role; Kanza is deft at identifying the external linkages of many leading figures. Communist cultivation of Zaire nationalists, he argues, was less intense than was generally believed. In Kanza's words, the Soviets sought out a few whose nationalism was anticapitalist and above all anti-American.

But when it came to financial aid, they were not very generous; they were quite willing to abandon financial corruption for the corruption of minds.

The few Congolese in whom they were interested were paid; but the payments were chicken feed, except to one or two—and these only got more as a result of fierce bargaining. The Belgian communist party acted as a liaison between the communist world and the Congolese nationalists; this was a grave mistake on our part, for the Belgian communist party was larded with double agents . . . (p. 141).

There are, of course, debatable points and noteworthy omissions in Kanza's account. For perhaps understandable reasons, he is careful to avoid critical commentary on Mobutu's role in the 1960 crisis. The animosity between the entire Kanza family and Kasavubu is a well-known theme of Zairian politics; this animus is apparent in the very negative assessment of the first president. Kanza clearly exaggerates the capacity of the national army to have carried through its abortive invasion of Katanga in August 1960; he is inexplicably silent on the atrocious massacres of Baluba civil populations when this disorderly force was unleashed in hostile territory, an event which Hammarskjöld emotionally stigmatized as "genocide," and one of the several immediate precipitants of the September coup. And Kanza's own analysis of Lumumba's increasingly erratic and impulsive responses to a deteriorating situation in August 1960, demonstrates why the author's efforts at the time to persuade Western powers and the UN Secretariat to have confidence in Lumumba's leadership were doomed to failure.

Just possibly the course of history in Zaire might have been different had Lumumba taken greater advantage of the wisdom, prudence, and insight of this faithful follower, who fully shared his vision of the future. As it is, Kanza adds an important contribution to the historical record of the crisis of independence in Zaire.

M. CRAWFORD YOUNG

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The Emancipation of the Polish Peasantry. By Stefan Kieniewicz. (Chicago: University of Chicago Press, 1969. Pp. 285. \$11.75.)

Here one of Poland's most distinguished historians of the nineteenth century in the Polish lands has undertaken a narrative account of one of the century's most critical issues. The book is an elaboration of lectures Kieniewicz delivered at Chicago in 1968, written in response to the intelligent encouragement of his colleagues and the university press. It constitutes the most adequate treatment of this particular complex of peasant emancipation processes now available in any language. As such it could serve almost as an advertisement for

the value of East-West cultural exchange.

Because their task is to construct narrative, historians face special difficulties, even when they deal with single issues. The history of these lands renders the difficulties all but insuperable. Northeast Central Europe was and is a region of great ethnic diversity, and between 1795 and 1918 these peoples, at disparate levels of social and economic development and national consciousness, were under the disparate sovereignties of Romanov, Hohenzollern, and Habsburg. The subject matter is thus exceedingly complex.

Kieniewicz has sought to do scrupulous justice both to this complexity and to the inadequacies of the very substantial data on peasant emancipation developed over the past generation by Polish research. There is synthetic treatment of the beginning (the Enlightenment period in Old Poland), the progress of agriculture and democratic propaganda (in the first half of the nineteenth century), and the end (the agrarian problem which faced New Poland in 1918), and there are comparisons throughout. Nonetheless, most chapters deal only with one sovereignty. In Old Poland, despite considerable variety in situation according to quality of soil, distance from markets, and even legal status, most peasants had been personally unfree and tenants at will. In general, emancipation began with the grant of personal freedom, continued with the strengthening of tenancy rights, and ended with the grant of freehold. But this process took different forms, had different timing, and followed different political imperatives in each sovereignty. At each stage, in other words, the particular mix of pressure from above (reforming landowners and harassed governments) and pressure from below (actual or potential peasant revolts) was unique.

The results were also unique. At the conclusion of reforms marked at start and finish by revolts, almost all peasants in Austrian Poland had land, but too little, and peasant life there was characterized by poverty, parceling, wild land speculation, and emigration. The more gradual Prussian reform created a substantial yeomanry alongside the large estates, at the cost of driving many peasants off the land into industry. The Russian reform, also gradual but punctuated more by upheaval like the Austrian, kept the peasantry on the land like the Austrian but in better conditions, since the state supervised land parceling and had less interest than the Austrian state in conciliating Polish nobles. Further distinctions among the three partition zones involved state and private estates, forest and common field rights, landholders and landless, compensation arrangements, rights to dis-

till and sell alcohol, local and central political prerogatives, and scope for peasant political action. The impression is almost inescapable that the historic task of the New Poland established in 1918 has been nation building.

Conceptually, there are only a few ways of imposing unity of vision on so much diversity. Polish historiography has traditionally concentrated on the struggle to recover state sovereignty. Kieniewicz has chosen to focus his narrative elsewhere, on peasant emancipation. Even here, however, one must choose.

The most comparable English-language work, Jerome Blum's *Noble Landowners and Agriculture in Austria 1815-1848* (Baltimore, 1948), managed the problem of political multiplicity by dealing with only one monarchy and the problem of ethnic diversity by framing the analysis in terms of class struggle, seen monarchy-wide. Blum concluded that "the landowners" had espoused emancipation for efficiency and profit and for fear of peasant revolt, but on terms advantageous to them, and that this "program" had been adopted in 1848. Postwar Polish research has analyzed new mountains of data and emphasized pressure from below, but it has commonly achieved narrative coherence by the same method: omitting non-Polish populations, engaging in class struggle, and confining analysis to one partition.

Kieniewicz has adopted some of these practices, notably by defining away non-Poles in his terms of reference and by extensive use of the class struggle concept. But because he wishes to describe the emancipation of Polish peasants everywhere, he must find a concept which comprehends them all, whatever the actual disparities in experience, situation, and national consciousness. This can only be the concept of potential Polish nationality. And in fact the theme of the book is that peasant emancipation was the necessary precursor of national liberation because it was the essential precondition to the growth of Polish national consciousness among the peasant majority. In this way the concept of Polish nationality is at once the cause of this narrative's unevenness and the cornerstone of its unity, and Kieniewicz rejoins the mainstream of Polish historiography.

The book will be of interest not only to students of Polish history in general but to political scientists concerned with developing societies, for the relation of social change and national consciousness in backward (and nonself-governing) polities is as topical in most of the world today as it was in East Europe a century ago.

THOMAS W. SIMONS, JR.

Department of State

Communism in Japan: A Case of Political Naturalization. By Paul F. Langer. Hoover Institution Studies 30. (Stanford: Hoover Institution Press, 1972. Pp. 112. \$5.95.)

Hadaka no Nihon Kyōsantō [The Japanese Communist Party Bared]. By Kōtarō Tawara. (Tokyo: Nisshin Hōdō Shuppanbu, 1972. Pp. 330. ¥550.)

The survival and growth of an organization depends both on the nature of its environment and on its own ability to adapt creatively to that environment. This is the implicit thesis of a concise, though broadly focused, study of the Japanese Communist Party (JCP) by Paul Langer, one of the foremost specialists on the subject, based at the RAND Corporation. Now in its fiftieth year, the party came into existence as an illegal organization because of the lack of freedom in prewar Japan and was only liberated from that status by Japan's defeat in 1945. In the mid-1920s, however, it did play a more important role in the labor, agrarian, and proletarian party movements than Langer gives it credit for. But by 1935 severe state repression and rising ultranationalism had almost completely obliterated it. It revived and grew rapidly during the first five years of the American occupation until the Korean War, when the occupation officials severely crippled it, and it responded with some attempt at guerrilla tactics. Since 1955 it has gradually readapted itself to the parliamentary path to power internally and to independence from external interference by the powerful ruling parties of the Soviet Union and China. This process of "naturalization" (p. 97), according to Langer, has assured it a place as "part of the Japanese political scene" (p. 36).

That the JCP no longer poses a "security" but rather a "political" question (Tawara, p. 2) is the basis for the longer, more journalistic but nonetheless competent book by Tawara Kōtarō, a free lance commentator and former editorial writer for the *Sankei* newspaper. The first of the four parts into which Tawara's book is divided attempts an "objective" survey of the party at present; the second, a more subjective evaluation of the prospects of the party's goal of a "democratic coalition government" (seen as slim for the next decade or two); the third, a detailed account of the JCP's relations with the Chinese, and, by way of contrast, the Russian Communists (decisively bound up with the rise of the present JCP Secretary General Miyamoto Kenji, who had not been abroad or beholden to them before the war); and the last, an analysis of Miyamoto's extraordinary control over the party apparatus through mobilizing older factions and through expulsions (the

most interesting section). Tawara's book is less integrated than Langer's, which moves logically from pre- to postwar, and from the party's national to its international environment. Also, Tawara's volume does not contain even the minuscule bibliography and chronology, or the name index and minimum footnotes that Langer's does, though it has an ample and convenient table of contents.

Tawara's book does, however, provide an interesting supplement to Langer's for those who read Japanese. Not only does Tawara include more detailed and slightly newer material (to March 1972) but also he sheds light on perhaps the one important weakness in Langer's analysis, namely, Langer's neglect of why, except for his vague "naturalization" thesis, the JCP is today the "largest non-ruling Communist party outside Western Europe" (Langer, p. 16) with a membership of some 300,000—far outstripping any other opposition party in Japan. The Buddhist-inspired Kōmeitō has less than half the membership of the JCP, and the Socialists (JSP) and the Democratic Socialists (DSP) each have only about one-tenth. Of course, election votes are not proportional to party size. Tawara attributes the large JCP membership mostly to the managerial abilities of Miyamoto who does not run for the Diet but concentrates on party building. Through a cluster of "new young bureaucrats," he directs a vast organization and publishing operation. The Sunday *Akahata* has reached mass proportions (more than one and one-half million readers). Another reason the party has grown is that it provides an active party life and creates a "community" (Tawara, pp. 205 ff.) for the people left behind in the economic dash or alienated by the emphasis on "materialistic" values as well as by the waste, pollution, and indifference of the "affluent society." Consequently, outside of the violence-prone radical "New Left," the JCP retains the greatest appeal for youth as seen in the composition of party membership (Langer, p. 28). It also successfully appeals to women, whose voting rate is higher than men's.

From reading neither of the books, however, would one anticipate the remarkable JCP achievements of the December 10, 1972, House of Representatives elections in which the party executed a "great leap forward" from 14 to 38 (plus 2 pro-JCP) seats and capturing 11 per cent of the vote. This catapulted it over the Kōmeitō and DSP to become the second largest opposition in the Lower House. Both authors agree, however, that the independence from the Chinese and Soviet parties is an aid to wider acceptance in Japan. They also conclude that

the JCP has an edge over the JSP in appealing to Japanese nationalism in that the JCP designates the main enemy of the Japanese people as the *foreign* "imperialism" of the United States, whereas the JSP considers the villain as *domestic*, Japan's own "monopoly capitalism." Langer appears more sanguine about the conservative government's ability to solve internal social and economic questions (p. 95). Both consider the leadership skills of the other opposition parties extremely important in determining the JCP's future growth, since at present it appears that it would be mainly at their expense (Tawara, p. 166; Langer, p. 97). The JCP's great hope, then, in the next two decades actually lies in its becoming the main spokesman for the opposition to the conservative ruling party or coalition. Tawara convincingly argues that this will be an increasingly responsible opposition, however, operating within the context of the democratic system and yet mobilizing the people at the grass-roots level (Tawara, pp. 324–27). Despite its lack of sufficient explanation for JCP successes, Langer's well-rounded survey will be welcomed by the comparative government specialists for personal updating and for classroom use. The area specialists with Japanese language competence will find that Tawara's popularized account complements Langer's in providing background color, personalized sketches, and more speculation on the Japanese Communists' future.

GEORGE O. TOTTEN, III

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Social Conservatism and the Middle Classes in Germany, 1914–1933. By Herman Lebovics. Princeton: Princeton University Press, 1969. Pp. 248. \$8.50.)

The fourteen years of the Weimar Republic exposed the Germans and in particular their middle class to three severe social, economic and psychological adjustments: first, the military defeat after four years of illusion about a victorious peace; second the "rationalization" of the economy plus the experience of inflation; and finally, the collapse of the political system which destroyed the emotional and social security the non-socialist groups had enjoyed under the monarchy. This, coupled with the impact of the anti-western trend of the war propaganda, led among the bourgeois intelligentsia to a general anti-democratic and pseudo-philosophical grouping, generally known under the label of Conservative Revolution or National Bolshevism.

Many scholarly books have been written on the groups that identified themselves with this trend, including *Linke Leute von Rechts* by

Otto-Ernst Schüddekopf, *Antidemokratisches Denken in der Weimarer Republik* by Kurt Sontheimer, *Germany's New Conservatism* by Klemens von Klemperer, *Oswald Spengler* by H. Stuart Hughes, and one wonders why they did not receive more credit in this volume. Granted that Lebovics purposely restricted his topic to the problems of the middle class and the economic and sociopolitical views propounded by Werner Sombart, Edgar Salin, Othmar Spann, Oswald Spengler, Ernst Niekisch, and Ferdinand Fried. Since their ideas, however overlapped with the other psychological problems mentioned above, one cannot really separate them from each other, and Lebovics, in fact, does not do this either. When discussing the ideology of those men, he is going far beyond his stated topic, the plight of the middle class and the suggested solutions to their problems.

Werner Sombart, called "the Red professor" in his youth, was converted by World War I and then discovered what he called "German Socialism" in contrast to the proletarian socialism of Karl Marx. Spengler saw salvation in "Prussian socialism," defining it as a return to the barracks of Prussia and good Prussian virtues such as "authoritarianism," "obedience," "discipline" and "self-sacrifice." Edgar Salin is less easy to identify because, under the spell of the poet Stefan George, he tried nothing less than to attempt to institutionalize George's "the beautiful life." The Viennese Othmar Spann possibly came the closest to reality with his (not very original) concept of "corporatism," as opposed to parliamentarism, but being a professor, he also tried to develop a theory which entailed what Lebovics wittily called "*packln* turned into metaphysics," *packln*—an Austrian term—being loosely translated by Lebovics as "wheeling and dealing." Ernst Niekisch looked toward "National Bolshevism" and "the East" for salvation, being full of contempt for Stresemann's efforts to smooth things over at Locarno. Finally, Ferdinand Fried and the *Tatkreis* tried to create a "Third Front," which would offer an alternative to democratic socialism on the one hand, and the vulgar nationalism of the Nazis on the other.

There appears little need to further discuss their ideologies. What is striking about this group is that they included many gifted men, and this may explain the fascination this topic has, and why it keeps attracting young scholars again and again, though one might hope that this chapter can now be closed. Dependencies

with regard to National Socialism is not a worthwhile approach to the problem of explaining the latter. I also believe that one does those "thinkers" and their literature too much honor to put so much solid and profound scholarship into the effort to make sense out of something that was partially, at least, nonsensical. In connection with this, one wonders therefore, why Zehrer, the editor of *Die Tat*, and Fried were "compelled" to leave the university (p. 182). Was it purely for economic reasons, or were they rebels who felt no need to subject themselves to the rigid discipline and the hardships of academic life because of their "intellectual superiority" over university professors? Having personally known some of them, I feel inclined to suspect the latter. There was a great deal of arrogance surrounding this group, who were neither able to adjust to life and its normal requirements nor to ordinary language, creating their own terms and giving them an exclusive "German" meaning. There was genuine idealism in them, but on the whole they were "aristocrats of the mind," "elitists" as well as political romanticists and thus very far removed from the mentality and the life of the middle classes and their immediate needs. This dichotomy between theory and practice is also evident in Lebovics's volume and the book falls somewhat apart between the introductory and concluding chapters, in which he discusses the plight of the middle classes, and the heart of the book in which he is analyzing the various theories. The question here arises: Who were the people who read the books and pamphlets of these men? Certainly *not* the white collar workers, the shopkeepers and the peasants who constituted the middle class in Weimar Germany. One may assume therefore that they wrote for their own self satisfaction and that they were their best subscribers to their "new" ideology.

Lebovics's book is well written, and one is grateful that he has condensed the basic theories of these people, while at the same time providing enough quotations to give the reader an accurate impression of them. I do not agree, however, with his emphasis on the "failures" of the Weimar politicians which have so often been repeated (and which certainly did exist) while neglecting the problems which the politicians had to deal with; above all, by neglecting the antidemocratic trend of that period, Lebovics unconsciously appears to prove correct those who called for a "savior" or great man. But in a democracy, one has to deal with ordinary people, and not with charismatic

leaders; in this sense, a leader such as Stresemann achieved much, as even Hitler later admitted. To Niekisch, however, this was not enough. For him, "humanity" and "internationalism" were embraced only by those Germans who lacked true "Germanness." In his view, a true German nationalist would "smash the whole world to pieces rather than be guilty of an act of disloyalty to his own German self" (p. 150), whatever this was supposed to mean.

Though it is considered to be the historian's task mainly to understand and to analyze, I feel that the people Mr. Lebovics is dealing with require more criticism than understanding because they belong to that category of academicians of whom Hans Heigert says "that for more than 150 years they failed to do what would have been their profession to do: to think rationally and logically, use human reason as judgment, and look with critical reason towards the future."

ANNELISE THIMME

University of Alberta

The Politics of Culture. Edited by Antonín J. Liehm. Translated by Peter Kussi, with an introduction by Jean-Paul Sartre. (New York: Grove Press, Inc., 1972. Pp. 412. \$10.00.)

With the benefit of hindsight, Liehm's anthology of interviews with Czech writers, during 1967 and 1968, appears in a different perspective, as if the "most cruel goddess of history" (Engels) would like to use its irony not only on the silenced Czech intellectuals but on the two comments by Liehm and Sartre as well. Further, a very different and more balanced view of Czech culture might have been presented with another selection of authors. *The Politics of Culture* presents 15 names of reformist Communist writers around the journal *Literární listy* (Laco Novomeský, Milan Kundera, Ludvík Vaculík, Jaroslav Putík, Dominik Tatarka, Eduard Goldstücker, Lumír Čivrný, Peter Karvaš, Ivan Klíma and Karel Kosík), with only four non-Communists (Jiří Mucha, Ester Krumbachová, Josef Škvorecký and Václav Havel). A second unwritten volume of *Politics of Culture* would have to include the equally or more important names of 11 ignored non-Communist intellectuals (such as Jaroslav Seifert, Jan Werich, Václav Černý, Vratislav Effenberger, Jiří Suchý, Jiří Kolář, Josef Hiršal, Valdimír Holan, Jindřich Chaloupecký, Jiří Němec and Jan Grossman) to which it would be a pleasure to add four orthodox Communists (for example Jan Drda, Ladislav Stoll,

Jan Procházka and Pavel Kohout) to show the real abysses between the two decisive orientations which Liehm tends to overlook. However, the anthology belongs among the most important source books about the Czech Spring '68, precisely because it reveals the authentic origins of the Czech catastrophe.

The Czech culture of the 'sixties, as reflected in Liehm's book, can be regarded as an integral part of the persistent, increasingly vigorous, and open protest against the repressive regimentation of art—a development which was inaugurated by a philosophical discussion (1956), by the so-called "poetry of everyday life," and later by J. Škvorecký's *The Cowards* and new literary criticism. It continued with the rebellion of abstractivists in the visual arts, in the experimental theatres, music and film, and culminated in the new novel, in television commentaries, and in an open revolt of the intellectuals against the cultural policy of the Communist party. The Czech culture of the 'sixties brushed aside the original antagonism between dogmatism and revisionism, and in its best works was represented by the non-Communists. A continuous flow of open protests increasingly involved broader strata of the general public. While the artists' associations as the transmission belts of the Central Committee of the Communist Party of Czechoslovakia were the principal centers of party policy, the artists as a social layer of the intelligentsia could no longer be tamed merely by the advantages ensuing from membership in the Association, trips abroad, and so-called literary loans as the transmission levers of corruption. The intellectuals were ready for a revolt.

When the revolt really came (1968), the fundamental differences within the Czech culture could not be openly stated, because they would have undermined the common course of action against the party control. The group around *Literární listy* (with Liehm as a leading spirit) and its exponents in the Association of Writers completely dominated the leadership of democratization campaigns in the area of cultural policy. Although most of them were reformist Communists, they suddenly, de facto and overnight, became the representatives of that non-Communist trend which in a small-scale cultural guerrilla war had defended the fundamental humanistic values and cultural freedom for more than ten years. This was a bizarre turn indeed. It may appear puzzling that it was the Communist intellectuals who became the foremost reformers without feeling the discrepancy between their former and

present attitudes, without questioning their own class consciousness, without realizing the transformation of their roles and the differences in the principles employed in 1948 and in 1968.

What conclusions can be drawn from Liehm's book?

(1) With due respect and occasional admiration for the works of Milan Kundera, Antonín Liehm, Jaroslav Putík, Ludvík Vaculík and other literary representatives of reformist communism, I believe that the Czech literature of the 'sixties did not reflect the idea of reformist communism, but a democratic opposition to the totalitarian dictatorship by independent groups. The true intellectual leaders—not mentioned by Liehm—were members of the non-Communist intelligentsia and were the most powerful personalities: Vratislav Effenberger in literary criticism; Václav Havel and Jan Werich in drama; Jiří Kolář, Jaroslav Seifert, Vladimír Holan in poetry; and Josef Škvorecký in the novel. Independent intellectuals (not the temporarily reforming Communists) made the Czech culture great in the 'sixties by promoting that organic growth of values which a totalitarian dictatorship always and everywhere hinders, no matter whether it happens to be momentarily conciliatory or aggressive.

(2) The cultural policy of "socialism with a human face" was romanticism of the Communist intellectuals which was incompatible with the real world of tank divisions. The employment of tank divisions against cultural works is not rational, but it is effective. Forms of political behavior must always be adequate to the subjects over whom the political influence is to be exercised. The Czechoslovak Communist intellectuals failed to give guidance at the decisive moment, and no matter how we appreciate their art production, the Prague Spring means their defeat. It also means that the tremendously growing social weight of the non-Communist intelligentsia and of the middle classes might give birth to political leaders in the future. Will they master the technology of politics at least to such an extent that they will not lose the decisive battle next time?

(3) Romanticism in politics is a sure way to defeat. Hölderlin, a romantic poet of the last century, once yearned for the banner under which he could shed his blood honorably, even in a hopeless battle like Thermopylae: "Ah, if I only had a banner . . . and Thermopylae where I could shed my blood honorably. . . ." The reformist communists from the literary clubs hoisted the banner, and the Soviet tanks pro-

vided Thermopylae. The blood that was shed, however, was not the blood of the reformists. Who are the romantics who are not willing to shed *their own* blood for their own illusions?

IVAN SVITÁK

California State University, Chico

Agrarian Reform and Peasant Revolution in Spain. Origins of the Civil War. By Edward E. Malefakis. (New Haven: Yale University Press, 1970. Pp. xxx, 469. \$15.00.)

Professor Malefakis's monograph, which won the American Historical Association's Adams Prize for the best book in European history, is essentially concerned with the reasons for the complete failure of the agrarian reform of the Second Spanish Republic, a failure which the author seeks primarily in politics. The book's first four chapters are dedicated to a detailed study of land tenure and social structure in rural Spain, especially in the southern latifundia regions. In the following ten chapters, the author studies the debates and political maneuvers which led to the passage of the agrarian reform bill, analyzes the legislation which was approved, and discusses the subsequent developments in the countryside and in the Cortes which prevented its implementation. In view of the ground-breaking character of this book, which is the first serious study of one of the most crucial problems in the modern history of Spain, it may be well to summarize some of its most important findings.

The author's introductory study of rural society lays to rest many venerable myths. He demonstrates, for example, that the existence of latifundia in Andalusia cannot be explained in purely geographic or economic terms but must be traced to historical patterns originating in the reconquest and resettlement of the land in the Middle Ages. Similarly he tells us that by the 1930s the Church owned less than one per cent of the land area of Spain and that noble holdings accounted for only about eight per cent of the cultivated land in the six key latifundia provinces and no more than six per cent in the country as a whole. This implied that, contrary to the belief of many contemporaries, no significant reform could be carried out by merely seizing the lands of the Church and the nobility without attacking the interests of other social groups. The author is to be especially commended for the combination of energy, ingenuity, and judgment which has enabled him to overcome the notorious deficiencies of Spanish statistical material and present in the early chapters a detailed, accurate picture without overstating the strength of his conclusions. This

part, which the author modestly describes as introductory, constitutes in itself a major contribution to our knowledge.

In the second part, the author presents an account of the Second Republic's efforts at land reform. The government formed by the Republican Action leader Manuel Azaña was supported by the Socialists and by a coalition of four Left Republican parties. Malefakis points out that Azaña and his supporters outside the Socialist party were essentially urban-oriented, with little understanding of or commitment to agrarian reform. A tiny Agrarian minority in the Cortes was consequently able to prevent the passage of any significant legislation for over a year, until the abortive Sanjurjo coup in August, 1932, finally galvanized the majority into action. The legislation which was approved in September, 1932 was poorly conceived. It unnecessarily affected many medium landowners, the author charges, largely because of the Socialists' insistence on extending it to the entire country rather than limiting its application to the latifundia provinces. Once the legislation was passed, the government lapsed again into inactivity. During the first year only 45,000 hectares changed hands to the benefit of a mere 6,000 or 7,000 peasants.

In September, 1933, when the breakup of the Left Republican-Socialist alliance led to new elections, the Right and Center emerged much strengthened. The Catholic CEDA party alone returned 110 deputies, more than the combined total of the Left, while the Radicals won close to 100 seats. The Center-Right governments which followed Azaña were even less committed than their predecessor to serious agrarian reform, and it is commonplace to charge them with the responsibility for its failure. Malefakis's account of their record certainly does not rehabilitate them, but it does show some patches of light—particularly the efforts of the CEDA Minister of Agriculture Jiménez Fernández—which relieve the panorama of black reaction.

More important, the author argues that the prospect of peaceful change in the Spanish countryside had already been lost by the time Azaña fell. During the first two years of the Republic the Socialists had followed a policy of moderation. Their progressive radicalization under the leadership of Largo Caballero was due, he tells us, to Azaña's failure to offer them concrete achievements in the area of agrarian reform. It meant, he believes, that an essential condition for nonrevolutionary agrarian reform had been lost, even if Azaña had retained power, and explains that "what might have

been the birth pangs of a new social order proved instead part of the death throes of the Republic" (p. 316).

The chief theoretical question raised by this book is the possibility of peaceful democratic agrarian reform. Although Malefakis does not discuss the point at any length, it seems clear to the reviewer that an *economically* successful reform that would have raised the standard of living significantly could not have been carried out in Spain by peaceful nor by revolutionary means until the country had achieved a higher degree of industrialization. As Stanley Payne has noted, Spain lacked in the 1930s the technical and financial resources necessary to improve greatly the lot of the peasants. (*World Politics*, 25 [October, 1972], p. 165).

On the *political* level, which is the main focus of the author's concern, the abundant evidence he presents suggests to this reviewer that the prospects for success were negligible. On this score there seems to be a certain unresolved tension in Malefakis's mind. The introduction and conclusions make it clear he is aware that the peasants' demands "fundamentally threatened the politically powerful large owners" (p. 6). The fact that the stakes for both landholders and peasants were too great to allow them to wait patiently suggests to him that "the Western model of peaceful, constitutional change is not feasible for large-scale land re-distribution" (p. 395), and that "the Republic would probably have failed even under the best leadership because the circumstances under which it functioned were so unpropitious and the problems it faced so complex" (p. 339).

Throughout the text, however, he lays stress on delays in drafting and applying the reform legislation (e.g., p. 249, 268, 283, 397) and on the tactical and personal failings of the Republican leaders (e.g., p. 252-253). This stress on the incidents of the political struggle tends to suggest to the reader that the author is convinced that had the persons who made up the government and the attitudes that influenced them been different, "the agrarian reform might have prospered and the progressive Republican regime might have established itself as the enduring form of government in Spain" (p. 399).

The complexity of this problem of counterfactual history suggests the need for further study of the conditions under which peaceful democratic land reform can take place, but in no way detracts from the merits of this excellent book. It is the best monograph on Spain in the 1930s and represents a highly significant

contribution to the literature on agrarian reform.

JOHN F. COVERDALE

Princeton University

Count Witte and the Tsarist Government in the 1905 Revolution. By Howard D. Mehlinger and John M. Thompson. (Indiana University Press, 1972. Pp. xiv, 434. \$17.50.)

Unlike the Soviet regime, the reign of the last tsar has been the subject of few first-rate analyses by Western scholars probing into the staggering tasks confronting the government. Until the present work we have not even had a study of the crisis of 1905-06 from the perspective of the man whom Nicholas II reluctantly chose to stave off a genuine revolution, Sergei Witte. Sponsor of the October Manifesto and Prime Minister of sorts for six months after its promulgation, he guided the tsarist regime from its threatened collapse at the time of the General Strike to its consolidation in the spring of 1906 under a new set of institutions and policies destined to see it through another decade. Dismissed before the meeting of the First Duma, he bequeathed the subsequent fulfillment—and the glory—of his work to Stolypin, a simpler and more likable personality, but obviously of lesser caliber.

This admirably thorough and well-documented study, illuminated occasionally by a cautious pursuit of might-have-beens, greatly advances our knowledge of the events in these months and of the basic issues before the tsarist regime: public participation in the governance of Russia, the form of its constitution, the conduct of elections, the rights of citizens; the peasant question; the loyalty of the army and the preservation of order; the financing of the state, and Russia's financial dependence on foreign countries. It also provides new insight into the crushing quandaries facing a statesman of unusual scope bent upon broadening the political base of the government while being forced to restore law and order in a vast, largely illiterate polity, rent by century-old tensions, and polarized beyond repair. Because of its realism, this study for the first time permits sound comparison between the pseudorevolution of 1905 and the real one of 1917, between the six months of the Witte regime and the eight months of the Provisional Government. In its largest perspectives, it invites comparison with similar troubles in other developing countries.

The key dilemma confronting Witte lay in the necessity of restoring order as a precondition for transforming the tsarist autocracy into

a regime based on law and allowing a controlled measure of public participation. Order meant repression, and repression forestalled the growth of consensus and legality in preparation for rallying public support. The condition of Russia at the time left no alternative: Stolypin soon proved the (limited and temporary) validity of a policy of order and reform combined. Yet in the half year after the General Strike, the inherent contradiction was obvious: Repression strengthened the reactionaries, and in turn prompted the continued radicalism of the opposition. The flaw rubbed off on Witte, who never succeeded in maintaining a clear course between his deeply ingrained loyalty to autocracy and his westernized, liberalizing vision; he meandered unpredictably between one and the other, to the dismay of his friends and the contemptuous delight of his enemies. Arrogant as a result of early success, he had even less chance than the much humbler Prince Lvov in 1917 to establish a political base of his own. Sociologically and psychologically oriented scholars might have made even more of the disorientation in the Russian polity and the mind of its leaders which was so essential an aspect of the Russian passage through the age of revolution. Is it not true of all revolutions that with the analytic tools available no one perceives the deeper meaning in the press of events?

A final delight in this book is the successful collaboration of the two authors. Each contributed, at the start, his own research interest; conjointly, they produced a seamless text of fine scholarly quality.

THEODORE H. VON LAUE

Clark University

Structural Change in a Developing Economy: Colombia's Problems and Prospects. By Richard R. Nelson, T. Paul Schultz, and Robert L. Slighton. (Princeton: Princeton University Press, 1971. Pp. 322. \$12.50.)

This book represents an interesting and unusual effort, based on background research of a team including economists and political scientists, to integrate economic and political aspects of the post-World War II development process in Colombia, with a clear attempt at relevance to a broader range of developing countries. While the framework is broad, specific focus is limited to three major themes: the relationship between economic development, rapid population growth, and internal migration; development as a process of structural change in a technologically dualistic economy; and policy making as a behavioral response by the govern-

ment to the shifting preferences of the governed.

The book is well written, clear, and interesting, and—more impressive—it pulls together into a rather well-integrated whole, discussions of what are inevitably only a subset of the relevant aspects of a growing economy. The analysis is solid, given the data and the analytical frameworks used. At a number of points, including the unconventional discussion of manufacturing development, it is important to interpret the book for what it is—an attempt to cut into the system from a few selected angles, for whatever new understanding this provides; there is no attempt to cover the waterfront. Given this, the only real weakness in the economic portion of the book lies in the data used; unfortunately, the ubiquitous statistical pitfalls of economic analysis in a country like Colombia sabotage some of the more interesting and central conclusions.

Among the basic contentions of the book, two deserve special comment:

(1) Growth (here the discussion focuses on manufacturing) is in considerable degree the result of technological diffusion, i.e., the spread of better ways of producing; in general, the modern firms which lead the way, technologically speaking, are the large ones. Growth proceeds as modern firms expand relative to traditional ones and some traditional firms become modern. This “structural transformation” framework for the discussion of internal growth is a refreshing variant from the usual one, where capital accumulation and automatic, generalized, technological change account for factor productivity increases. The study takes into account, as rather few have, the great heterogeneity of technologies in the production of a given good. As a description of what happens in the long run, this is clearly a useful approach, although the implication—sometimes strongly drawn—that modernity is almost assuredly beneficial is open to some question.

(2) “The introduction of a modern sector on top of the traditional craft sector, and the widening of the productivity gap between the two, present the potential for a growing inequality of the distribution of urban income” (p. 129). It is argued that the major beneficiaries of this change are likely to be the middle and lower-middle classes and that the income share of the lower half of the labor force will be reduced. The conclusion that “productivity growth in the large (greater than 100 employees) and medium size (50–100) firms tended to be significantly greater than in the small firms; indeed,

productivity in the smallest firms would appear to have declined over the period” (p. 126) has subsequently been shown probably to be incorrect, a result of quirks in the data used. And the conclusion (p. 142) that urban income distribution worsened between 1953 and 1965, based on information for Bogotá, is doubtful, because of a misinterpretation of the 1953 data; subsequent work by Miguel Urrutia and others has generally concluded that the distribution in question either improved or remained constant over this period. In short, the most recent data pertinent to this second contention lend it only limited support.

The focus on demographic issues distinguishes this book from most country analyses; in my view it is a healthy emphasis. The interesting contribution to the discussion of migration in Colombia (Chapter 3) reveals that interregional migration responds strongly to market forces, drawing rural labor to the cities from regions where the returns to labor are relatively low and the supply is growing relatively rapidly. It is argued that the wave of rural-urban migration has been heightened by the increase in the rate of population growth that followed improved sanitation, nutrition, and health practices after the 1940s.

The discussion of the functioning of a system in which the government formulates policies with an eye to maximizing electoral rewards is of interest. The authors highlight the tendency toward piecemeal policy and special purpose policy instruments; the high inertia with respect to reconsideration of policies whose effects on real income distribution are blurred and diffuse; espousal of policies which appear to redistribute from the rich to the poor. The detailed discussion of the political decision-making process is framed in the context of foreign exchange policy, which the authors feel was badly handled in the early 'sixties when a somewhat extreme version of the “disequilibrium system” involving an overvalued currency, quantitative controls, and administrative pricing was chosen rather than an exchange rate closer to equilibrium, which would have allowed the market to function more effectively. It is argued that this disequilibrium system is, in fact, the policy which maximizes the electoral support of the government, given the electorate's perceptions of the effects of foreign exchange policy.

By force of circumstance, the discussion of policy making is not supported with quantitative or other information which would help the reader appraise the various contentions as against alternative hypotheses. While this omission makes it hard to assess the validity of

some of the contentions, this section should be read by anyone who feels that the decision maker can for a moment restrict himself to the economic aspects of the issues at hand. It will, one hopes, provide grist for future researchers on these important issues. For the moment, it provides a useful framework within which to conceive the functioning of the system.

In summary, the economic side of the book is provocative. Clear framing of interesting hypotheses is stronger than empirical evaluation; in several cases, more recent data and interpretations tend to contradict the conclusions. The eternal dilemma of "how far to interpret weak data" is well illustrated here. One of the book's strong points is the neatly reasoned integration of disparate pieces of evidence; yet, when some of the pieces turn out to have been misleading, the reader may be at something of a loss as to how to "adjust" the results.

All in all, the book warrants wide readership: Economists can benefit from the clear discussion of policymaking and politics in a framework of issues familiar to them; political scientists can symmetrically benefit from the clear discussion of economic issues in a framework they understand.

R. A. BERRY

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Politics in Newfoundland. By S. J. R. Noel.
(Toronto: University of Toronto Press, 1971.
Pp. 328. \$13.50.)

Newfoundland politics has received scant attention in the literature of political science. Most people associate Canada's easternmost province with its airport at Gander, and few are aware of its distinctive political history or style of politics; from 1855 to 1933, Newfoundland enjoyed responsible government, first as a self-governing colony and later as a dominion in the British Commonwealth. Politics hardly resembled party government, British or Canadian style. Instead, Newfoundland was governed by a succession of fluid factions and parties of St. John's merchants and lawyers who gained support from the sparsely populated periphery through liberal application of patronage and spoils. Faced with bankruptcy as a result of the depression and continuing mismanagement, Newfoundland surrendered its sovereignty in 1933 and consented to be ruled by a commission of British civil servants. In 1949, Newfoundland became Canada's tenth province, governed by Joseph R. Smallwood, the only living father of Canadian confederation and a self-styled Huey Long of the north.

Under the Smallwood administration, the earlier patterns of patronage and spoils continued alongside massive (and usually unsuccessful) efforts to develop the island's economy. Employing his own populist skills as well as the material benefits of confederation (Newfoundland was plugged into the Canadian welfare system), Smallwood dominated the island's politics, ruling as a virtual dictator, until his defeat in 1972.

S. J. R. Noel's *Politics in Newfoundland* purports "to analyse the forces shaping the politics of the island during its decline from independent dominion status to bureaucratic dictatorship and its subsequent conversion into a province of Canada" (p. vii). In clearly written prose, Noel, an historian, examines the major events and intrigues of Newfoundland's political history, focusing on the rise and fall of political leaders, shifting alliances, deals and scandals, foreign relations, and the island's economy. In doing so, the author provides us with a sketch of a centralized political system in which cabals of politicians governed an island dotted with remote and destitute poor fishing villages strung out along 6,000 miles of coastline. Isolated from each other and from the capital at St. John's, but at the same time dependent on the St. John's "fishocracy" for credit and supplies, the outports elected St. John's merchants and lawyers to represent them. Parachuted into their constituencies, elected representatives served as the main link between center and periphery, channeling some patronage and benefits to their districts while engaging in a struggle for wealth and power in St. John's.

Because Noel is preoccupied with the development of a lucid historical narrative, we never learn as much as we should about "the forces shaping politics." Why, for example, did such a system persist? Throughout the book, Noel places a great deal of emphasis on animosities between Catholics and Protestants. Although these tensions were minimized early on by a proportional allocation of spoils, Noel claims that "sectarian" or "denominational" questions were used to swing several elections (but he provides no tables or charts enabling the reader to see just how Protestant, Catholic, or mixed districts voted) and that religious tensions prevented the growth of class consciousness. While this is probably true, it does not completely explain the general absence of protest movements or mass-based political parties. Early on, Noel suggests that the remoteness of the outports and their economic dependence on St. John's were inimical to political organization. But

later in the book, he describes the rise of the Fisherman's Protective Union (the exception to the rule) on the northeast coast. In this instance, Noel claims that relative poverty, communal ties among villages, and participation in the dangerous Labrador fishery facilitated the spread of the union movement. But if conditions were so favorable, why did the union movement disintegrate when its leader abandoned it some years later, and why didn't the movement spread further? Noel tells us nothing about the former problem, and attributes failure of the movement to religious differences (the F.P.U. was predominantly Protestant) and differences in social and economic structure between the northeast coast and other parts of the island. But were the differences indeed that great?

What is missing in the book is an analysis of the political culture and social structure of Newfoundland. In commenting on the continuity of political style and practices in Newfoundland, Noel alludes to a persistent political culture which has survived despite changes in regime and constitutional status. Nowhere in the book, however, does he tell us what the elements of this political culture are or how they contributed the kind of politics which Noel describes. Yet surely some analysis of the level of social trust, cynicism, and fatalism in Newfoundland outports, coupled with an analysis of social structure, particularly relationships between fisherman and local notables—merchants, ministers, and priests—would go a long way toward explaining the absence and failure of protest movements and the persistence of the system.

From the point of view of political science, one of the greatest shortcomings of the book is failure to develop a comparative context that could tell us what Newfoundland exemplifies. Reflecting on the decision of the British government and Canadian bankers to place the island under Commission rule rather than allow it to default on interest payments, Noel argues that the patronage-dominated politics of Newfoundland may not have been all that different from the politics of Quebec or New Brunswick or other remote, less developed parts of Canada. True enough. However, the author fails to make the point that the politics of Newfoundland, past or present, are not all that different from the politics of many underdeveloped or partially developed regions or nations: the marginal position of the average voter eking out his existence in a semifudal relationship to a local notable provides a ready-made base for patron-client

relationships. Dependency on distant outside forces provides opportunities for brokers and intermediaries. Party alignments are fluid because voters become attached to particular patrons and not party labels or doctrines.

It is perhaps unfair to berate an historian for not answering a political scientist's questions. Noel's book is eminently readable and provides an excellent background for anyone interested in either current Newfoundland politics or Canadian provincial politics. Moreover, by tracing the machinations of Newfoundland politicians, Noel provides us with the historical description which must be a prelude to systematic analysis.

STEVEN B. WOLINETZ

Memorial University of Newfoundland

Russia Enters the 20th Century. Edited by Erwin Oberländer. (New York: Schocken Books, 1971. Pp. 352. \$12.50.)

This series of cameos reveals how, in many respects, the Soviet regime represents a reversion to the traditional Russian pattern of political centralism and territorial expansion. They offer something of a contrast with the consistent and accelerated western involvement in the forces of nationalism, liberalism, humanitarianism, and industrialization. The essays reflect a general dynamism in all aspects of Russian life, but retarded by archaic attitudes of the "ruling spheres," and the inertia of the lumbering bureaucracy.

There is relatively little new information or interpretation in these studies except in specialized investigations like the analysis by Professor Dahm (of the State Institute of Oriental and International Studies at Cologne) of Russian philosophical writings and the investigation into the Moslem intellectual revival by Professor Bräker (of the same Institute). The essays are, of necessity condensed and often sketchy, yet interpretations are sufficiently lucid. Professors Katkov (Oxford) and Futrell (University of British Columbia) in their consideration of foreign policies observe the moderation of expansive and centralist currents by the turn of the century and argue that continued international instability, as reflected by world conflict, plus Bolshevik ideology brought a resurgence of statist autocracy and territorial expansion. The reader would appreciate an elaboration of the promise of investigating the process of decision making and implementation. For example, Witte's independent initiative would seem to imply incomplete control by autocracy over influential figures.

The article by Professor Schultz (Göttingen) on constitutional law performs a distinct service in focusing on the positive legislative activities of the Provisional Government in the areas of civil liberties and judicial safeguards. This offers some antidote to the unrelieved impression of its complete flaccidity. The movement away from centralized, aristocratic control is adequately determined. But the extent of the exclusion of the nobility from the zemstvos would seem to be overstated. The conservative nobility was able to block the establishment of the "small zemstvos" in the post-1905 period. And the reader might find the term "bourgeois" cavalierly handled. The Octobrists included all important bourgeois elements, but they are dominated by the moderately liberal landowning nobility in the spirit of the reform tradition of the 1860s. The Kadets were intellectuals, and their "bourgeois" character has to be carefully qualified in terms of Marx's "cash nexus."

In his statement on political parties Professor Oberländer (Institute for Oriental Studies, Cologne) properly focuses on the question of the course of the revolution. The Soviet position on the inevitability of a Bolshevik victory is clear. But in the United States the center of controversy focuses on the inevitability of revolution—with or without world war. In this connection the progressive isolation of the regime from its loyal support is significant and is related to its obvious inadequacy during the war. The cursory treatment of the Duma gives no real indication of the impact of parliamentary experience; even the "constitutionalist" Stolypin resigned in the face of parliamentary opposition.

Both Professors Thalheim (Berlin) on industry and Willets (Oxford) on Agriculture stress weak agrarian technological development as a key element in conditioning the slow emergence of Russia from an underdeveloped status. Willets particularly notes the refusal of the intellectual opposition to recognize the existence of the factor of a low technological level in its antagonism to enclosure. Thalheim's comprehensive study of the Russian industrial revolution stresses the traditional military goals and their negative effect on a balanced economy and living standards. Professor Poppe's (Leningrad and Berlin) related discussion of Siberian migration leaves a generally positive impression. The improved economy of the settlers would be more meaningful if compared with the rest of the empire, and analysis of returnee activity is skimpy.

The discussion by Dr. Connolly, the British expert on ethnic minorities, reveals still another

facet of the regime's tendency to violate its own rules and develop a stimulant for minority nationalism. Miss Connolly correctly stresses the essentially autonomist nature of the effective minority leadership. Because of its peculiarities the reader might wonder about the omission of the "Jewish Question." In a study of a special minority, Professor Bräker's enlightening account tells us that the rise of Moslem nationalisms in the nineteenth century reflects features not uncommon in the West and is related to the impact of faith and positivism on the Russian intellectual scene.

Professor Simon (Cologne), turning to the Orthodox Church, competently argues for the dynamic nature of Church activity at the turn of the century; and for reasons resembling those of the seventeenth century—competition with dynamic "sects."

Professors Dahm, discussing philosophy, and Stenbok-Fermor (Stanford) analyzing the Silver Age of Russian literature, project trends away from idealism toward natural morality and positivism. And Professor Anweiler (Ruhr University) recounts the realistic effort of educational reformers to promote stability in Russian society through vocational training. On the whole the collection offers an updated perspective on continuity and change in Russia at the turn of the century.

ALFRED LEVIN

Kent State University

The Army in Papua-New Guinea. By Robert J. O'Neill. (Canberra: Australian National University Press, 1971. Pp. 31. \$1.95.)

As Papua New Guinea (PNG) approaches independence, a question often asked is, "What will be the role of the army after independence?" Knowledge about the army in PNG has been limited, largely because it is part of the Australian army and therefore answerable to the Australian parliament, not to Papuan New Guineans. Writings about it have thus been either intellectually biased or, more usually, based on army press releases. O'Neill has the advantage of being both an ex-insider (formerly a career officer in the Australian army) and also a scholar of some repute. It is therefore disappointing to find that his monograph is more descriptive than analytical, more suited to a journalist than an academician. But it does at least correct some of the inaccurate and misleading reports of the calling out of the army in aid to the civil power in 1970 (pp. 14-15), and it does discuss some of the possible conflicts and difficulties which face the future PNG army.

O'Neill's use of words is surprising at times. Regarding the maintenance of internal law and order, he says that "after the recent disturbances . . . the PIR [Pacific Islands Regiment] has been given this *additional* role" (p. 11) (emphasis added). But armies usually have this role, and certainly one early Commanding Officer of PIR was aware of it: The difference in 1970 was that the role was publicly stated and there was a possibility of having to carry it out. O'Neill points out that the administration overreacted in this situation (p. 13), which belies his contention that there is "close consultation and cooperation" between the army and the administration (p. 4). The 1970 situation pointed to a lack of understanding about the army's capabilities and functions on the administration's part and possibly also to the army's failure to adequately inform the administration on these matters. O'Neill notes that the PI (Pacific Islands) soldiers "have little real idea what it takes to lead a political organisation or to run a government" (p. 17) without also pointing out the implications of this if the soldiery become dissatisfied with the way in which the country is being run.

O'Neill's perspective on Australian military relations with an independent PNG is unconsciously revealing. He believes that Australia should provide financial and logistic support, but that a tied grant would be necessary, although this would imply "some limitation on sovereignty" (p. 28). He admits that this logistical dependence "places a ready tool in Canberra's hand which could cripple the power of the military in a short space of time" (p. 26), but implies that it is desirable that Australia "can still exercise some degree of control in a New Guinea crisis," although this could be "embarrassing" (p. 30). A natural Australian view, but a chilling thought for a newly independent government.

PETA COLEBATCH

University of Sussex, England

Bulgarian Communism: The Road to Power, 1934-1944. By Nissan Oren. (New York: Columbia University Press, 1971. Pp. 293. \$12.50.)

Those of us who consulted Nissan Oren's history of the Bulgarian Communist Party, 1934-1944 when it was still in doctoral dissertation form found it a supremely authoritative narrative and analysis that would worthily complement Joseph Rothschild's "The Communist Party of Bulgaria: Origins and Development, 1883-1936." With the works of these two scholars, the history of the Bulgarian Commu-

nist movement has been covered more thoroughly than any Eastern European Communist movement.

Actually, Oren's book is even better and fuller than his dissertation. For since 1960 (when his dissertation was presented) he has done further research on the subject, including study in archives in Sofia and personal interviews with veteran Bulgarian Communists who played a part in the events he describes.

The main theme of Oren's work is the interaction between the BCP in Bulgaria (outlawed during the period covered) and the Bulgarian Communists who had emigrated to the Soviet Union. These latter included not only Dimitrov and Kolarov but also Vulko Chervenkov, who was later to become Bulgaria's Stalinist dictator and the most influential figure in postwar Bulgarian Communism. Dimitrov, of course, became world-famous as early as 1933, but a point which Oren documents very clearly is the relatively small influence he had on the Party in Bulgaria, and even on many BCP members in Moscow, for several years in the 'thirties. Only when the Comintern endorsed the Popular Front tactic and Stalin moved decisively against his own "leftist sectarians" was the authority of Dimitrov secured. (His victory resulted in the death or imprisonment of many Bulgarians in the Soviet Union. Dimitrov himself, though hardly emerging with great credit from this bloody episode, comes off considerably better than the vengeful Kolarov.)

The most exciting part of the book is the chapters covering the BCP at war. Here, the problems facing the Communists in mounting a real resistance campaign, their isolation from a population relatively prosperous and fully satisfied over the (temporary) return of Bulgaria's lost territories, the BCP's uneasy relations with Tito, the significance of Macedonia, the real heroism of many Bulgarian Communists toward the end of the war, and, finally, the maze of negotiation and intrigue leading up to September 9, 1944—all this is handled by Oren with enviable clarity and a complete mastery of his sources. In fact, Oren's knowledge and control of his sources is perhaps the outstanding aspect of the whole book. What Oren gives us is the essence of what seems to be a practically inexhaustible reservoir of knowledge on the subject.

Criticisms? An occasional brief character sketch of prominent BCP leaders would have lent flesh and color to the book. More seriously, Oren's conclusion is tantalizingly short. It contains enough WISDOM and flashes of insight to convince the reader that he could have

had a magisterial summary of the place and the problems of Bulgarian Communism in the context of Bulgarian history as a whole. Perhaps in his forthcoming study on the sovietization of Bulgaria after 1944, Oren will satisfy the appetite he has whetted in those last few pages.

J. F. BROWN

Radio Free Europe, Munich

Soviet Image of Contemporary Latin America:

A Documentary History, 1960-1968. By J. Gregory Oswald. (Austin: University of Texas Press, 1970. Pp. xvi, 365. \$15.00.)

This formidable volume is a compilation of translated Soviet Russian writings on the current Latin-American scene. The work presents a cross-section of official and academic viewpoints. In general, the latter reflect a fairly well-established division of labor among Soviet scholars concerned with Latin America. Some of the materials, on the one hand, were produced by the Latin-American Institute of the Academy of Sciences. This institute, established in 1961, is concerned primarily with the contemporary economic and political problems of the Americas. There is also represented, on the other hand, some of the work of the Latin-American Sector of the Institute of History of the Academy of Sciences; that sector's work here is historical rather than contemporary in emphasis and deals with a somewhat wider range of Latin-American themes.

While some readers might be impressed by the range of analyses and opinions developed by the Soviet writers included in this enterprise, their work—whether official or academic, contemporary or historical—falls basically within a fairly rigid framework of Marxist-Leninist interpretation. The selected materials cover such matters as the Castro Revolution in Cuba, national liberation movements in Latin America, reform programs, the role of the Catholic Church, labor and trade unionism, and problems of the rural-urban dichotomy.

Essentially, the Cuban materials pursue two fundamental theses. The first does what it can to portray the missile crisis of 1962 as a victory for Soviet diplomacy. The second reflects a post-1962 decline in Soviet interest in, and knowledge of, developments in Cuba. "Research into the development of the revolutionary process in Cuba is an extremely complex problem," M. S. Al'perovich wrote after the middle 1960s in *Sovetskaia istoriografiia stran Latinskoi Ameriki*. "Thus it is not surprising that Soviet specialists have not yet supplied clear and exhaustive answers to many of the problems connected with the revolutionary

events in Cuba, and that in a number of instances their opinions diverge considerably" (p. 309).

Two basic propositions are espoused in the included Soviet writing on national liberation movements in Latin America. It is argued, in the first place, that the Cuban experience is not applicable to the bulk of these movements. There is, second, a somewhat subtle but nonetheless clear anti-Chinese Communist line in the Soviet writing about national liberation in Latin America. The Soviet writers are almost uniformly negatively critical of the programs developed by such non-Communist reform movements as the *Apristas* of Perú, *Acción Democrática* of Venezuela, and the leftist regimes led in Costa Rica by José Figueres. In the view of A. F. Shul'govskii, the main Soviet writer in this field, the chief aim of such reformers is to "subordinate the national-liberation movement to their influence and to distort its true aims and purposes" (p. 69). The sections on the Catholic Church strike this reviewer as being the least original, and the least peculiarly Soviet, portions of the book. Little more is said here than that the political influence of the Church has been in decline for some time. This observation has been made so long by so many writers in so many languages that it seems superfluous to translate it from the Russian.

On the labor front, the quoted writers express concern about the divisive schisms in Latin-American trade unionism represented by the emergence of such anti- and non-Communist labor organizations as the Inter-American Regional Labor Organization (ORIT), the *Agrupación de Trabajadores Latinoamericanos Sindicalizados* (ATLAS), and the American Institute for Free Labor Development (AIFLD). Here the Soviet writers express the hope for eventual reunification of the Latin-American labor movement under the ægis of the more Marxist *Confederación de Trabajadores de América Latina* (CTAL). Soviet views, both official and academic, on rural-urban problems are represented weakly in Oswald's collection. The excerpts printed here are concerned chiefly with attacks on the program of the *Movimiento Nacionalista Revolucionario* (MNR) in Bolivia, and against rural reforms reflecting Chinese-type Communist approaches.

The compilation thus documents a Soviet view of Latin America not only filtered through a Marxist-Leninist analytical structure but also colored by policy positions on a number of contemporary problems of the American republics. These positions include a standoffish and tentative posture toward the Cuban experi-

ence, coupled with a warning that it not be taken as a model for revolutionary movements in other countries. The Soviet writers also express hostility toward Chinese Communist influences in the Americas, on the one hand, and, on the other, toward such indigenous Latin-American anti- and non-Communist reform movements as APRA, *Acción Democrática*, and the Christian Democratic movement.

Soviet Image of Contemporary Latin America is an impressive volume, the result of a seven-year project. Moreover, a number of hands contributed to the venture. The central task of compiling and translating the Russian-language materials was done by Oswald. The book was edited by Robert G. Carlton, and varied types of assistance were provided by the Conference on Latin-American History. The foreword was written by the late Howard F. Cline of the Hispanic Foundation and the introduction by Herbert S. Dinérstein of the School of Advanced International Studies. A result is the somewhat ironic sense of top-heavy bureaucracy among the Americans producing a book about bureaucracy in Soviet scholarship. A further disturbing element is the reiterated "Watch-out,-you-are-reading-Communist-propaganda" warning, given judiciously by Oswald himself but repeatedly hammered at by the various supporters, aides, and well-wishers of the study as well. The reader thus comes to feel that he is being treated as though he were a youngster watching a motion picture he should not be permitted to see without parental guidance. Nevertheless, the work emerges, on balance, as a major and admirable piece of well-documented scholarship, likely to stand for some time as an authoritative source of information on Soviet views of Latin America during the 1960s.

GEORGE I. BLANKSTEN

Northwestern University

Hispanismo, 1898–1936: Spanish Conservatives and Liberals and Their Relations with Spanish America. By Fredrick B. Pike. (Notre Dame: University of Notre Dame Press, 1971. Pp. xx, 486. \$15.00.)

The *hispanismo* about which History Professor Pike, of the University of Notre Dame, tells us here is a Spanish-led movement (also at times called *hispanoamericanismo*, Pan-Hispanism, or *hispanidad*) that has long sought to strengthen ties between Spain and the countries that once were her colonies in America. Prior to Pike's book, little scholarly research on the movement had been published in English, and even the voluminous materials printed in Span-

ish consisted mostly of what are in effect primary sources written by Spanish and Spanish American advocates or foes of the movement rather than studies aimed at objectively portraying and analyzing the history of *hispanismo*. In the *Political Science Quarterly* of September, 1922 (pp. 389–414) J. Fred Rippy published an article entitled "Pan-Hispanic Propaganda in Hispanic America," treating the history of the subject up to that date; it later appeared in nearly the same form in Rippy's *Latin America in World Politics* (New York: Knopf, 1928). In 1959 the University of California Press issued Mark Van Aken's modest-sized book, *Pan-Hispanism: Its Origin and Development to 1866*. To this slim output is now added Pike's substantial volume on the period from 1898 to 1936, going much beyond Professor Rippy's article in extent and depth of treatment and in the terminal date of the study. It should be noted, however, that Pike deals mainly with the Spanish advocates and supporters of the movement, giving only limited space to its reception by the Americans.

Van Aken, Rippy, and I (in introductory parts of my doctoral thesis entitled *Hispanidad in South America, 1936–1945*, available only on microfilm as Publication No. 2321 of University Microfilms, Ann Arbor, Michigan, 1951) pictured *hispanismo* prior to 1936 as seeking cultural, economic, and political rapprochement and solidarity between Spain and Spanish America largely to defend the Hispanic race and civilization (often particularly from the United States) and to promote the international influence, prestige, and trade of Spain. While presenting considerable evidence of these features of the movement, Pike stresses in *hispanismo*'s defensive posture what his predecessors mostly overlooked—the frequent aim of preventing social revolution and preserving elite rule throughout the Hispanic world. Because he sees this purpose as an outgrowth of Spaniards' concern about threats to the existing order at home, a little more than a third of his book is devoted to social and political problems in Spain alone and particularly to the ideas expounded there for coping with them. Most of the rest of the volume deals directly with Spaniards' thoughts about their Spanish American ties and with their specific efforts to promote those ties through cultural, commercial, and other channels. In addition to describing such efforts and their limited results, Pike also analyzes and evaluates the strengths and weaknesses of *hispanismo* as a whole up to 1936.

Although I saw Spain's *hispanidad* of 1936–1945 as seeking to promote in Spanish

America a philosophy in tune with the fascism and strong Catholicism of the Franco regime. I did not view the pre-1936 *hispanismo* of Spain (except in so far as it involved immediate precursors of the Franquistas) as embodying a strong social conservatism. Pike maintains, however, that both the liberal and the conservative promoters of the movement in the mother country for decades prior to 1936 were in agreement in their deep concern about warding off revolution at home, the main difference between them being the strong Catholicism of the conservatives as contrasted with the anticlericalism and laicism of the liberals. According to him, these Spanish advocates of *hispanismo* believed they could receive needed strength to check revolution through coöperation with like-minded Spanish Americans in defense of the traditional and still existing hierarchical social order, regarded as essential in the value structure of the Hispanic world. Pike has gone much more thoroughly into the sources for the 1898–1936 period than his predecessors did, and he has buttressed his work as a whole with very extensive and impressive documentation. The 138 pages of footnotes that follow his 321 pages of text are mostly devoted to indicating and sometimes briefly discussing his sources, which are nearly all published items and largely, for his purposes, primary materials, a great many of them books and periodicals that he used during a year's research in the National Library in Madrid. One might possibly ask if the overall evidence warrants quite as much emphasis as Pike places on the antirevolutionary goal of Spanish *hispanismo*, but that would come close to cavilling. In its painstaking treatment of Spanish thought and action regarding Spanish America during the period covered, his book goes far to fill a virtual void on a subject that is important to students of Spain, Latin America, and international relations.

WILLIAM B. BRISTOL

Union College

Politics in Independent Poland, 1921–1939:

The Crisis of Constitutional Government. By Antony Polonsky. (London and New York: Oxford University Press, 1972. Pp. 572, \$24.00.)

Almost without exception, studies of "developing lands" or "emerging nations" have focused on Asia, Africa, or Latin America as the geographical regions in which the patterns of evolution from colonial possession to sovereign political status are available for examination. Those scholars treating East Europe after 1918 have tended to concern themselves with traditional discussions of foreign policy, economics,

or political history within the narrow framework of a given country or time period. Yet in many ways this part of the world should be considered as containing a form of "developing nation." It had lived under an imperial rule and colonial exploitation for a long time; it was ruled forcefully by powerful, alien powers, either directly or through loyal natives; and it spawned a powerful drive for national independence that intensified as the economics became more sophisticated in response to the demands of the industrial era.

The leaders and spokesmen for these East European liberation movements gave force and legitimacy to their causes by drawing upon the past achievements of their nations for proof that they were merely the latest heirs to the "state consciousness" of the medieval or early modern empires that had played important roles in the international affairs of their day. Accordingly, when World War I brought them their independence, all of these countries faced the economic, social, and cultural problems of industrialization and modernization—not to mention that of survival in the hostile environment of contemporary Europe—under the strong influence of their political past. When examined within this perspective, East Europe between world wars not only becomes more intelligible to the Western scholar, but also provides a new model of development that in many ways is close to the historical experiences of the American or West European observer.

Antony Polonsky's study of interwar Polish politics, despite several tantalizing hints, fails to take full advantage of this approach to contemporary history and politics. Although he overlooked many basic and easily accessible archival materials both inside and outside of Poland, nonetheless the author amassed a considerable body of facts and statistics concerning the economic and political life of the Second Republic. However, the absence of a viable methodological framework within which to present and analyze his material leaves it simply a mass of unrelated information and data from which the reader must draw his own conclusions on the interwar Polish experience. The book is divided into chronological chapters that concentrate on three basic topics: purely internal political developments, economic and, to a lesser extent, social trends; and Polish foreign policy. While each section in itself is a good if superficial study, the lack of a broad perspective prevents the author from utilizing the full potential of the problem at hand. The net result is a frustrating lack of coherence that deprives the work of any true scholarly significance.

The above remarks should not obscure the

relative merits of Polonsky's work. The author does present, for the first time in English, the fruits of both memoir and scholarly publication in Poland covering the period treated, and he has included a good amount of statistical data that should aid future researchers who are interested in problems of development and cannot read the original Polish sources. But these strengths are countered, and sometimes nullified, by the negative aspects of the book. Its research base, and hence its factual material, is perceptibly unbalanced in favor of the 1920s, with relatively little emphasis on the crucial final years of the Republic. Moreover, much of the potential impact of the work is lost through careless editing and occasional factual errors; for example, Gen. Edward Smigly-Rydz was not the Minister of Defense in the "People's Government" of Jędrzej Moraczewski, but in the short-lived cabinet of Socialist leader Ignacy Daszyński, a fact that played an important role in Polish politics after 1935. Finally, the laborious and stilted style, laced with occasional clichés, makes reading the book a major chore, although the faulty editing of the publisher is chiefly to blame for this flaw. In short, Polonsky has provided a useful introductory survey of interwar Poland's transition from a liberal parliamentary democracy through a traditional military-bureaucratic dictatorship to what was emerging as a quasi-Fascist regime by 1939, but the shortcomings of the work prevent it from contributing any meaningful analysis to the study of development politics.

EDWARD D. WYNOT, JR.

Florida State University

Institutions of Economic Growth: A Theory of Conflict Management in Developing Countries. By John P. Powelson. (Princeton: Princeton University Press, 1972. Pp. 281. \$10.00.)

Institutions of Economic Growth is a welcome contribution to the growing literature on change and development for three main reasons: First, Professor Powelson is an experienced economist who maintains that "Economics has run its course in the explanation of growth . . . the next step is to merge sociological and economic models" (p. 197). Second, the chief thesis of the book relates to a central topic in development theory: "Why, in some countries is economic growth stopped by internal quarrels and mistrust? Why, in others, do competitors not only control but use them to promote growth?" (p. ix). Third, the author devotes considerable attention to policy implications and recommendations.

On the basis of a succinct but critical evaluation of models of economic development, Powelson suggests that a nation's capacity to produce institutions to manage conflicts determines whether a sustained growth will follow the take-off stage. Since not all institutions are conducive to conflict management, the criterion of "institutional effectiveness" is postulated. Essentially, institutional effectiveness is a measurement of an institution's capacity to identify conflicts within its jurisdiction so that all contestants understand them, to institutionalize well-defined norms of resolution, and to command the consensus of concerned people that the institution is more appropriate than other feasible alternatives.

Conceptualizing of institutional effectiveness in terms of conflict-resolving power, places Powelson's theory in a nonideological domain. Thus, regardless of the normative or existential rationalization given to a particular institution in a political system, its value is assessed only in relation to its institutional effectiveness. An extended family system, or a one-party rule might be as adequate to development as a nuclear family or a multi-party system given the institutional effectiveness criterion.

That different institutions may perform similar functions is an established observation in the literature. The contribution of *Institutions*, however, is in advancing a micro- and a macro-theory of institution building which if empirically tested may permit predictions on the types of institutions that power groups in various nations establish as well as on the replacement rates of institutions.

Utilizing a cost-benefit framework Powelson suggests that "If the benefits of an institution exceed its cost, and if the beneficiaries are either able to compensate those who pay the cost or force them to yield with adequate or no compensation, then the new institution will be formed" (p. 116). Although intuitively appealing, the asymmetry between demand and supply prices in the market of institutions, poses a serious problem to operationalization and hence the testability of the proposition. Moreover, in spite of the suggested heuristic design to measure costs, benefits, and institutional effectiveness, the problem of *how much* benefits must exceed costs before a new institution emerges, is not treated carefully enough.

Another limitation relates to the criterion for the selection of institutions. The theory states that growth-sensitive groups select those institutions whose effectiveness rating is perceived to be high enough to increase the gross national product at an annual rate which is believed to

have a value at least equal to the cost of acquiring the institution (p. 120). But, if the cost of changing an institution equals the perceived rating, why bother with a new institution? Further, the perceptions of growth-sensitive groups about the effectiveness of institutions may not coincide with reality. This may lead to the selection of malfunctioning institutions. A correction option is explicated in the theory. Yet establishing a malfunctioning institution even for a short run is too costly for a developing nation.

Whereas at the micro level Powelson emphasizes the choice between single institutions ruling out value change as a cost, at the macro level the focus shifts to institutional types and their relations to national values. In the long run, institutional types are to conform to national consensus on economic and political ideology. Consensus is achieved either through successive institution formation, or direct pursuit of ideology, or a mixture of the two. Successive institution formation leads to selection of an ideology because each chosen type of institution makes easier a subsequent choice of the same kind of institution. To justify all choices, an ideology is necessary. On the other hand, in the case of direct pursuit, power groups select an ideology and form economic and political theories to support it.

This last point explains, according to the author, the American failure in Latin America. The U.S. policy has two goals: One is to promote economic growth in these countries. The other is to persuade power groups to adopt an ideology and institutions similar to those of the U.S. These two goals are in direct conflict. The American ideology is not the one by which consensus will be achieved at least cost. Indeed, the effect of the U.S. policy has been to deepen and intensify ideological conflicts and to hinder the emergence of authentic but effective institutions.

In conclusion, this should be a stimulating book for anyone concerned about the process by which institutions are built, maintained and transformed. Correspondingly, it should be useful in advanced courses focusing on either political development or political economy.

DAVID NACHMIAS

Tel Aviv University

Uncensored Russia: Protest and Dissent in the Soviet Union. Edited, translated and with commentary by Peter Reddaway. (New York: American Heritage Press, 1972. Pp. 499. \$10.00.)

This is a most informative compilation of material, with insightful commentary, on a sub-

ject of great and increasing importance. Reddaway has faithfully translated, meticulously edited, and skillfully presented the first eleven issues of the unauthorized (hence, from the Kremlin's viewpoint, nonlegal) *samizdat* (self-published) journal, *Chronicle of Current Events*, containing authentic information on the dissenting Soviet "Democratic Movement." The issues assembled here were distributed every two months from April 1968 through December 1969 by anonymous Soviet citizens. By June 1972, twenty-five issues of the *Chronicle* had appeared, but its basic continuity of viewpoint, focus, and style makes these issues representative of the whole. The usefulness of *Uncensored Russia* is enhanced by Mr. Reddaway's perceptive commentaries.

Since, as Reddaway notes (pp. 15, 20) the *Chronicle* is the semi-formal organ of the underground Soviet "Democratic," or "human rights" movement, one finds in it reflections of most of the currents of dissent voiced in the USSR since it began "publication" (in typewritten, sometimes in photocopy form) in 1968. Scholars will find here indispensable source material on the activities of a small but determined and energetic set of men and women who have, as individuals and as members of loosely organized groups, in recent years contested, protested, resisted, criticized, and exposed Kremlin policies, decisions, administrative and court actions, and laws that they regarded as unjust. The reader will also learn how the Soviet regime treats dissenters. Its tactics include blandishments, threats, and harassments of KGB (political police) interrogators, and the accusations and pronouncements of journalists, prosecutors and judges, followed by sentencing to prisons, forced labor camps, and special "psychiatric hospitals" of citizens the authorities find "socially dangerous." Sometimes they include the brutal, sadistic behavior of prison, camp, and mental hospital personnel.

The duel between the exponents of the official political culture and the dissenting counter-culture, or, in Reddaway's words between "Enormous physical strength on the one side, tremendous moral strength on the other" (p. 438) is grim, tense, and dynamic: The odds of course are, at least for the present, heavily on the side of the regime, but the dissenters have won moral victories, have probably prevented repression from being more severe than it might have been, and even at times have forced the authorities to retreat—as in the cases of the arbitrary commitment to mental institutions, and subsequent release, of the mathematician and poet Esenin-Volpin and the biologist Zhores

Medvedev; Esenin-Volpin, in a dramatic reversal of official policy was allowed with a number of other leading dissidents of Jewish or partly Jewish ancestry, to leave the USSR in June 1972.

The illuminating introduction (pp. 15-42) and the fascinating "Foreword of an Eyewitness," by the mathematician Julius Telesin, who emigrated to Israel in 1970, add much to the book's value, as do the extraordinary photographs, some smuggled out of labor camps, and the map (p. 204) of the Mordovian labor colonies.

Uncensored Russia's twenty chapters are classified thematically, and to a degree geographically, in terms of: a first chapter on "The *Chronicle* about Itself"; eight on "mainstream" dissent, beginning with events connected with the trial of the writers Andrei Sinyavski and Yuli Daniel, in February 1966 and ending with the "Case of the Baltic Fleet Officers," in 1969; three chapters on camps, prisons, and mental hospitals, five on "individual streams" (The Crimean Tatars, The Meskhetians, The Ukrainians, The Jews, and The Churches); two on "mainstream publications" (mostly on Alexander Solzhenitsyn and on writings by or about Andrei Amalrik, Andrei Sakharov, and Valeri Chalidze, as well as expressions of anti-regime opinion on the 1968 invasion of Czechoslovakia); and two final, miscellaneous chapters entitled, respectively, "Tributaries," and "Dams," dealing, in turn, with "Leningrad and the Provinces" and with "Stalin, Stalinists, Fascists and Censors." In this last chapter one finds the *Chronicle's* limited references to "right wing" dissent—this material is briefly supplemented in a footnote (pp. 430-433, and footnote 45, p. 485), referring to issue No. 17, published in December, 1970.

In the introduction, and in brief, perceptive comments elsewhere Reddaway seeks to explain the behavior of Soviet dissenters and the response thereto of the authorities. He highlights Khrushchev's 1956 denunciation of Stalin, which, he writes (p. 22), transformed well-known information about Stalin's "torture and execution of millions and the deportation of whole nations" into "legitimate subjects for reflection, discussion, written analysis." However, since Khrushchev's fall "reformism within the system has become increasingly difficult, and the birth of the Democratic Movement is one of the results" (p. 23). As for the regime's policy toward dissent, Reddaway describes it as one of "hesitant repression" (p. 27). He appears to share the view of Andrei Amalrik, expressed, however, before Amalrik's current sentence to three years in a strict regime labor camp in

Magadan, that the regime is too weak to resort to mass arrests of critics. The course of events since *Uncensored Russia* went to press has, on the whole, confirmed the soundness of this view. To be sure, in January 1972 Vladimir Bukovski, probably because he had done so much to expose the KGB's practice of imprisoning regime critics in "special psychiatric hospitals," received an exceptionally severe sentence for having allegedly "defamed" the Soviet system, but in March and June of the same year the twenty-fourth and twenty-fifth issues of the *Chronicle* appeared. Prominent dissenters such as Andrei Sakharov and Alexander Solzhenitsyn, though subjected to harassment or ostracism, remained at liberty. In January 1972, however, prominent Ukrainian intellectuals, including Vyacheslav Chornovil, who had already served a three-year term in a camp, were detained, and charges were brought against some of them; and in June a number of Lithuanians were arrested for alleged antiregime protests. Also in June, 1972, the historian Peter Yakir, who had hitherto stayed out of jail, despite years of persistent—but scrupulously legal—activity in the struggle for civil rights, was arrested for "anticonstitutional" behavior. Previously Yakir had been spared, probably because the leadership wished to avoid to appear to be persecuting the son of one of Stalin's most celebrated victims. Yakir's father, Iona, was a Marshal of the Soviet Union; Khrushchev had expressed public indignation about his fate and friendly interest in his son.

It seems safe to say that all serious students of dissent in the USSR will hail Professor Reddaway's valuable contribution. Many, doubtless including Mr. Reddaway, will wish for greater elaboration of analysis as we steadily deepen and widen our factual knowledge—to which the dedicated labors of Reddaway, Abraham Brumberg, Karel van het Reve, Max Hayward, Leopold Labedz, Albert Boiter, and others have indispensably contributed. The historical perspectives and insights offered by Professor Reddaway are fundamental to understanding the complex and contradictory phenomena of Soviet dissent, which bears, as he points out, many resemblances to pre-Soviet Russian protest movements, but also differs substantially from them—particularly in its "respect for law," and its "remarkable mutual tolerance and lack of dogmatism" (p. 24).

This reviewer suggests that understanding could also be enhanced by judicious application of such social science concepts as alienation, frustration, political culture and socialization, interest groups, and social structure, among others—perhaps somewhat along the lines of

Robert Dahl's remarkable work, *Polyarchy*—though it must be acknowledged that the opacity of the data available to the student of this, like other aspects of Soviet politics, interposes formidable obstacles.

Even on the basis of our general knowledge of Soviet politics, amplified by analysis of *samizdat* sources, the development of conflict between the established elite of party and government functionaries, on the one hand, and members of the "creative intelligentsia" of scientists and writers, on the other, is not surprising—though of course we now benefit handsomely from hindsight. Party rule, as presently practiced, both frustrates professionals, as professionals (for example by interfering with access to information they regard as essential to performance of their work) and constitutes an affront to their self-esteem. Of course, a poet like Ėvtushenko is still permitted, or assigned, to travel abroad—but only, it would seem, at the cost of moral compromise intolerable to a Solzhenitsyn.

To some degree the variations in the personal responses of individuals to alienation and frustration may be related to failures of socialization—this may be applicable in the cases of Alexander Ginzburg, the late Yuri Galanskov, who died in a labor camp in 1972 after an operation for ulcers, Vladimir Bukovski, and other still relatively young dissenters, whose families were affected by the traumas of Stalin's purges.

To be sure, one line of speculation about Soviet dissent would view it as a relatively short-run phenomenon, reflecting the delayed impact of World War II, and relative Soviet economic backwardness, now being overcome. This may seem farfetched, but it is striking that there seem to be few if any new recruits, of the caliber of Peter Grigorenko, Yakir or Alexander Solzhenitsyn, for example, in the "Democratic Movement," which is also being weakened by the emigration of its leading Jewish members. The Kremlin may calculate that departure of Jewish dissenters, combined with containment of the resistance of remaining dissidents, by intimidation and selective terror, plus satisfaction of mass cravings for more and better consumer goods and services, and the enhanced effectiveness of the political socialization effort, will cause dissent gradually to subside. However, the data in this book, and events since its publication indicate that the Soviet regime will need great skill to control and contain a society characterized by a growing diversity of aspirations and interests.

Whatever may be the outcome of the struggle between the guardians of an increasingly ir-

relevant official political culture, and exponents of a democratic ethos, struggling to be born, the moral drama that unfolds in *Uncensored Russia* is, as Leonard Schapiro reminds us in the epigraph, one that "no student of Soviet society, or indeed anyone who follows the survival of the human spirit in diversity, can now ignore."

FREDERICK C. BARGHOORN

Yale University

West German Politics. By G. K. Roberts. (New York: Taplinger Publishing Co., Inc., 1972. Pp. 206. \$10.95, cloth; \$5.95, paper.)

This "concise guide to the West German political system" by a lecturer in Politics at the University of Technology, Loughborough, (p. vii) combines four different approaches to the politics of the Federal Republic. Part One briefly surveys German political history from 1848 to 1969 in order to expose the historical roots of current politics. Part Two covers the traditional subject of political institutions; Roberts bases his exposition here on the systems models of Easton and Deutsch, but properly does not allow these models to lead him into glossing over persistent tensions and conflict within the political order. Part Three, "The Political Culture," treats in rapid succession political socialization, political participation, "ideologies" (including, prelates take note, Christianity), and the nature of the political elite and of political leadership. Part Four concentrates on current issues of economic, social, and foreign policy; this section concludes with an assessment of developmental trends in West German politics and a positive verdict on the democratic character of the regime. The chronological coverage of the work extends roughly to mid-1971; comments on the Brandt government are necessarily rather sparse and inconclusive.

In several respects the book represents a promising break with many standard treatments of comparative politics. First, Roberts insists upon the importance of historical background, though his account is rather more brief and simple than one might wish; no four-page account (pp. 14-17) of the decline of the Weimar Republic is likely to be intellectually satisfying. Second, Roberts seeks to understand the evolutionary tendencies of the system; on the whole he anticipates more of the same: greater political stability, greater governmental "administration" of the society, and an increasingly less "ideological" politics—but he does hold open the possibility of a public repudiation of technocracy and concomitant demands

for greater popular participation in decision making. Third, Roberts raises the question, "Is this system democratic? He devotes only two pages to this absolutely fundamental issue and fails to specify his criteria for democratic government, but at least the problem of the moral quality of the government has been broached. One can only regret that Roberts did not deviate still further from the standard format to raise such issues as the quality of the social services that the *Bundesrepublik* offers its citizens, the degree to which "technocracy" presently prevails in West Germany, and the impact of geography and international alliances on West German policy. On all of these matters Roberts makes a promising start, but falls short of the more detailed treatment that these critical issues deserve. The result is a generally solid but not strikingly original volume.

The tone of the book is comfortably centrist. There is no treatment of the youthful opposition other than a brief (and correct) statement that the SPD's adoption of the *Jusos'* policies would lead to electoral losses. "Radicalism" of the left and right are lumped together and dismissed as reflections of an annoying personality trait (pp. 134-5); the author recognizes no basis for radical or even strongly reformist criticism of German society.

On occasion the author makes highly dubious claims ("The Christian Democratic programme [sic] has . . . [maintained] its Christian ethical orientation . . ." [p. 53]) or excessively general ones ("The Second *Reich* collapsed as a result of a combination of environmental stresses [!] and the withdrawal of support by important sections of the political public" [p. 45]). On the whole, however, the book is well executed; there is, for example, an excellent brief discussion of the armed forces. In short, while *West German Politics* does not make significant original contributions to scholarship, as a textbook it is more satisfactory than many of those currently available and merits consideration by teachers of comparative politics and German government.

TIMOTHY A. TILTON

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Contemporary Brazil: Issues in Economic and Political Development. Edited by H. Jon Rosenbaum and William G. Tyler. (New York: Praeger Publishers, 1972. Pp. 460. \$20.00.)

The nineteen original essays in this volume provide "a detailed and often technical discussion of a significant development issue or problem which has confronted Brazil during the

past ten years" (p. v). Since almost a decade has elapsed since the Brazilian military overthrew the leftist civilian regime of João Goulart on April 1, 1964, the collection allows us to evaluate political and economic development under the auspices of the military government.

Approximately half the essays deal with political aspects of the military regime. There appears to be some consensus among the contributors that the military's goal is the establishment of a development-oriented authoritarian regime. As outlined by editors H. Jon Rosenbaum and William G. Tyler in their "Introduction," the current political system is characterized by greater centralization of authority than in the past, a depoliticized or apathetic population (in part, the result of military repression), and an elite "mentality" oriented toward national security, with increasing stress on nationalist themes. Although the editors concede that the level of institutionalization of the regime is still low, thus causing it to be dependent on a continually improving economic performance, they conclude that "institutions eventually will be created as a basis for long-term military rule" (p. 26). Rosenbaum and Tyler do not, however, furnish sufficient evidence to support their conclusion.

Most of the essays which focus on politics confirm the editors' contention that political institutionalization is weak. The two political parties organized and sustained by the regime, the "official" government party (ARENA) and the "official" opposition party (the MDB) are, according to Paulo Roberto Motta, elitist organizations which "lack effective links with the people and are not prepared to adjust themselves to a situation of increasing social mobilization" (p. 226). The new bureaucracy, created in an attempt to bypass the "anti-performance" bureaucracy which was a legacy of the pre-1964 system, has, according to Robert Daland, begun to assume many of the characteristics which undermined earlier planning efforts (p. 43). Thomas Bruneau's article on the Catholic Church indicates that the military has yet to demobilize the progressive elements within that institution which are actively opposed to military rule. On the other hand, a more ideologically acceptable Church-affiliated organization such as the Catholic Labor Movement, described by Howard Wiarda, lacks the mass support which would make it useful to the regime. Finally, Robert Myhr argues that the students also have not been incorporated, and have indeed been so repressed that some are turning increasingly to terrorist tactics to vent their frustration.

The only important organized sector of the population which appears to be relatively integrated into the regime and under its control are the labor unions. In a thoughtful essay, Kenneth Erickson illustrates how the military has recognized the utility of tradition in the modernization process by resurrecting the paternalistic, corporate structures characteristic of labor-government relations during the authoritarian regime of Getulio Vargas (1930-1945). A parallel essay on the relationship between the private sector (business) associations and the government, which would have been a useful contribution to the volume, unfortunately has not been included.

That the current regime does not represent a total break with the past is evident not only from the Erickson article but also from the various detailed essays on the economy. Many of the economic policies of the military government were conceived under civilian regimes by technocrats who have continued to serve as key economic decision makers in the present regime. The policies they advocate are often the same ones they supported prior to 1964; only now, the political obstacles which they confronted as civilian decision makers have been reduced or eliminated. Tax reform is a good example. As Harley H. Hinrichs and Dennis J. Mahar note, after 1964 "Brazil achieved in less than a decade tax reforms unparalleled in the nation's eight decades as a federal republic" (p. 191). Other politically unpopular economic policies which the military has implemented include opening Brazil to foreign trade on a scale which, according to Joel Bergsman, is unprecedented since the end of World War II (p. 76), and returning to dependence on export expansion in order to repay a huge external debt incurred in the course of what John Donnelly describes as a successful import substitution effort. In agrarian matters, discussed by Marta Cehelsky, the military has favored the more traditionally acceptable goal of increasing agricultural production under the present land tenure system and encouraging colonization of virgin lands, rather than a wholesale redistribution of land.

Few books to date assemble the kind and quantity of information regarding the current Brazilian regime as does *Contemporary Brazil: Issues in Economic and Political Development*. The volume thus helps fill a significant gap in the literature. From another perspective, however, the book's strength may be considered a weakness, in that it is not very concerned with elaborating or testing generalizations about civil-military relations, political change, or other

topics of interest to students of comparative politics. I would therefore recommend the volume mainly to persons specifically interested in the politics and economic development of post-1964 Brazil.

SUSAN KAUFMAN PURCELL

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The Collective Farm in Soviet Agriculture. By Robert C. Stuart. (Lexington, Mass.: D. C. Heath and Company, 1972. Pp. xx, 254. \$12.50.)

Stuart describes his monograph aptly as "an in-depth analysis of the structural and operational characteristics of the Soviet collective farm as a mechanism for the organization of economic activity in the countryside" (p. 9). Although he elsewhere alludes to the development role of the kolkhoz (pp. 4-5), the fact that his study focuses upon the period since 1950 deprives it of significance on this controversial question. Rather, Stuart's study throws light upon the attempts of Stalin's heirs to convert the kolkhoz into a viable organization for the production and expansion of agricultural output. This involves examination of the general institutional and operational characteristics of the kolkhoz and an appraisal of the various structural and other reforms of the last two decades. Soviet leadership will find little comfort in the conclusions so obtained, but Stuart's careful sifting of organizational facts and features does go a long way toward explaining why the economic performance of the kolkhoz continues to disappoint reformers.

Chapters 2-4 are devoted to a consideration of the main changes that have taken place in the structure of the kolkhoz itself and in the administrative units with which it has contact. The amalgamation campaign of the early 1950s greatly expanded the duties of the kolkhoz chairman. Internal subunits, particularly the brigade, grew commensurately. The increase in size, coupled with abolition of the Machine Tractor Station system in 1958, led to a qualitative transformation of the production brigade, which has gradually become a permanent and multiproduct farm in its own right. Consequently, the production brigade of today is essentially comparable to the pre-amalgamation kolkhoz, and contemporary reports on the improved educational and other attainments of kolkhoz chairmen are therefore almost completely beside the point. Moreover, the increased degree of centralization of decision making implied by amalgamation has apparently offset the effect of decentralization elsewhere in the system.

All of this is, of course, well known, but it sets the stage for what Stuart has to contribute that is both new and significant in Chapters 5-7. Despite substantial improvements since 1950, the managerial incentive system for farm chairmen remains sadly inadequate. Although generally consistent with the goals set for farm managers, the incentive system has been undermined by excessive complexity and by the relatively small magnitude and the infrequent and inappropriate forms of premia payments.

Interestingly enough, however, Stuart goes on to demonstrate in Chapter 6 that "the kolkhoz manager has typically had very little effective control over production decisions" (p. 135). Inputs and outputs have ordinarily been planned from above with little regard for any expertise the chairman may have and with little room for modification at the farm level. Thus the ineffectiveness of the system of managerial incentives would not appear to matter very much, unless this kind of petty tutelage is expected to diminish, and Stuart considers this an unlikely outcome in the near future.

The introduction of *khozraschet*, i.e., cost and income accounting and planning, in the kolkhoz sector in the late 1950s, and the general upward revision of state agricultural procurement prices are considered in Chapter 7. Laudable as these changes may have been, the contemporary system still does not provide a suitable framework within which the chairman might be allowed to exercise economic discretion over the composition of inputs and outputs. Costing methods are too primitive, and relative prices are not consistent with the proportions specified for planned deliveries. Stuart quite rightly seeks to disabuse Western specialists of the notion that these reforms foreshadow the development of a variant of market socialism in Soviet agriculture.

Chapter 8, describing the characteristics of the contemporary kolkhoz chairman, is something of a digression. Stuart sees in the recent past a "continuing effort to create a 'professional' kolkhoz manager—a rural counterpart of the industrial plant manager" (p. 186). But how does this square with the preceding chapters? Why the emphasis upon the recruitment of professional managers when the incentive system is known to be too inadequate to ensure appropriate decision making and when the price and costing systems are too irrational to serve as appropriate guides to managerial discretion anyway? And, to top it off, the kolkhoz manager in any case apparently has little actual discretion to exercise.

Presumably, however, the good, "profes-

sional" manager is one who follows the plan as formulated by higher agencies, whether or not he finds it to his, or the farm's, pecuniary interest. The conversion from physical-volume targets and costing methods to ruble-value magnitudes has been, it would seem, for the benefit of the planning bureaucracy, and is not preliminary to an expansion of managerial authority.

Stuart's study is, then, quite useful in helping us to understand why the seemingly thoroughgoing reforms of the post-Stalin period have proved so disappointing in practice. The reforms have been neither sufficiently substantial nor sufficiently consistent to produce results. Unfortunately, Stuart fails, even in his concluding chapter, to put all the pieces together for the reader, and he consistently hedges his conclusions. A great deal that is presented is indeed new and interesting, but much is not. The book is full of superfluous tables, and non-economists will find an excess of economic jargon. Stuart's persistent and frequent use of the term monetization as a synonym for pecuniarization will irritate some economists as well. In short, the book would have benefited greatly from better general editorship.

JAMES R. MILLAR

University of Illinois

The Political Economy of Underdevelopment.

By Tamás Szentes. (Budapest: Akadémiai Kiadó, 1971. Pp. 328. No price indicated.)

This publication of the Hungarian Academy of Sciences is best viewed in two contexts: as a contribution to the global literature on "underdevelopment," its cause and cure; and as an explicit effort to devise a new and more sophisticated Communist propaganda approach to the developing nations.

Part One reviews the Western literature on development under six headings: the classification of nations on the basis of statistical indexes of GNP per capita and other indicators of degree of progress; the definition of underdevelopment in terms of specific characteristics of backwardness vis-à-vis more industrialized societies; models of underdevelopment as vicious circles or low-level equilibrium traps; sociological and psychological explanations of underdevelopment, including, especially, the problem of dualism; stages of growth analysis; and external inhibitions on development. Mr. Szentes has obviously read this literature and enjoyed it, and he does battle against its believed inadequacies with respect and relish in its subtleties.

His analytic argument is that: (1) the statistics conceal "qualitative" differences, related to

the position of the underdeveloped countries in the world economy; (2) the characteristics of relative backwardness are symptoms or consequences of deeper forces at work; (3) arguments in terms of vicious circles and low-level traps lack historical perspective and explanation, as do the sociological and psychological explanations; (4) stages of growth analysis lacks an explanation for the first case (Britain), underestimates the problems imposed on late-comers, and, because capitalist countries initially experienced high mass-consumption, tends to "idealize capitalism"; (5) analyses of the negative effects of the international environment are "the most valuable part" of the literature on underdevelopment, but they suffer because they regard these effects as "relative and transitory" or the result of "internal deficiencies."

At last (p. 127) the ground is cleared for Mr. Szentes' positive contribution to the debate, entitled "The Causes, Substance, and Laws of Motion of Underdevelopment. A Historical-Analytical Approach." In the following hundred and fifty pages the author argues that the essence of the plight of the underdeveloped countries is that they entered the world economy as suppliers of foodstuffs and raw materials to more advanced nations; that this relationship was economically disadvantageous as compared to a more "natural" path of development (presumably via industrialization); that it led to economies and societies that were dualistic as well as dependent; and that this dependence has continued after their political independence was gained.

Viewed as a piece of historical analysis, this important section of Mr. Szentes' book seemed to me quite unoriginal and incomplete. After all, it was Alexander Hamilton in 1791, in a postcolonial nation, who first said: "Not only the wealth but the independence and security of a country appear to be materially connected with the prosperity of manufactures." And a century earlier, the British were just emerging from a sense of quasi-colonial dependence on the Dutch. The pre-1914 story of nations as different as Sweden and post-Meiji Japan (initially forbidden protective tariffs), post-1815 Germany and Czarist Russia after 1861, Canada and the Mexico that exploded into revolution in 1910, is shot through with the struggle to build an industrial, financial, and trading base which would substitute for unequal dependencies new relations of dignified interdependence with an external world dominated by those who had achieved earlier take-offs.

Mr. Szentes' analysis of the contemporary

world has a similar static cast and a certain parochialism as well. He has spent some time teaching at the University College, Dar es Salaam. Many of his specific references are to Tanzania; and many of his observations about the contemporary developing world relate to African situations. On the other hand, he lapses over easily into sweeping generalizations about the third world as a whole, and his language tends to become both opaque and liturgically Marxist (e.g., pp. 142-7). One would never know from these passages of the extent to which overcoming the initial distortions that go with excessive dependence on the export of foodstuffs and raw materials has, for a generation and more, been an explicit object of regional and international policy in Latin America; of the successes in diversification and industrialization achieved in, say, Mexico, Venezuela, Colombia, and Brazil; of the purposeful efforts to use resources derived from oil exports to mount a wide-based industrialization program (and a successful land reform) in Iran; of the rapid movement towards diversified industrial exports in South Korea, Taiwan, Singapore, etc.

Thus, Mr. Szentes' desire to assert one proposition renders his analysis of the contemporary developing nations excessively unvariegated, and, quite often, misleading. His central proposition is that their key problem is not their own incomplete domestic modernization but the patterns imposed on their economic, social, and political life by their quasi-colonial relations with the external world.

This is the proposition Mr. Szentes believes he needs for the doctrine contained in his concluding section: "Prospects for the Overcoming of Underdevelopment. Summary and Conclusions." He first considers the prospects for world revolution and the overthrow of capitalism or for a collapse of capitalism. He judges these possibilities sufficiently unlikely to commend national efforts to overcome underdevelopment. His recommendations are: "to break the monopolistic position of foreign capital"; and "to transform the distorted structure of the economy and society." To achieve these ends, his sovereign remedy is: increase the power of the state to intervene in the economy. Here the author confronts the fact that the powers of the state in contemporary developing nations are generally large, and already directed to many objectives he commended in the previous portion of his book. In fact, his central problem becomes: What will be the fate of the "state capitalism" already endemic in the nations of the developing world. He cites, in particular,

India, Egypt, and Brazil. Mr. Szentes' formal bias is that they convert themselves into fully fledged socialist states; but his analysis of the prospects for this conversion is not particularly sanguine, and his final plea is for the liquidation of underdevelopment "in the shortest possible time," rather than a call for Communist revolution.

Behind this argument is the author's conviction that Communist analysis and propaganda (p. 19) "simplified the critique of colonialism by investigating it from a one-sided point of view and evaluated the problem of colonial liberation merely from the aspect of the 'reserves' of the socialist revolution. Moreover, it took proletarian revolutions as *direct* preconditions for the liberation of the colonies and regarded their further post-colonial development as the natural and necessary repetition (i.e., imitation) of the pattern of development of the first socialist countries in all details." His creed in this volume is (p. 21): "The criticism of colonialism must . . . be made more dialectical and realistic, thereby becoming more effective and more faithful to the Marxist-Leninist concept." His operational conclusion in this domain is: the most realistic (but not certain) route to communism in the developing world is through state capitalism.

Mr. Szentes' book is interesting as the product of a period when the osmotic communications between East and West greatly increased and the fate of Asia, the Middle East, Africa, and Latin America moved to the center of world politics. I do not believe this study adds anything of substance to our knowledge of the development problems and prospects in these regions. It does tell us a good deal about an intelligent Hungarian's struggle to link his study of development problems and his insights into East Africa to the structure and objectives of Marxist-Leninist thought and to the purposes of Communist propaganda.

W. W. ROSTOW

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The May Thirteenth Incident and Democracy in Malaysia. By Goh Cheng Teik. (Kuala Lumpur: Oxford University Press, 1971. Pp. 76. \$2.50.)

As a basis for his discussion the author cites Carl J. Friedrich's notion of democracy that *the people* is the only source of legitimate authority (p. 1) and then establishes that a state is democratic when it satisfies two requirements: (1) its authority is derived from *the people* and (2) its power is exercised with restraint (p. 2). The author then states categori-

cally at the opening of Chapter 2 that "for six years [he does not say which six years] democracy was a living reality in the Malaysia Federation" (p. 9). In this same chapter the author reviews Malaysia's political history from 1955 to 1969 and concludes with the observation that the United Malays National Organization (U.M.N.O.) was disenchanted not merely about the outcome of the 1969 election but with democracy itself (p. 17).

Chapter 3 gives an account of the events leading to the 1969 racial riots; the twelve pages of Chapter 4, the largest of the six chapters, deal with unrest within the Alliance, the National Operations Council (N.O.C.) emergency government, the resumption of the elections in Sabah and Sarawak, the results of these elections and the conditions under which the federal parliament would be reopened.

In Chapters 5 and 6, entitled "Democracy and Political Stability I and II," and consisting of slightly more than six pages, one would expect an exposition of democracy in Malaysia. But apart from a spattering of references to democracy, they seem to be no more than a summary, with conclusions, of the foregoing chapters. The author speaks of the 1957 racial pact, the disturbance of the 1957 democratic equilibrium in May 1969, and the call for voluntary restraints on the part of the non-Malays (but not of the Malays), the possibility of change in the power structure in Kuala Lumpur, and the risk of a social revolution in the absence of serious party competition.

The author draws a forced parallel between the Malays and the non-Malays of Malaysia on one hand and the Javanese and the non-Javanese of Indonesia on the other; he claims (without any authority) that Sukarno had remarked "that the first three presidents of Indonesia should be Javanese so that the Javanese can have sufficient time to mature politically and to become fully Indonesian and to accept the non-Javanese minorities as equals" (pp. 40-41). In making this claim, the author seems to have overlooked the facts that (1) the Javanese and the non-Javanese of Indonesia belong to the Malay race, whereas the Malays, the Chinese, and the Indians of Malaysia belong to three different races which make the problems of the two countries basically different; and (2) the Javanese had always been at the forefront of Indonesian national and political movements. Therefore, to speak of the necessity of the first three Indonesian Presidents being Javanese so that the Javanese could fully become Indonesians is a rather pathetic attempt to project onto Indonesia one of Malaysia's ba-

sic problems, i.e., the reluctance of Malaysia's Malays, Chinese, and Indians to become fully Malaysians. Malaysia is in the unfortunate position of never having known a public act of commitment comparable to Indonesia's *Sumpah Pemuda* (Vow of Youth) of October, 1928 in Djakarta, at which Javanese and non-Javanese alike declared that there was one nation: *Bangsa Indonesia*, one language: *Bahasa Indonesia*, and one country: *Negara Indonesia*.

More remarkable, however, is the author's failure to define who constituted or constitute "the people" as the source of authority which in turn would determine the kind of democracy in Malaysia. The author makes no mention of the fact that the Malaysia Constitution looks differently at the three major racial communities of Malaysia (Malaysia Constitution, Part III) for the purpose of citizenship as the prerequisite to authority. He also speaks of a "check-and-balance mechanism" within the Alliance Party in the exercise of power (p. 4) without ever saying what this check-and-balance mechanism is. The truth is that the Alliance is a Malay-dominated party and that the U.M.N.O. is determined to see to it that "political power in the country would remain in the hands of the Malays as sure as the sun and moon would rise" (Tunku Abdul Rahman as Prime Minister of Malaysia in *Straits Times*, May 21, 1969).

The remaining 30 pages of the book are devoted to valuable data on Malaysia's political parties, major Malaysian party personalities, and some results of the 1969 election.

The book is useful as a reading on the national disaster of May 13, 1969, but as a discourse on democracy in Malaysia it is a disappointment.

B. SIMANDJUNTAK

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The Greek Tragedy. By Constantine Tsoucalas. (Baltimore: Penguin Books, 1969. Pp. 208. \$1.45, paper.)

Greece Under Military Rule. Edited by Richard Clogg and George Yannopoulos. (New York: Basic Books, 1972. Pp. 272. \$8.95.)

Most writing on modern Greece since the coup falls into three categories—analyses of antecedent conditions, accounts by disenfranchised public figures, and policy evaluations. Of the books reviewed here, *The Greek Tragedy* remains a good example of the first category and the essays in *Greece Under Military Rule* range across all three.

Explanations for the breakdown of Greek

democracy abound, and three views are represented here. George Zaharopoulos in *Greece Under Military Rule* argues that cultural deficiencies inhibiting the development of modern parties left a vacuum eventually filled by a highly organized military. C. H. Woodhouse, in another essay in the same volume, blames the ragged temperament of George Papandreou and the imprudence of the King. Constantine Tsoucalas, in *The Greek Tragedy*, examining events through the lenses of class conflict, suggests that vested interests linked to the United States were mainly responsible. These vested interests, sometimes called the ruling class, sometimes the oligarchy, are given key roles without evidence of boundary or cohesion. The place of the military in this lexicon is obscure. Fortunately, obvious lapses from the class model are explained in terms of personality clashes or concepts such as clientelism.

Despite conceptual flaws, *The Greek Tragedy* is an important book. First, the analysis of the collapse of the parliamentary regime presented by Professor Tsoucalas is widely accepted in Greece and elsewhere. Second, this is the first adequate Marxist interpretation of modern Greek history readily available to the American reader.

The earliest chapters are informative, but the best section traces the complexities, both domestic and international, of the resistance, liberation, and civil war and analyzes their postwar consequences. The last chapters are more troublesome. Postwar economic policy is criticized on a variety of grounds, along with a tacit admission that available policy instruments limited most alternatives. Although Tsoucalas provides an acceptable version of the dual authority—the competition between the military and civilians—his arguments that a secret society was the directing force in the military and acted as a surrogate for the Americans lack plausibility. Moreover, the claims that peasants, workers, and the new middle class were responsible for the electoral successes of the Papandreous would be more convincing with empirical support.

The remaining essays in *Greece Under Military Rule* are primarily concerned with policy evaluation—the essays cover a wide range of foreign and domestic policies. Despite the underlying assumption that the major goal of the regime is self-perpetuation, readers will join the editors in lamenting the lack of an essay on the institutionalization of power. Because of this lack, the chapters on ideology and regime opposition seem strangely incomplete.

The chapters on domestic policy conclude that the regime is retrogressive. The policy

changes and variations are not judged against the same benchmarks: Instead the criteria used are the Karamanlis period or part of it, the Papandreou government, the 1965-67 interlude, and the conditions that might have existed had the colonels not acted. Sometimes the comparison is with other military regimes, developed western countries, or ideal types. To compound the problem, time dimensions are muddled, and often policy pronouncements are taken as equivalent to action.

John Pasmazoglou, the distinguished economist, contributes the most balanced chapter. He notes that national income and the standard of living has risen but at a slower rate and with a widening gap between the advantaged and disadvantaged. This growth, he says, is the result of prior economic policy—monetary stabilization and various developmental mechanisms. To the extent that the colonels have an economic policy, it seems ad hoc. Although some alterations in tax rates and in budget allocations can be attributed to the necessity of gaining the support of the business community or retaining that of the military, other populist measures such as free textbooks or debt cancellations had been planned long before, and the emphasis on small local public works may simply indicate the lack of resources for larger ones.

In a chapter on labor, Yannopoulos notes that wage increases have lagged despite higher productivity. He attributes this to government control of trade unions. The regime is undoubtedly hostile to organizations generally, but unions and cooperatives have usually been viewed as political instruments in Greece. Many citations of "repressive" regulations suggest little knowledge of other economic or industrial relations systems. Curiously, Yannopoulos notes that government plans for changes in the social security system, working hours, and job tenure, have been stymied by pressures from interested parties, but he and other essayists still conclude that the government is essentially unrestrained.

The writer on education (who apparently insisted on remaining anonymous) suggests that past practices are continued. That is, there is still little in the way of a comprehensive and coherent national educational policy. Changes in funding, enrollment requirements, the consolidation of local systems, the level of direct involvement in the university, or in curriculum—including the return to *katharevousa* (the conflict between this purist form based on classic Greek and the demotic or popularly spoken Greek has been endemic in modern Greek history)—seem to be automatic reactions to

policies of past regimes or the elaboration of decisions "in the pipeline" prior to the coup. In either case, decisions are hardly immutable. Increased enrollment at all levels is apparently simply a function of demographic change.

An essay on culture notes that domestic literature directly attacking the regime has a precarious existence—the rest, including freely circulating foreign publications is ignored. Intellectuals, because of their elitist attitudes (unintentionally demonstrated by the author, Rodios Roufos in his caustic comments on popular television programming and the emphasis on sports) are unlikely to attract a popular following.

A. G. Xydis outlines the diplomatic isolation of the regime on the one hand and the flourishing commercial and tourist activities on the other. Ultrnationalism at home is not manifested in aggressive foreign policies; indeed, the reverse seems to be the case, especially with Turkey and Albania. Maurice Goldbloom argues that recent American policy has been guided by decisions made prior to the coup or altered because of changes in the conditions underlying the original decisions rather than by the character of the regime itself. The colonels, by initiating the coup and eliminating their rivals, left the United States with a dilemma—what to do about a state whose government was distasteful but which nonetheless performed as a loyal NATO partner.

Richard Clogg in his discussion of ideology supports the general conclusions of the policy chapters—that the regime operates without benefit of any coherent ideology despite an early populist stance. Unfortunately, Clogg and his associates miss an opportunity to integrate their own observations. Much of their analysis suggests parallels to authoritarian regimes elsewhere: a populist flavor which does not require the elimination of privilege—only the presumption of equal access, anti-intellectualism with emphasis on primordial values of church and nation, as well as a general depoliticization.

Despite the claims of regressive policies and heavy-handed implementation, George Yannopoulos finds little active opposition to the regime, and he attributes this to the ideological crisis of the opposition. Clientelist parties have disintegrated, the communists are severely fragmented, and the population, discounting the traditionally passive peasantry, is exhausted from prior years of political mobilization. The resistance reflects this fragmentation and popular exhaustion and faces a regime supported by the army, economic oligarchs, and the United States.

Regardless of the preferences and intentions

of the authors, these volumes suggest that the present regime in Greece is likely to persist. It follows that the present renewed interest by political scientists in the nature of authoritarian regimes is well timed.

KEITH R. LEGG

University of Florida

Politics in a Plural Society: A Study of Non-communal Political Parties in West Malaysia.

By R. K. Vasil. (Kuala Lumpur: Oxford University Press, 1971. Pp. xii, 338. \$13.50.)

As the contents of R. K. Vasil's book amply display, the subtitle describing this study as one of noncommunal parties may be rather inaccurate. No political organization in postindependence Malaysia has long been able to maintain a noncommunal character. The pressures of communalism have been such that no party entering the electoral lists has been able to co-opt large numbers of voters from all three major racial groups—Malay, Chinese and Indian—with the exception of the dominant Alliance organization. While parties such as the Independence of Malaya Party and Labour Party may have been born with high hopes of gaining adherents from more than one community, their membership soon became almost exclusively Chinese or Malay. In addition, their programs and goals appealed more and more to the needs and demands of their communal clientele.

In this interesting and detailed political history the author has described the birth, development, and, all too often, demise of the Independence of Malaya Party, Party Negara, Labour Party, Party Raayat, People's Progressive Party, and United Democratic Party along with what are described as "non-communal parties of personage," such as Abdul Aziz Ishak's National Convention Party. In each case he details the origins, policies, electoral programs and results, leadership, membership, and organizational structure of the party. It should be noted that the field research for this study was carried out primarily in 1964 and 1965 and the body of the work was completed by 1967. Thus, the analyses and descriptions are generally applicable to the middle 1960s. A postscript assesses developments to mid-1969. While the author's analyses and conclusions as of the completion of his initial writing in 1967 have not been seriously challenged by later events, it would have been more desirable if he had incorporated the post-1967 data into the body of the text, which would have given more coherence to individual chapters.

As a rule the author is objective and nonpartisan in his analyses, although he does have a

tendency toward overstatement, as, for example, in his comments that the rout of the Labour Party in the 1964 general elections "was entirely due to its stand on Malaysia and Indonesian confrontation" (p. 151), and that the voting of 1964 "was clear evidence of the complete lack of confidence on the part of the non-Malay communities in their communal organization, the MCA and the MIC" (p. 309). As a final minor note of criticism, additional comparative analysis with other communally-oriented political systems would have been welcomed. Thus, comparisons with countries such as India, Lebanon, and Nigeria might have provided useful insights.

There can be no quarrel with Vasil's central conclusion that in Western Malaysia noncommunal parties have been fated to develop into spokesmen for one of the two major communities, the Malays or the Chinese. The UMNO has been successful in characterizing itself to the Malay community as the defender of Malay rights, and anti-Alliance Malays who have joined with representatives of other communal groups have been politically ostracized as traitors to their people. This has meant that organizations such as the Labour Party, which began with hopes of uniting the Chinese and Malays, ultimately became Chinese in membership, goals, and policies. At the same time ideologically oriented Malay parties such as the Party Raayat were unable to expand their membership against the UMNO. Only the regionally based Islamic extremist Pan Malayan Islamic Party, not described in this book for obvious reasons, has been able to effectively challenge the UMNO by promoting highly communal Malay-Moslem programs.

Overall, this is a solid piece of political history. The author has incorporated material from newspapers, government and party documents, interviews with party leaders and members, and visits to party meetings. Despite the gaps occurring because of the period in which field research was completed and despite the occasional tendency to overstate conclusions, this book is a useful addition to both the study of party systems and Malaysian studies.

FRED R. VON DER MEHDEN

Rice University

Patrimonialism and Political Change in the Congo. By Jean-Claude Willame. (Stanford: Stanford University Press, 1972. Pp. xii, 223. \$8.50.)

Eight years ago in his *Politics in the Congo* Crawford Young identified the "primary challenge" then facing the student of Congolese

politics as involving "the basic task of providing a conceptual framework adequate to order the mass of disparate data available" (p. 607). Willame's work is the first serious attempt to pick up the gauntlet. Armed with a conceptual apparatus borrowed from Max Weber the author manages to achieve what most other students of Zairian politics have thus far consistently (and prudently) declined to try—that is, to impart coherence and intelligibility to the seeming chaos and confusion of Congolese politics.

To the all-too-prevalent views of Zairian politics as a game without rules *Patrimonialism and Political Change in the Congo* offers an important corrective. "Patrimonialism" is the name of the game. The rules, as specified by Weber, are essentially three: (1) the appropriation of public office by the elite, (2) the privatization of political relationships, in turn leading to political and territorial fragmentation, and (3) the utilization of private armies and mercenaries by patrimonial rulers. Analysis of these processes of change is skillfully combined with a series of diachronic perspectives to demonstrate the inherent frailty of patrimonial rule. The author shows in considerable detail how the massive absorption of economic and financial resources accompanying the appropriation of bureaucratic sinecures by petty functionaries led to a "wholesale waste" of human energies and economic wealth; how the privatization of political relationships tended to generate powerful centrifugal pressures, and how, with the emergence of private militias, territorial fragmentation reached an unprecedented scale, in time causing a breakdown of patrimonialism and a shift in the direction of a "Caesarist bureaucracy" (p. 129). Patrimonial politics, Willame concludes, "exacerbated personal antagonisms and centrifugal tendencies that neither governmental institutions nor the existing social structures could contain" (p. 160). Furthermore, by excluding from the spoils of appropriation specific social categories, patrimonialism sowed the seeds not only of its own destruction but of an alternative political system. What has come into existence is neither a praetorian nor a patrimonial polity but a Caesarist regime built on a tacit alliance among army men, students, technocrats, petty businessmen, and midlemen.

As anyone familiar with Willame's previous work on provincial politics in Zaire might have expected, *Patrimonialism and Political Change in the Congo* bears all the hallmarks of careful scholarship. The author's argument is, on the whole, convincing; his handling of the data

brought to bear on his analysis is appropriately selective and imaginative; the qualifiers he introduces are pertinent. The main question in this reviewer's mind is whether the projection of the Weberian concept of patrimonialism into the context of Zairian politics does not require more elaboration and adaptation than the author is willing to make. Without in any way denying the relevance of the patrimonial model to the phenomena discussed in the book, one nonetheless wonders whether its utility goes very far beyond that of a mere device for ordering data.

The limitations of the patrimonial model stem in part from the extreme vagueness of its conceptual referents. This is nowhere more painfully evident than in Willame's valiant efforts to distinguish among the different types of "patrimonial and cliental relations" in existence at the provincial level between 1962 and 1965, an effort which in retrospect seems to amount to little more than a taxonomic exercise. What does the notion of "appropriation of public office" tell us about Zairian politics that could not be conveyed through terms like "corruption," "interpersonal relations," or "clientelism"? What specific forms does the "privatization of political relationships" take in the provincial arenas, and how do they differ from the "privatization" observable at the center? The answers are anything but clear. Strange as it may seem in view of the author's first-hand acquaintance with provincial politics, the weakest part of his analysis is that which deals with patterns of patrimonial relationships at the local and provincial levels. Nowhere does one get a clear picture of the types of relationships involved in "direct suzerainty," "indirect suzerainty," and "factional comradeship," let alone of the linkages operating provincially and nationally.

Omission of the specific types of resources associated with evolving patterns of patrimonialism accounts for yet another limitation in Willame's model. If the appropriation of public office is undoubtedly an important aspect of patrimonialism, one is left to wonder what resources generate public office and what resources are in turn generated by it. Ethnicity, authority, wealth, coercion have all been instrumental in one way or another in generating political exchange, either as political currencies through which public office is acquired, or as political rewards for remaining in office. Yet relatively little effort is spent explaining when, where, and how these political resources were utilized by Zairian elites. Willame's neglect of political resources as an intervening variable also

helps to explain his tendency to overstress the difference between the Mobutu regime and its patrimonial predecessor. Appropriation of public office, the personalization of political relationships, the setting up of private militias are still very much part of Zairian politics. Where the Mobutu regime would seem to diverge most sharply from the previous one is in the overwhelming importance accorded to wealth, expertise and information, in this order, for acquiring, retaining and distributing authority. Mobutu's Caesarist bureaucracy is not so different in style from the patrimonial polity it has abolished. The critical difference lies in the manner in which political resources are handled, and in the sectors to which they are allocated.

In spite of these reservations, Willame's book deserves to be widely read by political scientists. Even though it promises more than it actually delivers, its contribution to our understanding of Zairian politics is a major one. Better than anyone else so far the author has managed to reduce to a rational framework much of the irrationality involved in Zairian politics. If, as the author modestly claims, this book is nothing more than "an unfinished product," one can only look forward to the promise of his completed work.

RENÉ LEMARCHAND

University of Florida

The Long March: The Epic of Chinese Communism's Survival, 1935. By Dick Wilson. (New York: The Viking Press, 1971. Pp. xx, 331. \$8.95.)

The Long March during which the Chinese Communists moved from their old Soviet stronghold in southeast Kiangsi to the Shenkan-ning Border Region was one of the great military feats of this century. Those who survived have ruled the Chinese Communist Party from 1935 until the present—thirty-seven long, tumultuous years. If any one event was responsible for the deep rift between the Chinese and Russian Communist Parties, the Long March was that event. Only three months after the March began in October 1934, Mao's supporters eliminated Otto Braun, the German Comintern advisor, and his Chinese supporters from their political and military positions in the leadership. At the Tsunyi Conference in January 1935, Mao himself took over as Chairman of the Revolutionary Military Council and Chairman of the Politburo.

The author, Dick Wilson, has been the director of the *Far Eastern Economic Review* and is now the financial editor of the *Straits Times* in Singapore. He summarizes in this book every-

thing that is known about the March. Using Chinese, English, and a scattering of German, Russian, French, and Japanese sources, he has put together the very best account of the Long March in English, and probably the best in any language. In Wilson's chronicle, the March is not only an epic of courage, idealism, and perseverance, but also the story of how the Maoist faction first gained control of the leading organs of the Chinese Communist Party. Almost everything that he records deals with the main force of the Red Army lead by Mao and Chu Teh. The epic of the other Red armies under the leadership of Hsü Hai-tung, Ho Lung and Chang Kao-t'ao are neglected here as they are in all Chinese sources. According to Wilson, the chief legacies of the Long March have been a stringent political and military discipline, a "guerrilla ethic" that links revolutionary ardor with reliance on the peasant masses, an independence from Russia based on a profound distrust of Comintern and Soviet advice, and, eventually, the consolidation of Mao's power within the CCP.

None of these outcomes, Wilson tells us, was planned; they grew out of the experience of those who survived the Long March. The survivors continued to build on that experience, first expanding their guerrilla bases behind Japanese lines, and then driving the Nationalists from the Chinese mainland. Of the 100,000 Chinese who began the retreat from Kiangsi, only 5,000 arrived at the base in North Shensi (p. 227). By the early 1960s, only 800 of that number were still active members of the CCP. Those who rule the CCP to this day were members of that select elite. They are the men who have guided China into the modern world.

For China scholars one of the most valuable bits of new information recorded in the book is Dr. Jerome Ch'en's list of all the twenty-eight Bolsheviks, the young, Russian-trained, proteges of Stalin who led the CCP from 1931 to 1935 (Fn. 15, p. 296). In any book using so many Chinese names, American proofreaders fight a losing battle trying to get every Chinese name correct. One error that struck my eye occurs in p. xi: The *kuo-yü* spelling of Chiang Kai-shek is not Chiang Chung-cheng, but Chiang Chieh-shih. More important to me, the Tsunyi Conference was not the point at which "Chu Teh's superb generalship and Mao Tse-tung's political acumen united to turn victory into defeat" (as I am quoted as saying on p. 109). In my original sentence, defeat was turned into victory. Fortunately such errors are relatively infrequent.

This book will be read for a long time by

those who need a well-written summary of the significant military and political events that occurred on the Long March. Dick Wilson provides us with that, and with much more. This is a story of courage and endurance rarely matched since Xenophon wrote his account of the retreat of the Greeks from Persia to the Black Sea. When the Chinese Communists finally emerged from the wild grasslands of Chinghai and came upon genuine Chinese houses in Kansu, their joy can be likened to that of the Greeks when their vanguard saw the Black Sea and shouted, "Thalassa, thalassa" [The sea, the sea]. The remnants of the Red Army emerged from the Tibetan wilderness with a passionate attachment to the symbols of their homeland. They have made their experience a legend in their own lifetime. They have used the story of their ordeal as the prime inspiration for binding the Chinese people together in an attempt to achieve the good things offered by modern technology without sacrificing the idealism and cohesiveness that they once discovered in themselves, and that they attribute selectively to the best representatives of their Chinese and Marxist traditions.

JOHN E. RUE

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Planning for Economic Growth in the Soviet Union, 1918-1932. By Eugene Zaleski. Translated from the French and edited by Marie-Christine MacAndrew and G. Warren Nutter. (Chapel Hill: The University of North Carolina Press, 1971. Pp. 454. \$15.75.)

This is a very useful book. It presents a detailed analysis of the formative period in which Soviet economic planning methods took shape. As a revised and amplified translation of the original 1962 French text, the present treatise involves two forewords, two prefaces, and seven chapters, followed by a substantial statistical appendix, a thorough bibliography, and a good index.

The central chapters describe the formation of the planning system and the adjustments that developed during the hectic years after 1928 when the targets of the first Soviet Five-Year Plan were being raised, lowered, or abandoned. Chapters 6 and 7 offer a thoughtful evaluation of Soviet planning and reflective conclusions on the implications of this experience for planning theory in general.

Political scientists as well as economists will find that this study makes a major contribution to the recent literature on a fundamental aspect of Soviet development. It should be consulted

along with the two-volume 1969 treatise of E. H. Carr and R. W. Davies, *Foundations of a Planned Economy 1926-1929*, Moshe Lewin's *Russian Peasants and Soviet Power* (1968), Alec Nove's *An Economic History of the USSR* (1969), and Naum Jasny's *Soviet Economists of the Twenties* (1972). These works together provide a new and more thorough basis for understanding Soviet experience than has been available from earlier writers like Maurice Dobb.

The central politicoeconomic issue raised by early Soviet economic planning concerns the forces that led to its excessive ambitiousness. Cautious economists were swept aside by fiery party leaders, and a style of planning took shape that has, in somewhat milder form, persisted ever since. A mystique of plan precision is associated in practice with ad hoc adjustments and widely divergent results.

During this first period, as shown in detail by Zaleski, the wildly excessive targets led to near chaos by 1932. (The forces at work in the background are the subject of an interdisciplinary debate in the June, 1973 issue of the *Slavic Review*.) During the second and later plan periods, the discrepancies between target and fulfillment decreased, but even now, during the ninth Five-Year-Plan period, revisions and infeasibilities are making their appearance.

Although more than 40 years have passed since this initial tumultuous era, its legacies are still evident in the USSR. The campaigns unleashed during the first Soviet Five-Year Plan period led to a sullen peasantry, a crippled agricultural sector, a long period of domestic terror, and a permanent setback for the idealistic international influence of communism. Moreover the approach to national economic planning that took institutional form under the first plan is still being used in spite of its defects. Thus the keys to current Soviet difficulties lie in the forces that were at work even during the 1920s. Hence the relevance of this volume.

Zaleski's treatise is useful in at least three ways. He provides a great deal of statistical detail in tables and graphs compiled from primary Soviet sources whose yellowed pages are fast crumbling away. He offers an objective review of the intricate administrative developments that were involved in the evolution of Soviet planning practice. In addition, he enlivens the discussion with the pungent insights of a thoughtful political economist.

One can quibble here and there with the English translation, but in only two places is the meaning of the original obscured. In discussing Lenin's April Theses at page 29, Zaleski is not

saying that the theses contained elaborate projects, but that these projects were worked out later to meet specific needs. In footnote 122 at page 190, he is warning the reader that "putting into operation" does not signify full utilization of capacity. The whole volume is impressive, however, for the general accuracy of the translation and the precision with which the immense tasks of data transcription, bibliographic referencing, transliteration, and proofreading have been handled.

HOLLAND HUNTER

Haverford College

The Conventions of Crisis: A Study in Diplomatic Management. By Coral Bell. (London: Oxford University Press, 1971. Pp. 131. \$3.75, paper.)

Professor Coral Bell has written a very useful and insightful (though not pathbreaking or revolutionary) essay on the occurrence and meaning of crises and techniques of crisis management in the current international system.

In the methodological terms of the present era her volume is an example of traditionalism *par excellence*; as the author herself remarks, "The results of a piece of crisis management can only be observed in history, not established by theory" (p. 6). Professor Bell both divorces herself from recent American scholarship and establishes her own implicit claim to a distinctive position in the study of crises. Most recent analyses of crises have centered on bureaucracy, but Professor Bell's principal interest is in output and outcome rather than input or process, and she therefore provides a different perspective: systemic in origin, comprehensive in scope. Though her methods are quite distinct, her approach is in many ways similar to that of Oran Young and even Charles McClelland.

A critical problem however lies in Professor Bell's definition of crisis—a definition which is consciously broad:

To my mind the essence of true crisis in any given relationship is that the conflicts within it rise to a level which threatens to transform the nature of the relationship. In adversary crisis, the potential transformation is from peace to war; in intramural crisis it is from alliance to rupture (p. 9).

This notion of a "breaking strain" in a relationship is an intriguing one, but since a good deal of international politics is about near-misses and relations almost but not quite broken, the author tends to treat most major events as crises. Characteristically, the Middle East and Southeast Asia are discussed as areas of "crisis

and potential crisis," which is very different from specific incidents like those that have occurred in Berlin and Cuba, which she also considers (chapter 6). Because the author talks of "slow-motion or cumulative crisis" (p. 95), the reader begins to feel what is being analyzed are transformations of international conventions rather than established techniques of crisis management. It is indeed the dual existence of these two critical issues—"turning points" and "boiling points" and Professor Bell's attempt to identify them—which stands as the singular weakness of her approach to the subject. A crisis in a relationship which comes close to war may be quite different from an experience which substantially alters attitudes and perceptions of participants toward each other. In the Cuban missile crisis the two seem to have coexisted, but this is not the case in such disparate crises as Berlin (1958–1961) and the Congo; the Bay of Pigs and the Pueblo crisis; Cyprus and Czechoslovakia—1968.

Despite some difficulty with the meaning of crisis, Professor Bell's distinction between intramural and adversary crises and their effects on international relations are both useful and generally applicable—as her later analysis amply demonstrates. Moreover, the notion of a "crisis slide" (pp. 14–15, i.e., a series of episodes which result ultimately in a deterioration of relations and finally war), though it is not entirely new, is functional and enables her to make an excellent comparison of the interwar and present periods. The author's major conclusion that the "early recognition of an adversary relationship is the most important single political difference between the postwar and interwar periods" (p. 21) seems both apt and accurate. Her Coser-like belief that the cold war represented "a learning curve in crisis management" (p. 24) sheds light not only on the dynamics of post-1945 international politics but on the reasons for breakdown in the earlier period as well.

The comparison between the interwar and postwar periods also reasserts the ongoing paradox of this volume; though it is a book about crises, its author places greater stress in her analysis on patterns of relations and attitudes and prevailing perceptions than on specific events themselves. The difference in her message from both bureaucratic and systems theorists is to stress the tentative nature of patterns. We are thereby led to the old Aristotelian imperative of prudence: "The assumption of rational behaviour is a condition for the growth of the system of conventions: it can be established only by communication between the

dominant powers" (p. 124). Professor Bell is then both diplomatically and methodologically traditional. Removed as she is from the political and theoretical debates in this country, she provides in this essay a helpful reminder that crises can be viewed as part of overall processes in international politics rather than only as dynamics of the domestic scene. It is unfortunate, however, that Professor Bell fails to take her major themes beyond the timid conclusion that "... if the society of states can learn to manage the crises, it may learn to live with the conflicts, even perhaps finding in them a source of moral and political development" (p. 124). This book therefore provides students of world politics with an example of the significance of comprehensive historical analysis in the understanding of the techniques and instruments for the control of international instability; but it fails to move beyond this level to suggest specifically the future role of crises, the means of dominant power communication, and the types of world order which might be achieved as a consequence of the author's analysis.

STEVEN L. SPIEGEL

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After Imperialism. By Michael Barratt Brown. Revised edition. (New York: Humanities Press, 1970. Pp. lviii, 535. \$13.50.)

The early 1960s brought not only the liquidation of much of the British Empire but also, as a last brief and brilliant afterglow, the publication of a number of superior books analyzing the Empire's causes and consequences. Works by Rupert Emerson, John Strachey, Ronald Robinson and John Gallagher, D. K. Fieldhouse, and E. J. Hobsbawm are the most familiar, but there are others. Together, they form a distinguished literature analyzing one of the most important institutions in the international relations of the past century.

It was, however, an institution of the *past* century, and in more normal times one would have expected these accounts of the British Empire to assume a sedate position on quiet library shelves, respected but unread, like those other splendid books on the causes and consequences of the Byzantine Empire.

That they did not is in part due to John Kennedy and Lyndon Johnson. For at the time that the British Empire came to an end and the books analyzing it came into print, the American military intervention in Indochina and the lesser interventions elsewhere were getting under way. It was not long before indirect rule through petty princes, imperial garrisons on remote frontiers, and corporate investments in

underdeveloped countries seemed much more relevant for comparative purposes than the alliances and diplomacy of even so recent a period as World War II, and books about the old British Empire made their appearance in new American college courses in international politics.

One such book was Michael Barratt Brown's *After Imperialism*. Originally published in Britain in 1963, it was less familiar than the works by the authors mentioned above, but in the course of the 1960s it enjoyed a growing and favorable reputation among scholars and students taking an economic approach or following a radical persuasion. It has now been reissued, with the addition of a new, lengthy, and informative preface, in which the author answers some of his critics and amends and updates some of his arguments.

Aside from the new preface and an introduction, the book is divided into three parts: "Imperialism As It Was," "Imperialism Today," and "Towards an International Economy." Because these sections were written in 1962 for a British public concerned with the issue of Britain's relations with the Commonwealth and with the Common Market, it is not surprising that the three parts will be of unequal interest to American political scientists reading it more than a decade later.

The first part, "Imperialism As It Was," is by far the most important. Brown's basic approach is a Marxian one, and his emphasis is on the economic causes and consequences of British imperialism, both in Britain and in the colonies. His account is unusually balanced, subtle, and informed. Included is a critique of the earlier economic analyses of Hobson and Lenin. As such, Brown offers an important interpretation of British imperialism to place alongside several different economic analyses as well as those analyses which stress strategic or bureaucratic causes.

The second part, "Imperialism Today," primarily offers an account of the foreign trade, finance, and investment patterns of Britain in the postwar period, especially in the 1950s. The emphasis is on the role of the major British corporations and is similar to Harry Magdoff's later analysis of the international role of American corporations. Not surprisingly, the details of this part are of little interest to a reader concerned with the British role today, but those who have an historical interest in this important transition period will find the section useful, especially if they balance it with more conventional analyses. The book also contains sev-

eral pages of useful statistics on the foreign trade and investments of Britain and other industrial powers.

In the third part, "Toward an International Economy," dealing with the issue of Britain's economic relations with other countries, Brown urges Britain not to enter the Common Market but to develop a socialist economic group based on the Commonwealth. In 1963, the choice between Common Market and Commonwealth, between capitalist and socialist paths, was an engaging topic of debate, made all the more interesting perhaps by de Gaulle's veto in January of that year. But a decade later, with Britain's entry into the Common Market at last accomplished, and with its ties to the Commonwealth almost extinguished, few debates are more dated. Even the Labour Party declined to follow a program such as Brown's when it had the chance and the choice from 1964 to 1970; it is unlikely to do so when it again gets into office. One weakness in Brown's book is that he never gave an account of how his program would come about, given the structure of British capitalism as he analyzed it.

And now, in 1973, the great recession of the British Empire seems to have divided in two, part going down the dingy alleys of Belfast, part down the shiny corridors of Brussels. It is hardly a splendid spectacle. Once, however, long ago and far away, it was otherwise. It is true, as Brown says, that the British Empire at its height exploited its peoples. So has every empire. Perhaps the factor that distinguishes one empire from another is not economics but esthetics. The British Empire may have impoverished some of its subjects. But the passing of a political institution that celebrated its power, not with PXs, USOMs, and B-52s, but with Georgian mansions, public parks, military bands, and dignified courts of law has impoverished some of us too.

JAMES R. KURTH

Swarthmore College

World Communication: Threat or Promise? A Socio-technical Approach. By Colin Cherry. (London: Wiley & Sons, Ltd., 1971. Pp. 229. \$9.50.)

If a student were going to read only one book on the subject of communication to learn what that field is all about, this would not be a bad choice.

The first part of the book analyzes the process of human communication, and then describes the communication revolution of the past two centuries, comparing ancient and modern modes of communication and docu-

menting the explosive growth of means of communication in the modern world. There follows an excellent treatment of the present network of international communications, and in particular the growth of telecommunication facilities. Among the social problems dealt with, in a communications context, are the gap between the advanced and developing countries, the formation of international and intercultural attitudes, and trends toward supranational organization.

The author is an engineer and student of linguistics, not a social scientist. His subject, the social impact of the communications revolution, requires him to command all of those fields and he does so well. A social scientist may have some parochial inclination to carp occasionally at deviations from the stigmata of social science professionalism. But probably no social scientist around could handle the technical material of this interdisciplinary area well enough to maintain Cherry's across-the-board level.

Such a comprehensive treatment is possible only in a book addressed to laymen, for attempts to be more technical in any one of the disciplines that go into this book would make it incomprehensible to practitioners of the other fields. What results is a readable, well-written statement for the nonspecialist (including, for example, a page explaining the use of the correlation coefficient).

It is an old saw that specialized disciplines learn more and more about less and less, and so it should be. If one contemplates the books and articles on communication by the most important recent social science practitioners of the field, such as Lazarsfeld, Hovland, Schramm, Klapper, Lerner, or even perhaps, Lasswell, one has to recognize that they have not made their main contributions when they treated communication as a total system. Rather they have clarified how some pieces of the process work; that is how science functions. The scholars whose writings have dealt with the communications process as a social whole, such as George Herbert Mead, Cooley, Innes, or Lippmann are in a more essayistic tradition. Cherry's book is of this latter genre, though it draws heavily on what science has learned.

World Communication is an erudite book. One learns a good deal here and there in it about the history of postal service, the etymology of various political words, and the development of the international organizations in the communication field.

It is also a sensible book that documents carefully the dramatic social and political

changes which the communications revolution has brought about in both developed and developing countries. It avoids flights of futuristic fantasy about where this revolution is leading. Cherry is hard on clichés about “a shrinking world” or about communication producing understanding.

Inevitably, however, in a book as comprehensive as this one, a few errors creep in. Cherry slips slightly in interpreting the Soviet distinction between agitation and propaganda. He also errs on Soviet jamming. He notes the inefficiency and costliness of jamming of foreign short wave broadcasts, and says it was dropped in 1963 and “not resumed” since. In fact, it was resumed during the invasion of Czechoslovakia and has continued ever since.

A few such slips do not prevent this book from being an excellent compilation of basic facts about the world communication system, with the facts set in a context of generally wise and humane judgments.

ITHIEL DE SOLA POOL

Massachusetts Institute of Technology

The People's Republic of China and the Law of Treaties. By Hungdah Chiu. (Cambridge, Mass.: Harvard University Press, 1972. Pp. 178. \$10.00.)

This study of China's doctrines and practice regarding the law of treaties is of great value to all concerned with the universality of international law beyond the current ideological divisions of the world. Focusing on Chinese treaty practice, Dr. Chiu sets out to examine to what extent the People's Republic of China (PRC) differs from, or agrees with, states in the West and in the Socialist camp. He also attempts to relate his findings to the 1969 Vienna Convention on the Law of Treaties, to which the PRC is not a party.

Chiu has made profuse use of a wide range of primary and secondary sources, including texts of treaties and agreements, other official documents, and writings by PRC jurists. The six chapters broadly cover most of the procedures and technicalities concerning treaty making, entry into force, validity, interpretation, suspension and termination, and the position of treaties in international and municipal law.

The PRC's treaty law and practices are not widely different from those of the West, Chiu concludes; but, “where differences do exist, they are profound” (p. 119). One such example is the Chinese attitudes toward “unequal treaties,” which hold that inequality created consequent upon a treaty vitiates the document's validity. Another example, Chiu adds, is

the perceived effect of revolutionary change of government upon the state's treaty obligations. He attributes these and other differences to China's past frustrating experience more than to its present ideology. More critical scholars might question whether Soviet influence, at least on the two questions just cited, is as insignificant as the book appears to suggest (p. 119). Others might also wish that the author had compared Peking's stand on “unequal treaties” with what the 1969 Vienna Convention has stipulated regarding the invalidity of treaties concluded under duress (Art. 51) and of treaties in conflict with a “new peremptory norm” that has since emerged in general international law (Art. 64)—such as, presumptively, the principle of sovereign equality codified in the Charter of the United Nations.

A better example in which Peking's current attitudes are traceable to those held by previous Chinese governments would be the PRC's stand on the doctrine of *rebus sic stantibus* (vital changes in circumstances). While the book devotes considerable length to the topic, it bears noting that China in 1926, then under a different government, had invoked changed circumstances in an attempt to abrogate an 1865 treaty with Belgium. The final settlement, however, was by the conclusion of a new treaty. The instance seems to have anticipated the present PRC position that whereas changed circumstances do not *ipso facto* obliterate a treaty, they provide legitimate grounds for renegotiation.

This particular position differs from that of the Soviet Union to the extent that the latter considers *rebus sic stantibus* a capitalist ploy to beg out of unpalatable treaty obligations. Nor is the PRC stand the same as the more predominant view in the West adopted in the 1969 Vienna Convention (Arts. 62, 65, and 66, and Annex), which relate changed circumstances to treaty termination, suspension, and withdrawal, as distinct from treaty revision. In its pronouncements on *rebus sic stantibus*, Peking has quite consistently maintained a distinction between treaty revision and treaty termination, between what is repudiable and what is repudiated, more especially in regard to treaties of cession and boundary delimitation (hence, the Sino-Soviet border talks). There seems to be some confusion on this point (pp. 102–103).

The newness of the field offers opportunity and challenge. But Dr. Chiu's work will stand on its merits, which are legion, and will take its proud place in the inchoate literature on China and international law. One might demur about his interpretations, such as whether the PRC

considers treaties "an important source" (p. 3) or "the most important source" (p. 5) of international law. But no one can deny the value of this timely monograph on a subject of growing importance. The book does not purport to be definitive nor to go beyond the legal dimension to a consideration of policy behind and underscoring treaty making. Hence, some of the omissions are quite understandable: among these is the lack of any extensive discussion of the Five Principles of Peaceful Coexistence, which are embodied in many of Peking's more important political treaties and held by its jurists as among the peremptory norms (*jus cogens*) of general international law.

The book is a well-researched and well-documented product of many years of intellectual drudgery, and it will add to the laurels of the Harvard Studies in East Asian Law directed by Jerome A. Cohen. The footnote section, the extensive bibliography, the Chinese-English glossary, and the appendices containing texts of selected treaties, are just as valuable as the main text, all executed with a meticulousness essential for serious scholarship.

JAMES C. HSIUNG

New York University

The Soviet Invasion of Czechoslovakia: Its Effects on Eastern Europe. Edited by E. J. Czerwinski and Jaroslav Piekalkiewicz. (New York: Praeger Publishers, 1972. Pp. 224. \$14.00.)

This is a collection of very brief, often superficial, often repetitious accounts of the Soviet invasion of Czechoslovakia in 1968. The ten contributors, all academicians, for the most part attempt to discover the impact of the occupation on its several environments—Czechoslovakia itself (Jaroslav Piekalkiewicz, Hana Benesova and Ivan Sviták); USSR (Roger E. Kanet); Hungary (Rudolph L. Tokes); Hungary and Poland (George Gomori); Romania (E. Bennett Warnstrom); Yugoslavia (Werner Sichel); and East European theater and drama (E. J. Czerwinski); in addition, Ivan Volgyes compares the Hungarian Revolution of 1956 with the 1968 Prague Spring. We are thus confronted with a *potpourri*, more suggestive of ready availability than of systematic insight, or even another catch-as-catch-can treatment of a hot topic.

But this may be too harsh a criticism. Why include other East European countries, for example, if their cases are less interesting than those discussed? Or why treat the topic on several levels of analysis, or for East Europe as a whole, for that matter, if a significant key is provided via the chapter on East European the-

ater and drama? In their introduction, the editors point out that the contributors "represent various ideologies and points of view," . . . "uniformity of opinion is notably absent," . . . and . . . "this is as it should be" (p. x).

Whether it should or should not, it is not the case in this compilation. Here various ideologies do not necessarily mean various points of view. Ivan Sviták, for example, member of the Czechoslovak Communist Party since World War II, lost his good standing only lately when his point of view became similar to those now contributing to the volume. And if uniformity of opinion means that all the authors agree that the Soviet invasion was a bad thing, then the book is certainly uniform.

As to quality, insight, and innovation, the contributions vary. Jaroslav Piekalkiewicz's chapter on surveys of public opinion in Czechoslovakia before and after the invasion provides solid evidence that there was no counter-revolution there. Far-reaching reform, yes; revolution, no. The Communist party under Dubček would have won in a free and democratic election because it would have been the popular—not an elitist—organization. Hana Benesova contributed a useful description of the important influence of Czechoslovak writers on the sequence of events. Ivan Sviták, in two chapters, spins his customary grandiloquent and decisive views on the cosmic consequences of the occupation, ready as always to digress on all kinds of significant though not necessarily related topics at the faintest suggestion. Reading him is getting tedious—he has said many of these things before, and equally vehemently and in an equally grand style. Without new research, I fear, he may get bogged down in banalities. Roger Kanet's chapter on Soviet foreign policy is informative, though not as profound as his other writings. George Gomori's comparison of Hungarian and Polish attitudes is insightful and well-researched. Ivan Volgyes offers a useful comparison of the similarities and differences between 1956 Hungary and 1968 Czechoslovakia. But how important are the similarities and differences for the dynamic process and its outcome? What we need are tight, parsimonious assessments, rank orderings and causal correlations: We have by now too many "first steps" in the right direction but few follow-ups. Rudolf L. Tokes's essay on the reactions of Hungarian intellectuals is refreshing and careful. E. Bennett Warnstrom's otherwise perceptive study of Romanian reaction is incomplete and even obsolete because there is no evaluation of post-1969 developments. Werner Sichel's chapter on the effects on Yugoslavia is solid, systematic, and revealing. And E. J.

Czerwinski's evaluation of the effects on theater and drama in Eastern Europe is both articulate and professional. Professor Czerwinski convincingly shows how important are the insights that can be gathered from an East European intellectual mainstream such as theater and drama.

Taken as a whole, the volume is inadequate for two principal reasons: one, lack of intellectual direction by the editors; and two, the brevity of the individual contributions. The editors, in my opinion, fail to make the book a coherent whole. Apparently their instructions to the contributors were minimal, and this shows throughout the book. Moreover, the reader looks in vain for a chapter bringing the scattered observations of the several authors together in an overall, meaningful analysis of the "effects on Eastern Europe." What does it all, indeed, amount to? And the excessive brevity of most of the contributions results in superficial treatment of the respective subjects (exceptions are Piekalkiewicz and Sichel).

Still, the effort should not be discarded as wasteful. I, for one, find it useful to have several postinvasion cuts across the several environments discussed here. The principal value of this heterogeneous volume is indeed historical. The several country profiles, changed or changing since they were frozen in the individual chapters, may be fruitfully compared with the subsequent developments for a longer-range analysis. And here, I submit, rests the heuristic saving grace of the book. I would like to return to it five years from now.

JAN F. TRISKA

Stanford University

International Law, National Tribunals, and the Rights of Aliens. By Frank Dawson and Ivan L. Head with the collaboration of Peter E. Herzog. (Syracuse, N.Y.: Syracuse University Press, 1971. Pp. xviii, 334. \$11.75.)

In his foreword to this timely and informative book, Professor Richard Lillich suggests (p. vii) that no published work to date has attempted to do what this volume sets out to accomplish. The book is the product of fieldwork in twenty-five countries, funded by a Ford Foundation grant. One of the authors, formerly a practicing lawyer in New York and Caracas, is an investment banker in London; the other is a professor of law at Syracuse University. They acknowledge in their preface the assistance of practitioners, government officials, businessmen and scholars in the countries visited.

An introductory chapter examines the idea of injuring a state through its nationals, submitting (p. 23) that foreign offices have been more

important than national or international tribunals.

A chapter on "The Challenge of New Perspectives After 1945" notes that the European Commission on Human Rights is an indirect means by which a private party's grievance can be brought to the attention of an international body (p. 30). Powerful states are regarded as less able to intervene in the affairs of weaker states to secure compliance with international law, and the concept of sovereign equality takes on a "new vitality" as a result of the nuclear stalemate (p. 44).

Developing practice in the Commonwealth receives particular attention. Despite the Judicial Committee of the Privy Council's ceasing to be the final court of appeal for many Commonwealth countries, the authors discern a common measure of attitude toward aliens (pp. 62-63), and British forms are said to be used in thirty-one countries (p. 73). The study notes that extraordinary judicial remedies are available in Commonwealth states which have a common-law heritage (p. 90), and that constitutions in Western Europe "quite generally provide that the judiciary shall be . . . subject only to law" (p. 101). The authors advocate that alien investors in developing countries pursue their claims against both governments and individuals through legal channels (p. 105).

An instructive chapter notes that most nations allow aliens access to tribunals on a basis of equality, mentions theories that are applicable (p. 109), and points out some historical variance that has evolved over centuries (p. 157). A following chapter notes that, if one party in litigation is from a civil-law country and the other from a state in which the common law is basic, some rules may seriously affect the conduct of the litigation (pp. 159ff). The authors maintain that even in the United States there are problems about delayed litigation, and they refer (p. 175) to the United States policy of not espousing claims against other states unless there is claimed violation of international law.

Assistance requested between the courts of two sovereign states normally presents problems that may be treated differently in applicable treaties and legislation. Summarizing, the authors submit that "the structural procedural problems discussed in this Chapter and in its predecessors . . . may become increasingly complicated when an alien seeks to bring suit against his host government . . ." (p. 192).

As to the theoretical capacity of nationals and aliens to sue, the authors note, in a chapter on "Protection against Governmental Action," that this recourse has been hindered by the

concept of sovereign immunity; United States statutes permit an alien to sue if he can show that United States citizens are permitted to sue in the courts of the alien's country (p. 215). All the countries whose practices were examined are said to permit suits against the governments in contract, in tort, and for violation of fundamental rights—if there is a showing of reciprocity (pp. 230–231). In Austria the rule of international law requiring compensation to aliens for expropriation is part of Austrian law; and in this matter aliens may be in better positions than Austrians (p. 243). The United States and West Germany provide by treaty that nationals of a party state may not be deprived by the other party state of property without just compensation; the German Supreme Court has held that nationals so deprived would be entitled to full compensation (pp. 244, 244n).

In 1965 the Executive Directors of the World Bank submitted to its member governments the Convention on the Settlement of Investment Disputes between States and Nationals of Other States. It is "posited on the theory that arbitration and conciliation are alternatives generally more preferable to alien investors than either exclusive reliance on the tribunals of host States or on the uncertainty of diplomatic representations" (p. 245).

The final chapter on "Techniques for Achieving Minimum Standards of Procedural Justice in National Tribunals" refers to the Latin American states' suspicion of "international attempts which they believe somehow might weaken whatever protection is afforded them by the Calvo Doctrine against overreaching alien investors" (p. 300). The Local Remedies Rule "would be of little relevance if States did not accept the obligation to open their courts and tribunals to alien litigants on a non-discriminatory basis with nationals" (p. 311). Private practitioners, the authors conclude, "must learn to regard themselves as agents or officers of the international legal order. . . ."

Students and practitioners of international law, as well as individuals and firms doing business in foreign countries, will find in this volume an instructive and welcome addition to the literature on the legal rights of aliens and alien companies.

ROBERT R. WILSON

Duke University

The Communist International: 1919–1943, 3 vols. Selected and edited by Jane Degras, (London: Frank Cass and Company Ltd., by arrangement with Oxford University

Press, 1971. Vol. I (1919–1922): 1956, 463 pages, \$22.50; Vol. II (1923–1928): 1960, 584 pages, \$30.00; Vol. III (1929–1943): 1965, 494 pages, \$22.50.)

The 1971 reissue of the three volumes of Comintern documents selected and edited by Jane Degras, and originally published in 1956, 1960, and 1965 respectively, is another indication of renewed interest in the activities of the international Communist movement in the period between the two World Wars. We have witnessed the publication of excellent studies on early Soviet foreign policy, biographies of key Communists (for example, Karl Radek and Antonio Gramsci), and newly uncovered documents and correspondence relating to the Communist International. Important work on the Comintern is currently being published, especially by scholars at the Hoover Institution. This material supplements but does not supersede Mrs. Degras's pioneering work and reinforces the judgment that her selections were judiciously made.

The Degras volumes hold up very well. The commentaries on the documents are informative and historically sound. The translations are uniformly excellent, models of care and clarity.

By far the most interesting years of Comintern activity are the 1920s, and the volumes are strongest here. Communists in Germany, France, and Italy were in the thick of political life. The Communist International was not yet the subservient tool of Soviet foreign policy that it was to become after its Stalinization in 1929–1930, and its internecine intrigues provide additional insights into the rivalries then existing within the Communist Party of the Soviet Union and even into the nature of international Communist relationships today.

The volumes concentrate on European communism; they are relatively weak on the Third World, whose parties, with the exception of the Chinese Communist Party, were marginalist movements. They are admirably organized and are valuable sources for scholars working on generalized treatments of Soviet and international Communist policies of the period, as well as for students being introduced to the language of intra-Communist discourse and communication. However, for specialists working on particular aspects of the 1919–1943 period, a detailed examination of all the original materials will still be essential.

ALVIN Z. RUBINSTEIN

University of Pennsylvania

Presidents, Bureaucrats, and Foreign Policy: The Politics of Organizational Reform. By

I. M. Destler. (Princeton, N.J.: Princeton University Press, 1972. Pp. xii, 329. \$10.00.)

There seems to be a small band of scholars, journalists, and practitioners who find a continuing fascination with the foreign policy and national security machinery of the United States. They are well served by Mr. Destler's book, which, it should be said, deserves a wider audience. He has written perceptively and thoughtfully about the foreign policy organization, carrying the story of its trials and tribulations through the presidencies of Kennedy and Johnson and most of Mr. Nixon's first term. Mr. Destler has read the relevant literature carefully and critically, and apparently talked to many knowledgeable insiders with equal skill.

His analysis is clearly strengthened by the "bureaucratic politics" perspective he brings to bear on the subject. He has not adopted the specific framework of any one writer but freely admits his intellectual debt to the work of such scholars of foreign policy making as Graham Allison, Richard Neustadt, Roger Hilsman and Morton Halperin, among others. While there are strong differences of opinion about the explanatory weight of bureaucratic-political factors, it is true that most of us who have written in this field have tended to overemphasize rational explanation and formal organizational structure and function while ignoring organizational processes and politics.

Wherever an appropriate balance may lie in the study of particular policies or crises (e.g., Allison on the Cuban missile crisis, Neustadt on the Skybolt and Suez decisions), the bureaucratic politics perspective is ideal for Destler's purposes, that is, the question of organizational effectiveness in the making and carrying out of American foreign policy or, as he puts it, "the central question of how organizational reform is to affect governmental practice" (p. 40). Destler defines bureaucratic politics as "the process by which people inside government bargain with one another on complex public policy questions" (p. 52). He goes on to explain the pervasiveness of the process as arising from two inescapable conditions: shared power and differing views concerning these questions. Thus, bureaucratic politics is viewed as a normal dimension of governmental decision making, to be taken into account in any efforts to improve the latter rather than to be circumvented or transcended by proposed reforms.

Armed with this critical stance, Destler has some shrewd and sensible things to say about the many reform efforts and proposals directed to the foreign policy organization over the past twenty-five years, and the doubtful assumptions

underlying most of them. His chapters on White House and State Department foreign policy making are excellent. Indeed, his description and analysis of foreign policy making patterns and procedures in the White House during this ten-year period are the best that I have seen in print. He makes clear the strengths of the Nixon-Kissinger arrangements while at the same time illuminating the more fundamental shortcomings that make them in his view an undesirable model for the long-term. He echoes the critical analysis of the State Department and the Foreign Service that has become almost standard in the postwar period, but more persuasively than most.

In characterizing the "general substantive staffs" that have in his view been most successful in meeting presidential needs in foreign policy, he cites the National Security Council staffs since Kennedy, and the Office of International Security Affairs (ISA) in the Defense Department in the 1960s. Given his strong emphasis on politico-military affairs and the generally low marks he gives to the performance of the State Department, I was a bit surprised that he did not give more attention to the role of the State Department's Bureau of Politico-Military Affairs in the Nixon-Kissinger period, as well as its earlier role as a staff in the 1960s. But this is a minor cavil (perhaps arising from my own service with that staff).

To raise serious critical questions about Mr. Destler's book, one must turn to his basic assumptions. If one asks why the foreign policy organization should be more effective, Mr. Destler's answers are the conventional ones: the overseas influence of the United States should be "exercised by coordinated government actions which support conscious, central United States policy purposes" (p. ix); the aim is "*purposive and coherent* foreign policy" (p. 4; italics the author's). Around whom is this strength and sense of direction to be built in the American government? Again the answer is the expected one: the president.

Mr. Destler is well aware that the conventional wisdom no longer goes unquestioned. He makes clear his own awareness that presidential will and wisdom are limited and that coordinated and coherent policies and programs are not necessarily good ones. He recognizes that his proposal for strengthening of State Department staffs at various levels of that organization is no guarantee of improved policy, while at the same time arguing that "the more that good people close to key officials are commissioned to raise hard questions, the more likely these questions are to be heard" (p. 291).

Clearly, none of this will satisfy those critics who insist that the concern to strengthen central, presidential control and direction is fundamentally misplaced. In their view, our direct problems have not come from lack of coherent, centrally directed and coordinated policies and programs but from too much presidential and national capacity to become involved and to project tremendous national energy and resources overseas. Before we further strengthen the system's effectiveness and its responsiveness to the will and purposes of the president, we had better concentrate, say the critics, on altering the values, attitudes, and perceptions of those who run our government, or the broader goals and purposes of the society, or indeed something even more radical, like fundamental change in its institutions.

Mr. Destler does not tackle these questions in any extended fashion, and there is no reason why he should, given the tasks he has set himself. He argues, as I have indicated, that the changes he proposes make sense in bureaucratic-political terms and that they should increase the system's capacity to think critically as well as to move more effectively. He will not satisfy the more radical contemporary critics of American foreign policy, whose questions do require serious pondering by students of the process as well as the substance of American foreign policy.

BURTON M. SAPIN

George Washington University

Soldiers Without Enemies: Preparing The United Nations for Peacekeeping. By Larry L. Fabian. (Washington, D.C.: The Brookings Institution, 1971. Pp. 315. \$7.50.)

This is a very good study indeed of the "peacekeeping alternative to collective security" by a Research Associate in the Foreign Policy Studies Program at the Brookings Institution. In the author's words: "This study attempts to provide a basic history of preparedness for UN peacekeeping, to isolate the factors most instrumental in shaping preparedness institutions and diplomacy, and to suggest some desiderata for analysts and policy makers thinking about the future of UN preparedness" (p. 9).

Chapters 1-6 consider past and current experience in planning and organizing international military forces. The first true peacekeeping actions are thought to be the League of Nations' efforts to insure peaceful plebiscites in Vilna (never carried out) and the Saar (completed, principally under British direction). They differed significantly from subsequent UN experience, however, in that major powers directed

the planning and execution and even participated in the operations with only the aid of soldiers and civilians from smaller countries. In contrast, UN arrangements have depended on initiatives by the Secretariat and smaller powers. The sum total of this experience has given rise to principles of action that dominate current thinking: consent, noncoercion and impartiality. Fabian takes note of a paradox in the UN experience. As peacekeeping activity increased, governments, in general, exhibited a marked reluctance to *prepare* for future contingencies. "Yet preparedness did make advances—and notable ones. What happened was that while peacekeeping services became more visible, peacekeeping preparedness went underground" (p. 79). Later in the work (p. 191) Fabian concludes that "tacit understandings about peacekeeping are more permissive than explicit consensus ever could be under present circumstances." A particular merit of the work is that planning is seen as a three-way struggle among diplomats, secretariat officials, and peacekeeping technicians. The reasonable demands of the latter two groups often cannot be met by the diplomats who have been unable to agree on the basics—how to authorize and direct an international force and how to supply it. This framework tells much more of the story than would an analysis limited to the national positions formally taken in formal bodies.

What advances have been made? A number of countries responded to political initiatives taken by the Secretary General or took initiatives themselves in concert with that official. While the Charter provides a shadowy model for this sort of thing, it is faint compared with the big-power initiative and control prescribed for enforcement forces. The newer model required, first of all, secretariat initiative and responsive "middle powers" such as Canada and the Nordic countries—not so large as to threaten and not so small as to lack influence. These powers have been persuaded of the need to prepare, and prepare *for*, the use of "standby forces." They have been joined for practical purposes by a "wider fellowship" of countries "that supply forces without setting up standby systems." Both in practice have depended on at least big-power acquiescence and crucial logistical and technical support principally by the U.S., the U.K., and Canada. It is difficult, of course, to qualify as a peacekeeping power. The U.S. is too big to be noncoercive or impartial, although her logistic and financial support has been essential. The Netherlands has so far been largely unable to put her preparedness measures into practice because she has been

compromised by her colonial past. While several countries—Ireland, India, Brazil and Yugoslavia—refuse to designate standby forces, they are nonetheless “prepared” by virtue of their participation in at least two of the UN’s three big peacekeeping operations in Egypt, the Congo, and Cyprus. Their balking at designating forces in advance preserves their freedom of action in a future perceived dimly at best.

In the two final chapters, dealing with the perspectives and the future shape of preparedness, Fabian sets forth a rationale and a plan of action to maintain the slight momentum that has been gained. Drawing on both the experience of past and current operations and the record of the Committee of Thirty-Three (UN Special Committee on Peacekeeping Operations), he posits a ten-point strategy looking toward a “parity of super-power noncontrol over the preparedness system” (p. 226). Concomitantly, an increase in political responsibility and resource planning is required of medium-sized and small powers. This is neither new nor earth-shaking. More impressive is the evidence Fabian adduces that this sort of thing has been and will be tolerated informally by the major powers despite *pro forma* disagreement. Although there is no possibility of enforcement schemes under the “original Article 43 punitive model,” Fabian concludes that the framework provided by this article plus the existing but unused Military Staff Committee might be used to prepare for peacekeeping contingencies. He suggests the Russians might go part way to meet American views on the operational responsibilities of the Secretary General since he is required as the UN chief administrative officer to execute the relevant decisions of the Security Council.

Although there is a long way to go before the world is prepared properly for peacekeeping, Fabian points the way. This policy study merits the influence it is doubtless having already on decision makers at the UN and in national capitals.

DANIEL S. CHEEVER

University of Pittsburgh

History of the Cold War: From the October Revolution to the Korean War, 1917–1950; From the Korean War to the Present. By André Fontaine. Vol. I translated by D. D. Paige; Vol. II translated by Renaud Bruce. (New York: Random House, Vol. I: 1968, Pp. 432, \$12.50; Vol. II: 1969, pp. 523, \$10.00.)

André Fontaine, a distinguished journalist from *Le Monde*, has produced a work of dis-

tinguished journalism; it is certainly not the best kind of history, although better history than many professional historians produce. Essentially, what Mr. Fontaine has written is an informed narrative with occasional analytical comments, in two volumes each divided into two parts. In the first volume, he discusses “Socialism in One Country,” which takes the story from 1917 to 1945, then “The Duel,” which looks at the years 1945 to 1950. In the second volume, “Learning the Facts of Coexistence” moves from Korea on to Hungary and Suez, and “China Takes up the Torch” starts with the launching of the first sputnik and ends with the deaths of Pope John XXIII and President John F. Kennedy. The style of presentation is lucid and for the most part congenial, although not everybody will care for such chapter headings as “Just a Moment, Mr. Executioner . . .” and “His Majesty Sputnik,” or for some of the thumbnail portraits which seem to come from the school of *Time* rather than from that of *Le Monde*.

Already widely praised for its fairness of approach, the work does indeed present its account honestly. Moreover, Mr. Fontaine draws on his own working experience to give useful descriptions of Hungary and Suez and the other critical moments of the Cold War. Examining the first “week of truth,” as he calls it, he draws an interesting picture of the predicament of Janos Kadar and convincingly describes the right-wing turn that the Hungarian government would probably have taken if Imre Nagy had not been removed. Turning to the second “week of truth,” he exposes the conduct of Anthony Eden in a merciless manner which may well have given Lord Avon cause to regret his own praise for the “objectivity and sense of purpose” of the first volume, and rightly points out how the whole adventure marked the effective end of the pretensions of the United Kingdom to act as a great power in special relationship with the United States, whether to the east or west of Suez.

While Mr. Fontaine’s two volumes are among the worthiest of the many produced on the subject during the last decade or so, they still fall short of the highest academic level, for his viewpoint is, surely if unwittingly, circumscribed by his background. In the first place, his outlook is very much that of a Western European. He talks of the “barbaric name” of the Azerbaijan (I, 280), which would not please the Azerbaijanis, and of the Berlin Wall as “most sinister symbol” of the Cold War (II, 3), which would bring howls of righteous indignation from Southeast Asia. The Chinese could

not be happy that the section entitled "China Takes up the Torch," starts and finishes with events taking place outside their borders, as described above. Second, the sources used are overwhelmingly of Western origin. Although Soviet sources (mostly in translation) are cited more often than in some self-styled "impartial" Cold War histories, there is little or no mention of such standard Soviet works as *Mezhdunarodnye otnosheniia posle vtoroi mirovoi voiny* [*International Relations after the Second World War*], edited by N. N. Inozemtsev and his colleagues. If such works are to be ignored for willful perversions of the truth, then a large number of Western interpretations, not to mention official government pronouncements, could be rejected on the same ground.

More seriously, the longer historical perspective presented in the work of the two major protagonists in the Cold War suffers from a severe imbalance. This is particularly noticeable in the first part and is indicated by the title, "Socialism in One Country." Considerable emphasis is given to the Russian Revolution of 1917, its aftermath and implications for international affairs. There is also the suggestion that the attitudes and policies of prerevolutionary tsarist governments were by no means completely dissimilar to those of their Soviet successors. Like George Kennan, Mr. Fontaine considers that many of the observations made by the Marquis de Custine on the Russia of Nicholas I have a strange applicability to the Russia of Joseph Stalin. He makes several references to the Byzantine and Muscovite origins of the "Soviet Empire" in a neo-Slavophile manner which posits a strong continuity from the past to the present. On the other hand, he does not seriously contemplate the possibility of comparing, say, the United States of James K. Polk to that of Franklin D. Roosevelt, nor does he even devote much attention to the foreign policy imperatives of the New Deal's "Capitalism in One Country," if that it can be called. There is little reference to "Manifest Destiny" or to the American variation of imperialism.

Exceptionally, the author does tell us something of the origins of the Monroe Doctrine as well as of its later extended application, while not following up the full background to Ivar Maisky's reported remark that "Russia, too, has its Monroe Doctrine." Although he cites an appropriate passage from the London *Times*, he fails to point out with sufficient force the purliness of Truman's decision to take action against Soviet interest in the Dardanelles on the grounds that "we might as well find out whether the Russians were bent on world con-

quest now as in five or ten years" (I, 217, 290), even if he goes on to call the enunciation of the Truman Doctrine a move with "all the brutality of a declaration of war" (I, 293). He gives nothing like sufficient weight to the impact on the Soviet Union of the Second World War (for example, its loss of about twenty million people as opposed to the loss of well under half a million by the United States) and to the fever pitch consequently reached by the traditional mania for security.

The severe imbalance in his longer historical perspective hampers Mr. Fontaine when he comes in his "Epilogue" to consider the years since 1963 and the direction that might be taken in the future. On the one hand, he fears that the partition of Europe will be consolidated "as long as the peoples of the Russian Empire have not succeeded in overthrowing the bureaucracy which has brought a world revolution to a path of the most selfish conservatism" (II, 490). This view is consistent with the neo-Slavophile picture previously presented of Soviet autocracy. On the other hand, he writes:

For a while it looked as if the triumphant wind blew, rather, from America—the new Rome. But soon, from the dollar crisis to the murders of Martin Luther King and Robert Kennedy, in the midst of the profound division created in the public mind by the Vietnam drama, it became apparent that the country suffered from a major internal crisis, perhaps the gravest since the Civil War, and that anything, including the advent of some kind of fascism, was henceforth possible in America (II, 483).

Here, the references to Rome and fascism are introduced without much possibility of referring the reader back to preceding argument, which has not prepared him for such allusions.

Although the work suffers from deficiencies in its handling of the fourth dimension of history and of the three dimensions of the contemporary world, it is to be recommended as a worthy attempt to write an impartial account of the Cold War.

PAUL DUKES

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Origins of American Intervention in the First World War. By Ross Gregory. (W. W. Norton & Co., Inc., 1971. Pp. 157. \$6.95.)

Ross Gregory has written an exemplary essay on the American entry into World War I. Designed as supplementary reading in college courses, this work also repays careful reading by specialists. The author's background as a recent biographer of Walter Hines Page serves him well. He properly stresses the course of

Anglo-American relations against the general background of European conflict during 1914–1917. This analysis is truly synthetic, drawing upon all the extant schools of interpretation. Gregory gives careful attention to the economic entanglements with the Allies that developed after 1914 but correctly decides that these do not account for the act of April 2, 1917. He emphasizes but does not distort the roles of important figures such as Colonel House and Secretary Lansing. Like others in the mainstream of contemporary scholarship on World War I, he recognizes the president's realistic and practical bent, even as he points up Wilson's indisputable idealism. Wilson had no alternative to war in 1917, that is, if he wished to avoid an "unthinkable abandonment of rights and interests" (p. 130). Only a policy of complete isolation could have kept us out of war, and Wilson was in accord with popular feeling in refusing to retire entirely from international politics. The author wisely notes that considerations of national honor also played an important part, simply because of the nation's strength. "The more powerful the nation, the more the world expects of it and the more the nation expects of itself" (p. 135). American policy was "less a case of the man guiding affairs of the nation than the nation, and belligerent nations, guiding the affairs of the man" (p. 136). Wilson was free neither from domestic nor international pressures in reaching his decision.

I agree that Wilson was perfectly capable of staying out of the war, had he chosen that path. I would demur only in giving somewhat greater stress to the extraordinarily intense commitment Wilson made to bringing about a just and lasting peace. The failure of attempts to mediate the European struggle during 1915–1917 accompanied the president's growing obsession with controlling the postwar settlement as a means of disciplining not only the Central Powers but the Allied nations as well. As much as he hated war, he gave in to a stronger emotion, the desire to make a peace, and he finally came to believe that only by waging war could he force a peace of his own design. He fought so that, if necessary, he could dictate the terms of the treaties, and he succeeded. It cannot be said that he failed because of the way things went during the 1920s and after. The settlement that he imposed was never tried. The failure of the United States to participate in the treaty provisions fundamentally altered the character of the system that eventually materialized, so much so that it bore little resemblance to the one that Wilson had envisioned, even in the imperfect

form that he negotiated in Paris before the American defalcation.

Gregory offers a book that is judicious in scholarship and charmingly written as well. No one else need attempt to duplicate this essay for a considerable time to come. It is hoped Gregory will now turn to the course of events during 1917–1918, when the war was waged by the United States, a subject that still requires up-to-date synthesis.

DAVID F. TRASK

State University of New York at Stony Brook

Latin American Economic Integration and U.S. Policy. By Joseph Grunwald, Miguel S. Wionczek, and Martin Carnoy. (Washington, D.C.: Brookings Institution Press, 1972. Pp. 216. \$6.95.)

Industrialization in a Latin American Common Market. By Martin Carnoy. (an ECIEL Study, Washington, D.C.: Brookings Institution Press, 1972. Pp. 267. \$8.95.)

These companion studies represent a cogent plea to governing elites throughout Latin America as well as the United States to get on with the job of Latin American economic integration. Yet one cannot read them without wondering if even the authors themselves hold out much hope for significant progress toward the construction of a Latin American common market over the next decade.

In the first place, the authors are well aware that during the latter half of the 1960s the process of economic integration in Latin America was slowed almost to a halt. They trace this process, noting that only the subregional integration efforts within Central America and the Andean countries made any significant progress, and that the Central American Common Market itself entered a period of crisis and disintegration in 1968 which continues to the present moment. They conclude that "the road to a Latin American common market may well lead through such subregional arrangements as the Andean Group whose size and homogeneity make their integration easier than that of the highly diverse LAFTA membership" (*Latin American Economic Integration*, p. 61).

In the second place, the authors recognize the magnitude of the obstacles to the formation of a Latin American common market. Their awareness of difficulties which face further moves toward integration is revealed in their brief analysis of several major problems—including differing levels of industrial development; vested political and economic interests;

the attitudes toward integration of the military regimes in Brazil and Argentina; the relative indifference toward integration efforts on the part of Argentina, Brazil, and Mexico in consequence of the size of their domestic markets; a general fear of the potential predominance of U.S. multinational firms in a Latin American common market; and a significant, if ambivalent, desire among most Latin American countries to continue their independent bilateral relationships with the United States.

Against this admittedly pessimistic background both books seek to engage renewed support for the flagging integration effort. The ECIEL study does so by attempting to measure potential economic gains to be derived by Latin America from further integration. Specifically, it tries to determine the optimum location and size of plants in six selected industry groups within a regional common market in the target year 1975, and to measure the benefits to be gained from the integration of these industries. While stressing the uncertainties of the measurements attempted, the study concludes that the market expansion and product specialization accompanying the formation of a common market would offer significant opportunities for making Latin American producers of manufactured goods competitive in world markets, would free from 3 to 4 per cent of GDP each year to be used for further investment, and could benefit many Latin American countries which do not as yet possess large industrial sectors. One of the more interesting conclusions is that "whenever output moves to levels where economies of scale operate, as it would for many commodities in a Latin American common market, and transportation costs are significant, production costs would permit competition with developed countries even without tariff protection" (pp. 69-70).

While the message of the ECIEL study would seem to be directed at a Latin American audience, the message of *Latin American Integration and U.S. Policy* is clearly directed at Washington, D.C. The book begins by reviewing the now familiar history of Latin American economic development since the turn of the century, up to and including the moves toward regional integration in the 1960s. It then examines the U.S. view of integration efforts, a view which moved from initial indifference—if not hostility—to a gradual, though formalistic, embrace of the goal of Latin American integration by the middle and late 1960s.

The greater part of the book is devoted to a series of propositions which run approximately as follows:

(1) It is (or should be) a matter of significant U.S. national interest that Latin America experience rapid and "sound" economic growth;

(2) Such growth can be significantly aided by the formation of a Latin American common market;

(3) The initial costs of integration are too great for the less developed countries of the region to bear; and

(4) The United States should therefore support the integration effort by paying those costs which will otherwise seriously, if not fatally, affect further progress.

In elaborating the fourth proposition the authors call on the United States to reorient its Latin American foreign aid effort away from bilateral programs toward the support of regional projects and institutions which will entice Latin American governments into further integration. Such programs would include balance-of-payments support for countries experiencing difficulties during the transition to a common market, and adjustment assistance support for local industries hurt by the integration process. In all, the authors present a rather detailed blueprint for U.S. actions to reinvigorate the presently moribund Latin American commitment to the establishment of Common Market by 1980.

One suspects, however, that their blueprint, like so many others for Latin American unity, will never get beyond the drawing boards. In the first place, any relationship between the U.S. national interest and growth patterns in Latin America is at best highly tenuous. The decline in official U.S. aid to Latin America at least in part reflects that judgment, and the judgment is likely to become more pronounced as the concept of "U.S. national interest" is reconsidered in the post-Cold War international setting. In the 1970s more than ever, appeals such as the authors are making will stand or fall on humanitarian—not "interest"—grounds; and since the relationship between foreign aid and the development process is generally difficult to illustrate, even humanitarian appeals are likely to fall upon deaf ears.

In the second place, the proposition that Latin American integration can succeed if the United States pays all the insurance premiums is highly debatable. The authors explicitly recognize that the effort, in the long run, will depend upon the degree of Latin American commitment to integration, but that recognition often tends to disappear as the arguments concerning U.S. policy orientations are developed. A proper balance is struck in the conclusion that

Latin American nations must choose whether to make some sacrifices for more serious regional cooperation or, following the line of least resistance, to continue the autarchic policies that limit their development and deepen their dependence on developed countries. The United States can help in making a positive Latin American choice more feasible. Its offer of integration assistance, on a take-it-or-leave-it basis, will bring into the open Latin American development alternatives (p. 161).

For a host of reasons, some covered by the authors themselves and others developed in more theoretical approaches to the phenomena of regional integration, one suspects that at the LAFTA level, the Latin American choice will continue to be "the line of least resistance" for quite some time even if the authors' U.S. policy prescriptions were to be adopted.

ROGER D. HANSEN

Georgetown University

Studies in War and Peace. By Michael Howard. (New York: The Viking Press, 1971. Pp. 262. \$8.95.)

In an introduction to this collection, the author apologizes for reprinting essays and lectures that were produced originally as occasional pieces. The apology is entirely unnecessary. The book would be justified if it did no more than illustrate the development and broadening scope of Professor Howard's interest in military studies during the past fifteen years, which it does exceedingly well. But even the oldest of the pieces assembled here deserve to be reprinted for their own rather than their contextual merits, demonstrating as they do Mr. Howard's grasp of military theory and, when dealing with operational questions, his descriptive talents.

No one will complain, for instance, about the inclusion here of "Jomini and the Classical Tradition in Military Thought," a brilliant essay which appeared first in a *Festschrift* for Basil Liddell Hart. Here the great Swiss theorist's characteristic contributions to strategical thought are laid forth with clarity and economy, and those modern theorists who have fallen too completely under the influence of the German school are reminded that "in practice the influence of Jomini has been no less than that of Clausewitz, for it is in Jominian rather than in Clausewitzian terms that soldiers are taught to think" (p. 30). The essay on Waterloo which follows was written for the 150th anniversary of that decisive encounter; it remains the best brief account of the battle that this reviewer has ever read. As for three essays dealing with the evolution of military establish-

ments in the nineteenth century—on the decline of the Wellington system, on the reforms that gave European hegemony to Prussia in the wars of 1866 and 1870, and on Lord Haldane's institution of the Territorial Army on the eve of the first World War—they should be required reading for all military historians who aspire to be more than button-collectors or experts on forgotten battles.

Howard's interests in recent years, thanks in part to his association with the Institute of Strategic Studies in London, have turned increasingly to the study of international relations in the light of nuclear and missile technology, and this emphasis is reflected in the essays that fill the second half of this volume. The political and technological revolutions of our century have forced contemporary students of military affairs to grapple with problems unknown to their predecessors. "Strategic thinkers," Howard writes,

from the pioneers of the eighteenth century to Liddell Hart in his earlier writings, were able to assume a fairly simple model of international relations within which armed conflict might occur, as well as a basically stable technological environment. Neither assumption can now be made. No thinking about deterrence is likely to be of value unless it is based on a thorough understanding of 'the state of the art' in weapons technology. Any thinking about limited war, revolutionary war or indirect strategy must take as its starting point an understanding of the political—including the social and economic—context out of which these conflicts arise or are likely to arise (p. 183).

Howard pays tribute to American academic intellectuals of the 1950s—scholars like Bernard Brodie, Klaus Knorr, William W. Kaufmann, Albert Wohlstetter, Herman Kahn, Thomas Schelling, and Henry Kissinger—for having been pioneers in the new approaches to strategy. "Between them," he says, "they have done what Clausewitz and Mahan did in the last century, during times of no less bewildering political and technological change; laid down clear principles to guide the men who have to take decisions" (pp. 174f.).

But they have not, of course, answered all of the questions that trouble reflective men. "In most of the societies known to history," Howard writes, "war has been an established and usually rather enjoyable social rite" (p. 204). In the nuclear age, it is difficult to think of it in those terms. Even the persuasive argument that force is an inescapable factor in international relations and that one must therefore learn its forms so that it may be used, effectively and with restraint, for legitimate purposes leaves

the awkward questions dangling: *Whose* legitimate purposes? Does the legitimacy of authority legitimize the force it uses, or must we apply other criteria? Are there ethical considerations that should limit the use of violence, as many men throughout history have believed, without knowing how to define them?

These issues Mr. Howard discusses in a study called "Morality and Force in International Politics" in which he concludes, in effect, that it would be nice if we could assume that, if only statesmen were conscientious enough, they would always find ways to make moral and pragmatic considerations parallel each other rather than conflicting; but that neither history nor the Scriptures give us any reason to do so. The responsible statesman, as Bismarck once remarked to a censorious clergyman, does not have to be reminded about sin. "Anyone who reproaches me for being a politician without conscience . . . should sometime try out his own conscience on *this* battlefield."

GORDON A. CRAIG

Stanford University

Psychological Dimensions of U.S.-Japanese Relations. By Hiroshi Kitamura. (Cambridge, Mass.: Harvard University Center for International Affairs, 1971. Pp. 46. \$1.75, paper.)

This short monograph, containing a number of provocative insights but remarkably uneven in quality, may be seen as something of a watershed in studies of Japanese-American relations. Written in the months immediately prior to the announcement by President Nixon of his impending visit to Peking and the end of the post-1945 international monetary system, it embodies the narrowly bilateral focus and rather pollyanna-like assumptions that characterized the bulk of the American and much of the Japanese scholarship in this field in the postwar era. Moreover, the "area specialist" approach employed, discursive, highly factual, and analytically loose, succeeds in illuminating important aspects of the subject but lacks the rigor and clarity to which social science scholarship aspires. The sudden uncertainties and expanded importance of Japan has already led the more aggressive policy-oriented scholars (without special knowledge of the country) into the field, and it is certain that monographs like this one will in the future compete with more "professional" but not necessarily more insightful works.

Kitamura's monograph makes two main contributions: (1) an emphasis on the fundamental cultural differences between the two countries, as manifested in the Japanese perception

of the international system and (2) an inventory of relations since 1945 from this "psychological" perspective. Because of its remarkable cultural homogeneity, Japan is one of the few nations in which a "national character" study of this sort is useful. As a corrective to works focusing exclusively on the economic, political, or strategic dimensions of relations between the two countries, the book has considerable merit, for the cultural-psychological dimension of interaction is of unique importance in Japanese-American relations.

Unfortunately, the impact of the argument is substantially diluted by fuzzy conceptualization and a tendency to overgeneralize. Although it is assumed that there is a basic compatibility between Japanese and American "interests" and that harmonious relations will result simply from proper communication, "interests" are left undefined, and no consistent distinction is made between real and perceived interests. The term "psychological factors" is not carefully defined, and in the end the author ends up psychologizing politics (e.g., on pp. 6-7, bilateral economic conflicts are essentially seen as the result of "psychological factors"). In dealing with Japanese opinion, no effort is made to distinguish between "elite" and mass opinion and even to confront how these opinions are distilled into policy through the political process. Finally, evidence ranging from sweeping historical generalizations to data from opinion polls and casual observations of literati is cited without any real discrimination. The result is a kind of smorgasbord of unconnected comments which baffles as much as clarifies.

The monograph deals with an important subject from a fresh perspective. While it has intrinsic merit and the author is obviously sensitive and knowledgeable about Japanese foreign affairs, it is rather surprising that a greater effort was not made to incorporate more fully into this work theories and concepts from psychology and politics.

DONALD C. HELLMANN

University of Washington

Seeking World Order: The United States and International Organization to 1920. By Walter F. Kuehl. (Nashville: Vanderbilt University Press, 1969. Pp. 385. \$8.95.)

A work primarily of interest to specialists, Professor Kuehl's book traces the development of ideas about international organization in the United States up to 1920, concluding with an assessment of the role played by the American "internationalists" and publicists in the debate over the League of Nations. As intellectual

rather than political history, the book exhibits the weaknesses and strengths of this genre. On the plus side, Kuehl records the wide diversity of ideas, opinions, and organizations which worked for the cause. The result is a clear picture of the confusion of aims, commitment to over-rationalistic and legalistic conceptions of the problems involved, as well as the role the publicists had in creating a somewhat auspicious climate for Wilson's Covenant while eventually helping to doom it to defeat because of their disagreements with Wilson and among themselves.

On the negative side, the book's organization makes the reader's job quite tedious. The strict emphasis on chronology has the disconcerting effect of intruding descriptions of minor publicists of little importance into the narrative, apparently solely to insure comprehensiveness and chronological neatness. Since the book deals mainly with ideas, there is little attempt to assess carefully the influence of various groups and individuals. No careful distinction is made between their impact on the general public as opposed to that on policy-influencing elites. Nor is there any useful discussion of the social and political groups involved or how they relate to each other. All we have is a bare-bones account of the organizations and brief biographies of individuals. Admittedly the author set out to chronicle ideas, not politics, but without some more careful attempt at analyzing impact—admittedly difficult without survey data—we are left in a welter of confusion and profusion of names, dates, and concepts. The book comes to life only in the all too rare excursions into the political dynamics of the period.

Yet even here the result is curious. For example, Kuehl concludes that one reason for Wilson's failure to sell the League was his ignorance of and inattention to the work of these internationalists in developing ideas about international organization. Thus he presents a paradoxical criticism of Wilson as unrealistic because he did not take sufficient notice of these people's work despite his implicit—and explicit in a couple of instances—assessment of these forces as being without significant political influence. Perhaps it is the overemphasis on the ideas and arguments of the publicists that leads Kuehl to give greater weight to the effects of Wilson's disdain for this group than is merited. This leads to a more general point; this kind of intellectual history is to some degree dysfunctional because it makes for poor political history.

JAMES S. MAGEE

Eastern Michigan University

Japanese Foreign Policy on the Eve of the Pacific War: A Soviet View. By Leonid N. Kutakov. (Tallahassee: The Diplomatic Press, Inc., 1972. Pp. 241. \$15.00.)

Dr. Kutakov is a distinguished diplomatic historian as well as one of the Soviet Union's most experienced senior diplomats in the Asian region. He served in China during 1955–57, certainly a crucial period in Sino-Russian relations, and in Japan during 1959–60. After serving as a senior member of the Permanent Mission of the USSR to the United Nations, he joined the UN Secretariat as Under-Secretary-General for Political and Security Council Affairs. Dr. Kutakov brings both academic expertise and professional experience (though more of the former) to his analysis. Both he and George A. Lensen, who edited the translated manuscript, are to be congratulated for making available a Russian perspective on the policies of Japan in the latter half of the 'thirties and first half of the 'forties.

Dr. Kutakov divides his study into four substantive chapters and one retrospective epilogue. In the first, he delineates the policies that led Japan into an alliance with Hitler's Germany and Mussolini's Italy. It is, in broad outline, the familiar story of the background and negotiations leading up to the Japanese-German "Anti-Comintern Pact" and, ultimately, the Tripartite Axis Alliance. Most of the details, at least according to the footnotes, are taken from documents of the International Military Tribunal for the Far East (I.M.T.F.E., i.e., the Tokyo "War Crimes" trials) which Soviet scholars refer to as collection 436b of the Foreign Policy Archives of the Ministry for Foreign Affairs of the USSR. Because it is Japanese archives which were brought to light by the I.M.T.F.E. it is extremely difficult to determine how Russian foreign policy makers in the 1930s were perceiving Japanese foreign policy. One can only regret the absence of any reference to Soviet Foreign Ministry archives of the 1930s.

In the second chapter, on Japan and England, the primary source is *Documents on British Foreign Policy 1919–1939*, published by the British Foreign Office in 1955. Much of this section is devoted to the proposition that the British were intent upon appeasing the Japanese with a Far Eastern version of the concessions made at Munich for Europe. This interpretation hinges on the Arita-Craigie Agreement of July 23, 1939, by which Britain acquiesced in Japan's territorial seizures from China. "The events cited above show that the Arita-Craigie agreement had not been forced on the British

government by the aggravated situation in Europe in the summer of 1939 as asserted by bourgeois historians [the footnote cites Professor E. L. Woodward], but was a manifestation of the time-honored strategy of directing German and Japanese aggression against Russia" (p. 126).

For the third, which focuses on Japan's relations with the Soviet Union, the author does refer to some of the material available in the archives of the Soviet Union's Ministry of Foreign Affairs. Much of it relates to the endless friction over fishing rights. There is regrettably all too little discussion of the origins (from the Soviet perspective) of the Neutrality Pact negotiated by Foreign Ministers Matsuoka and Molotov in the spring of 1941. By contrast with the motivations behind the Arita-Craigie Agreement, "the Soviet Union signed the Neutrality Pact in an attempt to preserve peace in the Far East . . ." (p. 193).

The fourth chapter, which concerns the collision between Japan and the United States, emphasizes "the long-standing rivalry of the imperialist powers [i.e., the U.S. and Japan, which] had grown ever more acute" (pp. 200-201). This judgment is followed by a kind of classic, if unfootnoted, set of assertions concerning the motivations behind American foreign policy.

American capitalists could not reconcile themselves to the gradual subordination by Japan of China, a potentially vast market for the United States, nor could they agree to Japanese seizure of Southeast Asia, a major source of strategic raw materials. But while some American leaders realized that war with Japan was inevitable, others linked with business concerns that had lucrative dealings with Japan, tried to avoid a collision by appeasing Japan (p. 201).

This volume is probably far more valuable to American readers for the insight into Russian research that it provides than as an analysis of how Japanese-American relations of that era ended in the explosion of Pearl Harbor.

In the epilogue, Dr. Kutakov sets forth the lessons that are to be learned from a study of Japanese foreign policy. Basically it "shows the cost and futility of resorting to force in international relations" (p. 230). Additionally, a brief summary of the war years is recounted. Interestingly enough, the only reference to the Soviet Union's denunciation of its Neutrality Pact with Japan in April 1945 is that it was a "heavy blow for Japanese diplomacy, which had hoped to take advantage of the contradictions existing among the allied powers to secure a peace which would leave Japan in possession of some of the territories she had conquered"

(p. 224-225). Possibly so, but what about the Soviet Union's leaders' motivations? Nothing is written.

Withal, this book does make for interesting reading, and not only as a Russian view of Japanese foreign policy. It also provides a different set of hypotheses on the origins of what the Japanese call "the Pacific War." Finally, by indirection, it gives some clues to the well-springs of the foreign policy of the USSR.

HANS H. BAERWALD

University of California, Los Angeles

Jungle Alliance: Japan and the Indian National Army. By Joyce C. Lebra. (Singapore: Asia Pacific Press, 1971. Pp. 255. \$8.00.)

This book adds another segment to the gradually unfolding story of Japan's wartime relations with Asian nationalist movements. There are certain unique elements in the case of the Indian National Army, the subject at hand. India was peripheral to Japanese concerns and its conquest and inclusion within the Co-prosperity Sphere were not part of Japanese overall strategy. Moreover, there was no Japanese occupation of Indian territory (one can hardly count the Andaman and Nicobar Islands). Consequently, as Dr. Lebra points out, there tended to be fewer direct confrontations between the Japanese and the nationalist leadership.

At the same time, certain general characteristics of the relationships in occupied areas do occur in this instance as well. For the Japanese, the requirements of the war had to take precedence; their interest in Indian independence was primarily for propaganda purposes. For the Indian, as for other nationalists, independence came first. The difference in priorities inevitably gave rise to misunderstanding, tension, and occasional conflict. Dr. Lebra's contention that Japanese policy granted the form but not the substance of independence (the Andaman and Nicobar Islands remained under Japanese Navy administration even after a formal transfer of sovereignty) is one that is valid for occupied areas as well.

In this, as in respect to other nationalist movements, the Japanese were able to secure the cooperation of a nationalist leadership with some stature. True, they were slow to recognize the potential value of Subhas Chandra Bose and to bring him back from Berlin. Prior to his return, their attempt to work through Rash Behari Bose, a long-time resident of Japan, was not particularly successful. To the Indians he was too much the spokesman for Japanese interests and too removed from current national-

ist concerns. S. C. Bose, on the other hand, was someone who fitted readily into the leadership role, who could inspire confidence and unite divergent interests. The force of his personality affected Japanese as well as Indians. In Dr. Lebra's view Bose's impact on Tōjō unquestionably gave INA interests a better hearing in Tokyo, for prior to their meeting, Tōjō had been generally unimpressed with the INA and its potentialities. It was a tribute to Bose's abilities that he was able to bargain as effectively as he did from what was an inherently weak position.

Among the most interesting parts of this book are the glimpses provided of the personal views of several of the participants. It is abundantly clear that there was no such thing as the Japanese "military point of view" in respect to the INA. (Nor was there one on the general question of the role of nationalist movements.) The variation in views was not just a matter of where one stood in the chain of command or of whether one's perspective was that of Tokyo or of the field of operations; individual differences existed. The change in Tōjō's evaluation of the INA after his encounter with Bose was mentioned above. General Kawabe, Burma army commander, became emotionally involved with the INA. On the other hand, Field Marshal Terauchi, commanding the Southern Area Army, was less sympathetic and reflected the imperious attitudes associated with the stereotyped Japanese officer.

The most appealing figure, on the Japanese side, and the person who dominates the early part of this story is Major (later Lieutenant General) Fujiwara Iwaichi, the head of the *F Kikan*, an intelligence unit operating in the Malay and Chinese as well as Indian communities in Southeast Asia. Highly idealistic, perhaps even a bit naïve, imbued with the idea that the cause of Japan was also the cause of Indian independence, and firmly believing that to inspire trust one had to demonstrate it, Fujiwara played a key role in the inauguration of the INA. The bond of personal trust between Mohan Singh and himself lent a touch of romantic idealism to the initial period before the inevitable rush of events overtook both of them and brought significant changes in the relationships on both the Japanese and Indian sides. Had there been more Fujiwaras in the Japanese military, the ultimate fate of the INA and other nationalist movements would doubtless have been little different. The image of Japan in postwar Asia, however, might have some softer tones.

In respect to this last point, Fujiwara was surprised at the scarcity of information about

India available in Japan, inside or outside the military. In spite of talk of the common tie of Buddhism, in spite of Tagore and Okakura Kakuzō, a wide gap separated India and Japan. In the postwar world, the quantity of information is greater, but the gap would not appear to have narrowed appreciably.

Dr. Lebra has produced a very fine book. Well written, with a generally good balance between the broad scope of events and personal viewpoints, it is eminently readable. Her wide-ranging research in a variety of sources, including interviews, affords the sort of multidimensional approach which this story requires.

WILLARD H. ELSBREE

Ohio University

Khrushchev and Kennedy in Retrospect. By James E. McSherry. (Palo Alto, Calif.: The Open Door Press, 1971. Pp. 233. \$8.95.)

The Kennedy Doctrine. By Louise Fitzsimons. (New York: Random House, 1972. Pp. 275. \$7.95.)

"Why Suffer for Saigon—or Die for Danzig?" The phrase alone (the title of Mr. McSherry's concluding chapter) should be enough to keep the best intentioned reader away from his book. Its author—a former intelligence officer—still lives in the unquestioned age of brinkmanship: the future of world peace, in his view, remains determined by the balance of America's account in "the Bank of Soviet Respect." As examples of substantial deposits which forced Soviet leaders to revise their judgments of America's will are Truman's decision on Korea, Eisenhower's dispatch of troops to Lebanon in 1958, the U-2 incident when Washington said in effect "Yes, they're ours. And just what are you going to do about it?" (pp. 53-4), down to Vietnam where Johnson's action in sending combat units is seen as being directly responsible for Khrushchev's downfall and his successor's restraint (p. 199). It is a strange reading of history to which we are submitted, one which continues to make the old Cold-War distinction between the Russian people and the Soviet regime, and a no less disturbing reading of the future as well, for which "liberal sheepish politicians" who persist in bleating their appeasing slogans within earshot of the Soviet wolves are in advance held responsible for the extinction of "the Common Man" (p. 201).

"Extensive, unilateral concessions on the part of Washington," the author writes, "will inevitably put the United States in the position in which France found herself in August 1939"

(p. 193). A reading of Mr. McSherry's book, and a review of the postwar period which is dealt with in this book, leave one unimpressed by America's "extensive" and "unilateral concessions." Nor is this notion of inevitability in the recurrence of historical situations easily acceptable either. Indeed, how much longer will allegedly serious men continue to live the present in the light of a misconstrued and misused past? Understandably, the guidelines which security policies follow are bound to be anchored in the past. But whether for the statesman who defines those guidelines, or for the observer who "explains" them, it is still useful to comprehend that past first and apply it correctly next. Thus there was not as much continuity in the appeasement of Hitler as Mr. McSherry appears to assume. The show of force by the Czech government in May 1938 is a case in point. To be sure, such action forced Hitler's diplomatic retreat. But it also prompted him to cancel a previous order that had ruled out any kind of military action against the embattled democracy. Coercive diplomacy and appeasement come together, and between May and September 1938 the roles were reversed. Of course, the other protagonists, namely the Western democracies, did preserve an appeasing attitude throughout. But the case against such a policy rests essentially on the subsequent discovery that Germany was not prepared for war in the fall of 1938 and that Hitler's ambitions were unlimited. Unfortunately, such information was not recorded at the time.

Furthermore, the application of this particular historical episode to the postwar situation leads the author to assumptions which are quite debatable. For one, it assumes that the objectives of the Soviet leaders too were unlimited—a notion which most scholars now reject as unfounded, on the basis of a research which goes far beyond Mr. McSherry's preferred sources, namely, the *Pravda* and *The New York Times*. Nor can postwar Soviet weakness be seriously compared to prewar German insufficiency. Nor, again, did the U.S. appease all that much. From Truman to Nixon one wonders, in fact, how much more we could have coerced, "For its part" writes McSherry, "the United States should respect legitimate Russian (not necessarily Soviet) national interests" (p. 193). One wonders how such recognition of Russian interests (however minimal) is reconciled with the author's recommendation that Washington maintain an "unbending attitude" toward the Soviet regime.

Much more commendable is Louise Fitzsimons' book, *The Kennedy Doctrine*. The

theme here is simple and, by now, is gaining increasing recognition. "Kennedy's policies were as dominated by cold-war thinking as were those of his predecessors" (p. 8); even more so in fact, since "American foreign policy before 1961 was essentially a reactive effort" (p. 7), whereas it became an aggressive one in the sixties. To Fitzsimons, as to so many of Kennedy's critics, the admission of the President's failure is a sad one. He failed, we are told somewhat regretfully, to understand and use "his own potential for leadership and accomplishment" (p. 243). He should have done and could have done more. As it was, his was "a counterrevolutionary and counterinsurgency policy" (p. 8) which failed to deliver on the many promises pledged by an otherwise brilliant rhetoric.

Thus the account of Kennedy's foreign policy takes us through a familiar journey: the Bay of Pigs, regarded by the author as Kennedy's "greatest mistake"—a "colossal error" from which "consciously or unconsciously (he) spent the rest of his administration and his life trying to recover" (pp. 18, 70); Khrushchev's manhandling of Kennedy at Vienna, and Kennedy's "extravagant" response and subsequent determination to reassert America's "role of leader in freedom's cause" (p. 81); the Cuban missile crisis when the American president "risked the destruction of all life on earth," a crisis which could have been avoided, Fitzsimons suggests, had Kennedy shown a greater willingness to pursue a diplomatic route at a time when the "survival of the republic" was not directly threatened (pp. 126, 170-2); and, finally, the "fatal illusion" of counterinsurgency war, especially as it was applied in Southeast Asia, and the escalation of the nuclear arms race at a time when "in the opinion of many knowledgeable observers, within the government and without, there was in early 1961 an opportunity to halt" it (p. 215).

However appealing, and in part true it is, the author's thesis appears to be confined essentially to a time period which starts too late and ends too early. It starts too late because it does not take sufficiently into account the legacy of the Eisenhower administration. Thus, by January 1961, an impressive number of key problems had been accumulated by the retiring president for the incoming one; a credibility gap in the U.S.-Soviet relations, following the U-2 incident; the Laos affair for which an invasion involving U.S. troops had been planned; the invasion of Cuba; postponement of a decision on the question of ABM deployment; relations with the Atlantic Alliance and, more spe-

cifically, relations with de Gaulle, whose September 1958 memorandum had remained unanswered—to cite but a few. Most of these issues matured in the spring of 1961. To be sure, as the author reminds us, “the Presidency of the United States provides little room for on-the-job training” and we should expect from the president “a mature and balanced judgment from the day he enters into office” (p. 69). Still, in the spring of 1961, whose foreign policy was it, Eisenhower’s or Kennedy’s? Can any one man “change” foreign policy single-handed in a matter of a few weeks? If he cannot, given the circumstances which prevailed at the time, is one justified in alluding disdainfully to Kennedy’s cold war policy, or even to such a thing as a Kennedy doctrine? For that matter, assuming that there is such a doctrine, can one retract it, uninterrupted, during the full Thousand Days? I, for one, doubt it. Occasionally, the author recognizes a “capacity for growth” in the late president (p. 69). It is this capacity which made him ultimately understand the true nature of his own potential for leadership. One may agree that much that happened before and during the Cuban crisis of October 1962 was unfortunate, to say the least. But one may at the same time recognize that following the crisis, Kennedy did look for new options as he began at last to think in terms of a new perspective, less imperial and more conciliatory. The evidence appears here and there—in the Test Ban Treaty, in the American University Speech of June 1963, in some of his latest statements on China, Vietnam, and the rest of Asia. It is not just a matter of iffy history either. Kennedy’s presidency was just too short to warrant passing much judgment on it.

SIMON SERFATY

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Russia and China: Their Diplomatic Relations to 1728. By Mark Mancall. (Cambridge, Mass.: Harvard University Press, 1971. Pp. 396. \$12.00.)

Together with Clifford Foust’s monograph on Russo-Chinese trade relations from 1725 to 1800 (*Muscovite and Mandarin* [Chapel Hill: University of North Carolina Press, 1969]), Mancall’s book on the beginnings of diplomatic relations between the two countries provides for the first time a sound scholarly analysis of the initial stages of such relations.

The background of the two crucial treaties, Nerchinsk (1689) and Kyakhta (1727), is explored from the time of the first officially authorized mission, Petlin’s (1618), to the Golo-

vin mission which negotiated the Nerchinsk treaty and finally that of Sava Raguzinskii-Vladislavich which concluded the Kyakhta treaty. The initiative was the Russians’ throughout, resulting from the conquest of Siberia and the penetration of the Amur region by outlaw, unofficial, and official Russians in turn. Both Chinese and Russian materials have been investigated to produce a close study of the purposes and results of each successive mission.

Mancall’s interpretative passages are interesting. He compares the Chinese “tribute system” (inherited by the Manchus) with “new ideas introduced into the traditional East Asian world by the Russians” (p. 8). His explanation of the difference between how the Ch’ing government treated the nomadic northwest and how it treated the agricultural southeast is clear and helpful, though the ease with which Russia was assimilated to the nomadic northwest in Chinese diplomacy might have warranted comment. Moscow, we are told, represented a European tradition not yet “universalized through imperialism and colonialism,” a tradition which “insisted on the equality of nation-states and sovereigns” while the Ch’ing system “recognized the existence of only one civilization . . .” (p. 65). Actually European countries had since the outset of the Age of Discovery dealt with many petty chiefs and principalities the world over, by no means as equals, whereas China’s exclusiveness and rhetorical pretensions to be the whole world had not prevented intercourse with foreigners. Mancall’s painstaking account of events does not in fact seem to suggest that differing world views impeded early Sino-Russian relations very much. Unauthorized or inept moves were made by both sides; but rational adjustments of conflicts and arrangements for trade and diplomacy were worked out with remarkably little fuss. The author is quite right to contrast the post-1840 shock to earlier Chinese ways of conducting foreign relations with the period of his inquiry. What seems worthy of note, as his book shows, is how well the pre-1840 system worked.

The author has striven for precision. In fact, he overdoes it a bit; for example, he translates Sava Raguzinskii-Vladislavich’s name in the text of the Treaty of Kyakhta fifteen times as “the Illyrian Count Sava Vladislavich” to avoid rendering “Raguzinskii” as a surname, and he refuses to use “Spathary” (Spafary) as the name of Miclescu, because “Raguzinskii” means “Ragusan” and “Spathary” means a “spatar”—an official title; but the ordinary reference book lists the two men under “R” and “S” respectively. The notion of “surname” was not a fixed

one in seventeenth-century Russia anyway, and one is reminded of those cataloguers who cross out "Lenin" and write "Ulianov" on book cards. There are a few minor errors: Križanić was a Croat not a Serb; Nurhaci was not in fact an "early Ch'ing emperor"; in 1689 Peter the Great was only technically "on the throne," with the actual rule in other hands; *deti boiarskie* were not "boyar sons" but lesser boyars; there is no such place as "Vyatsk" (Vyatka?) or "Yarench" (Yarensk); either "Buchholz" or "Bukhholts" (if soft sign is omitted) will do for a Russianized German name but "Bukhholts" will not. The K'ang-hsi emperor's remark that Peter I exposed himself too frequently to ocean voyages is called "not surprising" coming from a continental monarch, but it is indeed surprising because Peter was also a continental monarch who never took any ocean voyages at all. Paisios Ligarides was scarcely a "representative of the Greeks in Russia," but a renegade cleric looking out for his own interests who was titular metropolitan of Gaza while he was in Russia. Milescu's report that the Peking Jesuit Verbiest was more loyal to the Russian tsar than to the Ch'ing emperor deserves a skeptical comment. Russian transliteration is something of a problem to the author. He uses neither the standard British, the standard American, nor the so-called international system; what he does use is not open to serious question except for "sch" (instead of "shch") which is German, not Russian. But within his system there are too many mistakes, at least some of which an editor ought to have caught. There are also errors in French and German, though romanizations from Chinese seem almost perfect, and there is an admirable glossary of characters. There were formidable linguistic problems in the preparation of the manuscript, but three errors in three languages on p. 361, for example, are not what one expects of a major press.

Despite such minor faults, the book reflects commendable documentary research and objective, critical analysis throughout. Even if all questions regarding the inception of Sino-Russian relations are not disposed of, they have received a thorough and fair examination. Few people anywhere in the world can bring absolutely all of the desirable skills and knowledge to the study of such a topic.

DONALD W. TREADGOLD

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The Asian Engineering Brain Drain. John R. Niland. (Lexington, Mass.: Heath Lexington Books, 1970. Pp. 182. \$12.50.)

Concern over the international "brain drain"

from European countries became intense during the mid-1960s, but subsided after 1968. Recently, however, another current in the problem has surfaced, namely the brain drain from the less developed countries to the more developed ones.

This kind of drain has been widely regarded as hampering economic development in the countries of emigration, and with amounting to "exploitation" of the latter by the receiving countries, and more specifically with imposing "reverse foreign aid" which offsets the direct aid contributions. These new problems are not so susceptible as before to resolution by the theory of international comparative advantage and re-equilibrating movements in a worldwide free market. Then the Soviet Union in 1972 gave traumatic expression to the issue by imposing an exit tax on a sliding scale according to the extent of education received.

These new concerns have produced another spate of publications and debates. Currently the National Science Foundation is conducting a large questionnaire study on foreign scientists and engineers in the U.S. At the United Nations, two major studies have been launched: one by UNITAR on the *causes* of the migration from the less to the more developed countries; the other by the Social Development Division, under the direction of this reviewer, on the economic *effects* of the migration upon the countries at both ends.

Professor Niland's book falls in the new area of concern, but his title is too broad: he actually deals only with the drain of Asian engineering students who enroll in U.S. graduate schools and then postpone or avoid returning home. He makes only passing references to the persons immigrating into the U.S. as already trained professionals—although according to his own estimates they comprised 40 per cent of the engineers and scientists draining from the less developed countries, 80 per cent of the physicians, and nearly the whole of the brain drain from the more developed nations (and more since 1967, Niland's base).

The core of his book consists of the replies obtained from a questionnaire which Niland circulated among a sample of the Asian graduate students in engineering already mentioned. What he obtained is a description of their present traits (e.g., skills, previous education, sources of support, work experience, etc.) and their locational intentions, but not the record of their actual movements.

From the outset, Niland assumes that nonreturning students are a serious drain on their home country; but he never really demonstrates

this main point. First, he strains to establish extremely high percentages of "nonreturn"; but to accomplish this he must narrow the base from "all foreign students" to just those students who in any year are "in position to seek conversion of their visas" from student status to immigrant status. Second, when Niland deals with the direct costs of the foreign students to their home countries, he finds only the rather small costs of overseas transportation and of the first six months of U.S. residence—since the replies to his questionnaire showed that most of the foreign students quickly attached themselves to U.S. sources of support!

Trying to cast a wider net, Niland then proposes a kind of opportunity cost: the losses to the home country from early return, owing to the fact that younger professionals earn less, and bring home less accumulated savings, than if they had stayed abroad longer. On this basis he even advocates deferred return! But this feature can hardly be taken as the principal "cost of emigration," as compared with lost contributions to the national product. And Niland concedes that those who stay abroad a long time may indeed never return.

The book does provide some new and useful figures on the sources of support for these engineering students, and on their accumulation of savings. It is regrettable that Niland did not publish the replies on remittances and some other important items on his questionnaire.

Niland barely recognizes the dimension of loss from nonreturn in terms of the ratio to the home stock of professional manpower and the extent of home vacancies—although clearly the "loss" will be small or nil for a country like the Philippines, which already shows a surplus in certain professions. Finally, the author never compares the losses ascribed to nonreturning students with the losses associated with emigrants previously trained in the home country. It is the latter category which poses the urgent problem for the countries involved in the professional migration at both ends. Studies now in progress indicate the likelihood of a "negative-sum game" here: relatively heavy costs to some weak and poor countries of emigration, in direct outlays lost and occasionally production foregone, and on the other hand, only relatively small gains accruing to the large and rich economies of most of the receiving nations.

EDWIN P. REUBENS

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The Politics of Foreign Aid in the Brazilian Northeast. By Riordan Roett. (Nashville:

Vanderbilt University Press, 1972. Pp. 202. \$8.95.)

Roett's *The Politics of Foreign Aid in the Brazilian Northeast* describes how the emergence of political forces challenging the traditional oligarchies in the Northeast during the 1950s and early 1960s and the energy and genius of Celso Furtado led to the creation of the Superintendency for the Development of the Northeast (SUDENE), a new federal agency to plan, coordinate and supervise the development of the region. The author argues that the SUDENE's concern with economic and social injustices, independence from the traditional power structure, and promise of rational planning and honest project implementation offered the United States an ideal opportunity to support a movement consistent with the objectives and promises of the Alliance for Progress. Professor Roett demonstrates, however, that despite the pronouncements from the highest levels of the United States government supporting social, political and economic reform and the SUDENE as a promising institution for effecting such reform, the implementation of the foreign aid program between 1962 and 1964 actually served to undermine the SUDENE's position and support the traditional oligarchies opposed to reform.

This book provides a valuable description of the political factors surrounding the creation of the SUDENE and the United States role in the Northeast from 1962 to 1964. Unfortunately, the rather narrow focus on political reform has led to conclusions which overstate both the SUDENE's internal mandate and capabilities for reform and the reactionary impact of the U.S. AID program.

While the weaknesses of the SUDENE are given little weight in the conclusions, they do emerge from the book. The SUDENE appears as an agency which never had a mandate for effective reform, avoided the areas most essential to major reform, lacked the power to discipline or force compliance with its program, and had no proven ability to plan and implement programs that could successfully utilize foreign capital or personnel. Dr. Roett never reconciles SUDENE's internal weaknesses with his conclusions that the SUDENE was a radical reform effort that was undermined primarily because of the lack of U.S. AID cooperation, or that the U.S. AID's by-passing of the SUDENE was motivated by and designed to prevent the spread of communism. For example, it seems as plausible that the U.S. AID by-passed the SUDENE because of a belief that it was unwise to base an entire program on one techni-

cally and politically weak agency or that the SUDENE Master Plan was not the best approach to reform and development in the region. It might be argued that the U.S. AID's emphasis on education and health, areas which the author explains were outside the expertise and influence of the SUDENE (p. 47), are as important to long-run economic and social reform in the Northeast as the emphasis of the SUDENE program. It is not clear when or how the illiterate, malnourished, disease-ridden masses were to benefit from the SUDENE's emphasis on infrastructure, basic industry, and colonization projects that would never affect more than a tiny fraction of the people. The author's implication that the AID should have cooperated fully by turning over all funds to the SUDENE with virtually no strings attached appears warranted only if the AID's sole objective were to promote political reform.

The lesson I see from the SUDENE experience is that real reform in the Northeast will come only after there is a much stronger, long-term commitment for such reform at the highest level of Brazil's government. And when that date comes, Brazilian nationalism ensures that the United States' role, either as a deterrent or a catalyst for reform, will be marginal at best.

My downplaying of the role of the United States in the SUDENE experiment should not be interpreted as a whitewashing of the U.S. AID program in Brazil, which throughout the 1960s belied any serious commitment to the reform objectives of the Alliance for Progress. A central question raised by this book is how to explain the gap between the goals of the Alliance for Progress and the actual implementation of the United States foreign assistance program. The author's discussion of the time pressures imposed on the Northeast mission to produce a program with short-term results touches on a major cause of this gap. Roett implies that these pressures derived primarily from the desire to launch a highly visible program to stem the growing Communist influence in the northeast. The pressure to demonstrate immediate results, however, and the tendency of the AID program to support the traditional economic, political, and social forces within Brazil remained long after the 1964 military coup eliminated any serious Communist threat. In fact, as long as foreign assistance must be justified annually and bureaucrats rewarded on the basis of the funds moved, the U.S. AID programs will probably continue to reflect these characteristics.

Programs sensitive to economic, political or social reform generally require putting re-

sources into channels where the risks are higher and the results longer-range and more difficult to quantify. The U.S. AID has placed a high priority on minimizing the complications and risks of transferring resources. Problems tend to be viewed in such terms as crop yields and the number of agronomists, while issues such as income distribution, land tenure, and social and political reform have received only occasional lip service. Economists have been valued for justifying decisions already made by mission executives; other social scientists have been conspicuously absent from mission staffs.

Dr. Roett's book should enhance our understanding and awareness of the political implications of United States foreign assistance. Unfortunately, analyses such as Dr. Roett's, which question the simplistic, conservative approach of the U.S. AID, are likely to be ignored in the absence of a major restructuring of the entire foreign assistance program.

KENNETH D. FREDERICK

Resources for the Future, Inc.

A History of O.P.E.C. By Fuad Rouhani. (New York: Praeger Publishers, 1971. Pp. 281. \$17.50.)

In 1960 five major petroleum exporting states—Iran, Iraq, Kuwait, Saudi Arabia, and Venezuela—formed the Organization of Petroleum Exporting Countries (O.P.E.C.). Fuad Rouhani, the Organization's first Secretary General, gives a circumspect but clear description of O.P.E.C. in the last part of his book, and an excessively lengthy general discussion of the international petroleum industry in the opening hundred and fifty pages. He does not try to specify how much O.P.E.C., as opposed to other factors, has affected the bargaining power of the producing states.

For Rouhani "... the greatest value of the Organization to its members lies in its being a meeting place for regular consultations" (157). Despite a proclivity of the press and even members of the petroleum industry to talk about O.P.E.C. as if it were a genuine supranational entity Rouhani's position is correct. The Organization has a modest budget, which amounted in 1971 to only \$1.2 million. Its substantive actions are resolutions requiring unanimous consent addressed to its members or the operating companies. There are no provisions for sanctions against members that ignore resolutions. Any significance the Organization has must be attributed to facilitating the exchange of information and coordinating expectations.

O.P.E.C. has played an explicit role in two important developments since its founding:

halting a drop in posted prices and changing the tax treatment of royalties. In the late 1950s the major petroleum companies began to reduce the posted price used to calculate payments to producing countries. The prospect of falling per-barrel earnings led to the creation of O.P.E.C. In the mid-1960s the companies agreed to hold posted prices above levels prevailing at the end of the previous decade. Before 1966 Middle East countries allowed companies to write off royalty payments against their income taxes. In 1962 O.P.E.C. passed a resolution recommending that royalties be treated as a business expense deducted from earnings and not subtracted from tax payments. By 1971 the companies were operating in accordance with this resolution.

What is not clear from Rouhani's treatment is whether the existence of O.P.E.C. affected these developments. He notes that structural changes benefiting the producing states such as greater technical knowledge, increased capital resources, and the entrance of new firms into the market, have taken place in recent years. Raymond Vernon has maintained that such shifts typify raw materials industries and enable producing states to secure greater revenues over time. Rouhani sees market conditions as the principle explanation for the huge revenue increases secured by the producing states in 1970 and 1971. Economic arguments of this nature imply that O.P.E.C. at most affected only questions of timing.

Rouhani points out, although not with enough emphasis, that the greatest potential problem for the producing countries is the erosion of oligopoly market control now provided by the major oil companies. While O.P.E.C. has passed resolutions calling for production regulation by producing states, absolutely no joint action has been taken. The companies continue to set the overall level of production and to allocate it among producing areas. This allows O.P.E.C. members to bargain against the concessionaires and not among themselves. If nationalizations or resistance from consuming states erode the oligopoly O.P.E.C., the energy crisis, and the existing price structure will all disappear.

STEPHEN D. KRASNER

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India's Use of Force in Goa. By Arthur G. Rubinoff. (Bombay: Popular Prakashan, 1971. Distr., New York: Humanities Press. Pp. x, 134. \$6.00.)

Rubinoff's study is an objective and empirically based analysis of the issue of Indian mili-

tary takeover of Goa in late 1961. The author has quite rigorously examined the Goa problem in the context of India's commitment to what are seen as two directly conflicting roles or self-images: a professed desire to settle international disputes by peaceful means, and leadership of the anticolonial struggle along with championship of the newly emerged nations. Professor Rubinoff has surveyed the fourteen-year period during which India attempted to settle the problem peacefully, and he has analyzed the external pressure from the more militant Afro-Asian nations which, in his view, finally persuaded India to opt for the more forceful image preferred by these states. He deals with what he calls "misunderstandings" in the West regarding India's commitment to peaceful methods, and clarifies the dilemma for Nehru in choosing between the available alternatives to rid India of the last foothold of colonialism.

Having outlined the basic arguments in the brief introductory chapter, the author (somewhat formalistically) explicates the "nature" and "principles" of Indian foreign policy in the second chapter. Chapters III-V deal with the more substantive aspects of the Goa issue, India's efforts at peaceful resolution of the dispute, and Nehru's ultimate decision to use force. The concluding chapter recapitulates the main arguments and provides more narrative dealing with additional factors impinging upon the decision to use force: V. K. Krishna Menon's upcoming election in North Bombay, India's (retrospectively unjustified) fear of NATO's reaction, and calculations *vis-à-vis* China and Pakistan. A sizable bibliography and an index are included.

The thesis that emerges from the book is that although Nehru's India had cultivated the image of world's foremost advocate of peaceful settlement of international disputes, in practice, it had sought to rely upon "minimum use of force—not complete non-violence" (p. 23) in achieving its foreign policy goals. India's commitment to resolve the Goa issue peacefully was exploited by the Portuguese (p. 110). Nehru maintained this commitment until 1961 despite widespread domestic sentiment for a more militant posture toward Portugal, and despite Portuguese firings which resulted in twenty *satyagrahis* killed and two hundred injured in August 1955. Although Nehru had initiated a shift in India's policy toward *possible* use of force in Goa in mid-August, 1961, it was the pressure from militant Afro-Asian states at the Belgrade Conference of nonaligned nations, and particularly from the African states at the

subsequent seminar under the Indian Council of Africa in New Delhi, that brought about a "profound change" in Nehru's policy toward Goa (p. 84). Thereafter, Nehru talked of "other methods" (p. 83) to solve the problem, and the Indian press (with the government's complicity) embarked on a "clumsy" campaign to make it appear that Portugal was provoking India (p. 85). Menon's impatience, urgings by the Praja Socialist and the Communist Party for immediate invasion, and electoral considerations for the Congress Party in the approaching third general elections also contributed to Nehru's eventual decision to use force. The Western nations, particularly the United States and Great Britain, and the press joined the criticism against Nehru's India, clothing the issue in moral and personal terms rather than considering the merits of the case.

The conclusions in the book are, on balance, favorable to India's decision to use force in Goa. The author is critical of India's poor timing of the decision—which made it appear that India was simultaneously the peacemaker in the Congo and the aggressor in Goa. He maintains that although India had a great many legitimate claims to Goa, it discredited itself by fabricating alleged Portuguese atrocities before the invasion. Nehru's characteristic indecisiveness and failure to gather around him trustworthy men resulted in distortions and delays in the execution of his decision to use force. Ironically, the author observes, India's annexation of Goa neither served to stem its declining influence among some Afro-Asian countries, nor did it directly assist the struggles for freedom in Angola, Portuguese Guinea, and Mozambique. It did, however, prove that India had not unconditionally forsworn the use of force; and that its policy had never been rigidly tied to peaceful methods. After Goa, India could no longer claim a special status with respect to peaceful methods.

My main criticism of this book relates to the discussion of nonalignment in Chapter II, which could have been more rigorous—thus avoiding the occasional confusion between the concepts of neutrality and non-alignment (pp. 18, 76, and 77). The study, as a whole, could also have benefited from a more systematic analysis of various factors affecting India's decision to use force. There are some minor errors/oversights in the work, such as references to Kerala (rather than Junagadh) as an instance of India's use of force (pp. 2 and 113) and depiction of Indians as a "race" (pp. 25 and 32). The book is a competent analysis of the Goa issue, and a notable contribution to the

literature on Indian foreign policy as well as international relations in general.

MUKUND G. UNTAWALE

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International Law and Order. By Georg Schwarzenberger. (New York: Praeger Publishers, 1971. Pp. 298. \$17.50.)

This collection of essays by the distinguished British jurist, Professor Georg Schwarzenberger, examines various facets of international legal development in an era, as the author sees it, of increasing international lawlessness. Some of these facets reveal substantive problems such as the growth of terrorism, the revival of sovereignty, and the development of nuclear weapons. Others indicate procedural questions of interest to political scientists and students of legal philosophy. Among these are the second chapter on "Law, Order and Legitimation" and the closing chapter on "International Images and Models." The latter chapter is particularly stimulating in harnessing to legal analysis such wisdom and perspective as scholarship owes to Merleau-Ponty and R. D. Laing.

Professor Schwarzenberger, despite [or possibly because] of his interest in phenomenology and the sociology of law is scarcely a trendy legal authority. At times his views verge on the English curmudgeonly as when he speaks of "flower children, drug addicts and other exuberances of an indulgent society" and of "the part played by a liberal or lax application of law in hastening the decay of civilization." Moreover, he insists on a strictly formal distinction between "order" and "law," which is inevitably at variance with the prevailing trend of social research in the U.S. legal community. Fair enough. But, of course, while this preference for structured categories makes for clarity, it also makes inevitable his conclusion that international law is on the wane. In the international community, international law in the Austinian sense, or even in the Rousseauian, is of relatively minor importance. On the other hand, there is a great deal of structured, orderly behavior in relations between states which depends on shared reciprocal expectations and systems analysis. This systemic or, in Schwarzenberger's phrase, "consensual type of legitimation" of orderly behavior brings about a synthetic condition in international relations which is neither purely law nor purely order. Perhaps the international community reflects not chaos and the breakdown of law so much as a shrewd rejection of rules-for-their-own-sake. Sovereign immunity, for example, is a "law" whose time has gone past, as has much "law" on expropria-

tion of alien property; but the net standing of the legal system cannot really be measured by formal, uniform adherence to such time-honored precepts. The real question is whether the world legal culture (a term Professor Schwarzenberger would not, I suspect, willingly embrace) can provide those it serves—the political and business cultures—with adequate tools to achieve those levels of cooperation and mutual adjustment to which they aspire. The post-World War II era by this standard has perhaps not done so badly, at least in comparison with the period after World War I.

The author's impatience with what he considers soft legal thinking, especially his warnings against such self-inflating concepts as *jus cogens* reflect the British view that public policy is an unruly horse. Professor Schwarzenberger is quite right to warn us that "in any system of power politics in disguise, apparent 'progressiveness' can readily be made to serve hidden sectional interests." Yet the history of the World Court during the past two decades would suggest that it is dying of rigid legalism and narrow interpretation rather than of a surfeit of broad and flexible lawmaking. Apt, too, is the author's warning that proposals *de lege ferenda* concerning terrorism and guerrillas might be disguises for the disintegration of international law. However, the chapter on terrorists, etc., while emphasizing that the growth of random or revolutionary violence endangers the future of law, order, and civilization itself, does not point the way out of the conundrum of increasing confrontation between the violence practiced in the name of law and the violence practiced against the law. The author urges that we "should think less in terms of ideological commitments and temptations to be fashionable than in helping human beings in need of assistance" (p. 233). But if we walk with him along this path it leads us only to his rather modest recommendation:

... raising the international status and immunities of the delegates of the International Red Cross; securing for such delegates greater freedom of access to privileged and unprivileged prisoners anywhere; extending the freedom of communication between the regional and Geneva headquarters of the International Committee of the Red Cross, and providing long-term financial assistance on a more commensurate scale. Such a policy would probably yield better dividends in terms of diminishing human misery and cruelty than, on the surface, more grandiose schemes and humanitarian rhetoric (p. 234).

But the author, whose excellent chapter on hidden agendas, values, motives, models, and im-

ages qualifies everything else he says, certainly appreciates that the Red Cross is not about to be accepted as a substitute for a fundamental reordering of human priorities and of the institutional instruments by which both law and order are achieved. Thus the Schwarzenberger model of international order, and of the role of law in order creation, assumes that the existing, status quo-oriented legal culture can develop a capacity for systematic internal change and reform, which the author's empirical evidence denies.

In short, disorder may be better than bad order, lawlessness may be preferable to stultified law. Professor Schwarzenberger acknowledges this. He does not, however, appear ready to accept that lawlessness and disorder may be the law and order of rapid, transitional change, subject to its own internal reciprocal code of conduct and ameliorating norms worthy of study as "law."

International Law and Order is the sort of book which I absorbed while moving my lips, not because I am a handicapped reader but because I found myself discussing, arguing, and not infrequently, emphatically agreeing with this brilliant, provocative, and witty author.

THOMAS M. FRANCK

New York University

The Rise of Free Trade Imperialism. By Bernard Semmel. (Cambridge: Cambridge University Press, 1970. Pp. 250. \$11.00.)

In this book, his third on British imperialism, Professor Semmel traces the intellectual origins of late nineteenth-century British imperialism to the theories which supported the two "forms" of the mid-Victorian British "Empire of Free Trade" (p. 8). The first of these "theories . . . of capitalist imperialism" (p. 211) viewed England as "the Workshop of the World," the wielder of an "informal dominion by virtue of her industrial monopoly. . . ." The second, represented most fully by Edward Gibbon Wakefield and supported by the Radical Colonial Reformers, stressed Britain's more "formal" Colonial empire (p. 8). For Wakefield, free trade was necessary but not sufficient. He thought that only a "program of systematic colonialization" could alleviate the "glut of goods, of capital and of men" (p. 76) which plagued the capitalist industrial order. Semmel makes a convincing case that Wakefield had a "theory of capitalist imperialism" (p. 212) that anticipated in some respects that of the neo-Marxists such as Luxemburg and Lenin.

At the other extreme of Radical opinion were those who became, after 1850, the leading

spokesmen of Radicalism, the pacifistic, cosmopolitan Cobdenites who, in spite of their nationalistic and mercantilistic lapses, were not, Semmel apparently concludes, especially imperialistic. To establish that there was, strictly speaking, a Free Trade Imperialism during this period, Semmel must show that the "orthodox" (p. 11) Radical Free Traders who rejected both colonization and Cobdenite cosmopolitanism, men like Joseph Hume and Lord Brougham, were supporters of a Free Trade Empire.

Although the author displays great knowledge of the economic thought and controversies of the period and considerable ability to distinguish clearly the various schools of Radical Free Trade opinion, he does not make a fully convincing case that the commitment of this middle school of Radical opinion to free trade was in itself also a commitment to empire. Professor Semmel fails to make his case largely because his book, despite its impressive scholarship, is not ordered by a precise, well-argued view of what constitutes imperialism. He seems to reject the view that a free trade policy is necessarily imperialistic, or that it was imperialistic due to the great disparity between the industrial development of England and that of her trading partners. The basic, but largely unexamined assumption of his argument is that the anti-colonial Free Traders were imperialistic because they desired to preserve and extend the "industrial predominance" (p. 149) of England, to strengthen her position as the "Workshop of the World." But since he does not argue that a strict free trade policy necessarily, or as its leading proponents understood it, involved control over, exploitation of, or large capital exports to foreign nations, it is not clear why such an economic policy is imperialistic.

Some of the evidence cited, but not adequately discussed, in this book indicates that many Free Traders combined an economic policy of expansive free trade with a political commitment to empire, i.e., to making Britain predominant in power and prestige. By essentially confining himself to a history of economic thought and policy, Professor Semmel has been prevented from giving us a full account of the instances of mid-Victorian imperialism which he has uncovered.

In his perceptive final chapter it becomes clear that, although he disagrees with Hobson's and Schumpeter's view that "imperialism was thoroughly out of line with the temper of capitalism in its classic period," (p. 217) Professor Semmel basically agrees with them that capitalism is not inevitably imperialistic. If the Free Traders had understood, as Hobson did, that a

more "equitable distribution of national income" provides an internal solution to the problem of capitalist glut they could have combined a modest free trade policy with one of income redistribution. The equalitarian welfare state has ended the need for capitalist imperialism.

KIRK R. EMMERT

Bowdoin College

Western Technology and Soviet Economic Development: 1917 to 1930. By Antony C. Sutton. (Stanford, Calif.: Hoover Institution Publications, 1968. P. 381.)

This is a delightful book for those who like anachronistic museum pieces breathing the fiery spirit of the Cold War of the 1950s. Sutton's basic hypothesis is "that Soviet economic development for 1917-1930 was essentially dependent on Western technological aid" (p. 283). Chapters 2 through 15 of the book are long and tedious discussions of each major industry designed to convince the reader that this is so. Chapter 2 asserts that the Caucasus oil fields were totally dependent on Western technological aid for development in the 1920s.

The same assertion is made for the coal and anthracite mining industries (Chapter 3), the metallurgical industries (Chapter 4), the non-ferrous metal mining and smelting industries (Chapter 5), gold mining, platinum, and asbestos (Chapter 6), agriculture (Chapter 7), fishing, hunting, and canning (Chapter 8), lumbering (Chapter 9), machine building (Chapter 10), electricity and electrical equipment (Chapter 11), chemical, gas, and dye industries (Chapter 12), food, clothing, and housing industries (Chapter 13), transportation and transport equipment (Chapter 14), and military production (Chapter 15).

There is very little pretense of objectivity in this polemic. Although a great deal of "evidence" is presented, none of it is comprehensive, and all of it falls into the category of casual empiricism. Sutton's main sources of data are the U.S. State Department, the German Foreign Ministry, and Oberkommando der Wehrmacht (and an "economic analysis" by Scotland Yard!—p. 295). Thus, even the "facts" are suspect since the natural bias of the official U.S. and German Archives would certainly be toward overemphasis on the degree of their contributions to Soviet development.

It must also be noted that Sutton's is a peculiar use of the "aid" concept, since the capitalist governments all did their best to prevent any trade of any sort with the Soviet Union in the early years—and only the greed of individual

capitalists broke through that barrier. This "aid," therefore, was not gifts to the Soviet Union, but very carefully drawn commercial agreements, paid for by precious foreign currency.

Of course, the Soviet Union was a backward country before the revolution, and did borrow much technology from the West, mostly bought and paid for through the nose. But Sutton goes much further, pushing the point in an exaggerated rhetoric: "The Western contribution to Soviet production between 1917 and 1930 was total. No important process has been isolated which was not a West-to-East transfer" (p. 340). Of course, where else would they have found the technology in those days? The trouble is that Sutton implies from that truism that the main efforts to rebuild and expand the Soviet economy came from Western capitalists rather than the Soviet people. One should rather emphasize that their sacrifices were bitter and hard to take, beginning with the vast amount of agricultural produce that had to be exported to pay for Western machinery and technology (and ending with the Stalinist dictatorship that enforced these sacrifices).

In order to show that the Bolsheviks and the Soviet people did nothing, and foreigners did everything, Sutton totally rewrites history in several other respects. First, he argues that Tsarist Russia wasn't so bad, that it was really quite developed economically (p. 345). Yet this was a country that had almost all the characteristics we currently associate with underdeveloped countries: more than 80 per cent of the population was rural and agricultural and illiterate, most of the small amount of modern industry was owned and run by foreigners, and the transportation network was tiny and inefficient.

Second, Sutton admits that most experts have always thought that the collapse of Russian industry in 1917-1922 was due to World War I, the effect of two revolutions, many peasant uprisings, the anti-Bolshevik military intervention by 14 foreign capitalist powers including the United States, and the Civil War fought by all the forces of the counter-revolution. (See, e.g., Howard Sherman, *The Soviet Economy* [Boston: Little, Brown, 1969], chapter 3.) But he airily dismisses these minor dislocations. Instead, he claims: "The basic cause for the collapse was the economic illiteracy of an ideology which had neglected to think out its economic counterpart and drove a viable growing economy into a shambles" (p. 310). Thus, he finds that socialism caused the catastrophic decline, while foreign capitalists produced the re-

covery. One might wonder, though, just what kind of "viable growing economy" could exist in the midst of a world war, two revolutions, Civil War, and foreign intervention—even assuming the best possible economic ideology?

In emphasizing the importance of Western benevolence, he does modestly admit that technology is not the "only factor in economic development. Political, social, and psychological factors play their respective roles. This interplay is particularly interesting in the Soviet example but is, unfortunately, outside the scope of this study" (p. 4). Since he doesn't hesitate to make broad generalizations about many political events, this is a strange modesty. It simply gives him an out from considering the main trouble with his thesis. Soviet industrialization involved not only the importation of a minor amount of foreign machinery and technology; it also involved a major change of the society from capitalism to socialism and planning.

Many other underdeveloped countries have received large amounts of foreign investment and large amounts of free aid in the half-century since the Soviet Union began its industrialization. Why have none of these been successful? Why are India and Pakistan and Africa and Latin America still at abysmally low levels, while the Soviet Union is the world's second industrial power? Does this have something to do with the difference between capitalism and socialism? How did this one underdeveloped country achieve success with only minor foreign imports whereas others have so far failed with major foreign imports including free aid? Not only was the Soviet Union socialist economically, but there is also the fact (which Sutton clearly doesn't like) that it confiscated foreign capital, whereas all the others allowed continued imperialist domination.

In addition to the major thesis of the book, several other aspects reveal the author's bias. At several points in the book, Sutton seems to imply that it is a bad thing that the United States ever decided to allow any trade with the Soviet Union, and he hints at support for U.S. government restrictions on Soviet trade. Second, and closely related, he slanders all the American capitalists who participated in early trade agreements with the USSR. Thus, it turns out (on the sole basis of State Department files) that Charles H. Smith was "more or less" a Soviet agent (p. 284) as was Julius Hammer, and Ludwig Martens, and several others who wanted trade with the Soviet Union. It sounds more like a CIA fairy tale than a scholarly study.

Finally, based again mainly on "unbiased"

State Department files, he convicts most of the Soviet leaders of personal corruption in relation to the foreign concessions, including here slanderous attacks on Trotsky, Zinoviev, Chicherin, and Dzerzhinsky (p. 308). Moreover, just to show no bias against Trotsky, he also tosses in an attack (on the basis of no evidence whatsoever) on the personal corruption of Stalin and all the Soviet leadership of the later period as well (p. 309).

HOWARD J. SHERMAN

University of California, Riverside

Congress, the Executive, and Foreign Policy.

By Francis O. Wilcox. (New York: Harper & Row, 1971. Pp. 179. \$5.95.)

Francis Wilcox has produced the fair-minded study of the American foreign-policy process that one might have expected from an able scholar with substantial experience on both the legislative and the executive sides of that process. His treatment is pragmatic and constructive, with a dash of wit and much of that wisdom which derives from mature appreciation of the complexities of the process and tolerant understanding of the foibles and frailties of those involved in it.

Wilcox refrains from invoking abstract principles, simplistic slogans, or legalistic formulas pertaining to executive-legislative relations in the foreign-policy field. He offers several much-needed reminders: that debates concerning the legal competence of the two branches more often reflect substantive disagreement than differences in constitutional interpretation; that congressional inclinations regarding foreign policy are not uniformly or consistently more (or less) moderate, constructive, or "liberal" than those of the executive branch; that making policy conform to the "people's will" is not a simple matter of honoring the wishes of Congress, following the judgment of the president, acceding to the demands of articulate groups, or interpreting the results of elections or public opinion polls; and that somewhere between the extremes of tying the hands of the president and giving him a free hand lies our chance to secure essential presidential leadership.

Though the author insists, and this reviewer agrees, that the breakdown of consensus about the American role in the world is more the cause than the effect of controversy about the proper place of Congress in the making of foreign policy, he attaches great importance to the development of more meaningful consultation between executive and legislative branches and to the cultivation of a congressional involve-

ment that can provide both independent criticism of and helpful support for executive ventures in world affairs. He is even-handed in apportioning blame for the present unfortunate state of interbranch relationships: the executive too often treats the Congress as a nuisance or an object of salesmanship, while legislators are frequently less disposed to consult about the making of policy than to interfere in its execution, unreliable in respecting legitimate needs for secrecy, and unwilling to share responsibility, when things go wrong, for policies that they have previously supported. The take-off/forced landing syndrome looms large in the current unpleasantness. The president is more interested in involving Congress in the potential forced landing than in the take-off, the Congress wishes to participate in the take-off but to remain aloof from any possible forced landing; and neither is likely to concede to the one-sided preference of the other. Wilcox is wisely concerned with *what* as well as *whom* to blame for the recurrent failure of the two branches to collaborate effectively. He treats inherent features of our governmental system and of international politics along with the failings of presidents, bureaucrats, congressmen, and representatives of the media as sources of difficulty in the conduct of foreign relations.

Consensus is essential for effective foreign policy, but the right to dissent and the right to change one's mind are also high values in a democratic society. The most thorough consultation may not produce consensus, and the consensual foundations of policy may dissolve at critical moments. What should be done when consensus cannot be reached, or when it falls apart? The duty of statesmen to act in accordance with what they regard as the state's basic necessities, and their obligation to perform responsibly even when the going gets rough are no less fundamental than the rights to withhold and to withdraw support from policy. The urgency of rebuilding consensus should be recognized, but so should the necessity of carrying on American foreign relations when consensus does not exist. If the peculiar merit of Congress is responsiveness to the popular will, that of the president is willingness to bear responsibility for the conduct of foreign relations. The ideal must be to blend responsiveness and responsibility. Wilcox has obviously given deep thought, and his book stimulates the reader to give deep thought, to the perplexing problems of achieving that ideal.

INIS L. CLAUDE, JR.

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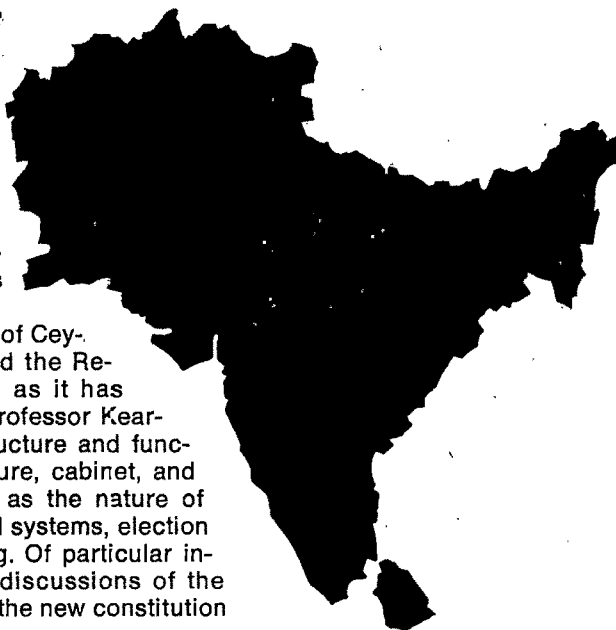
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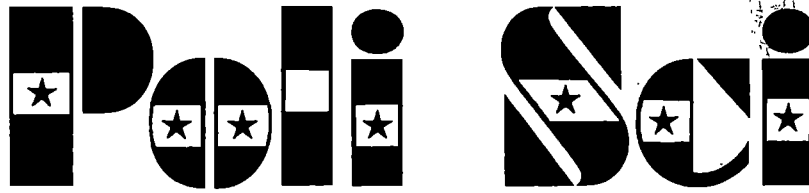
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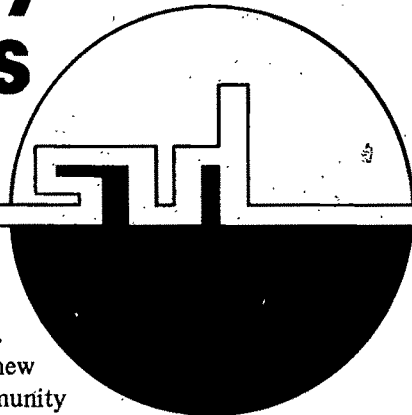
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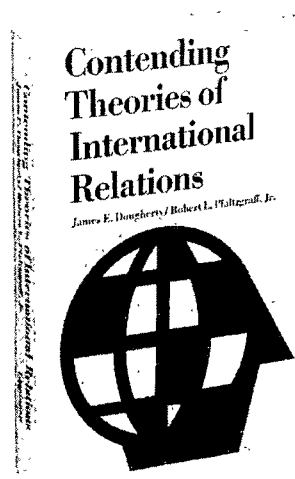
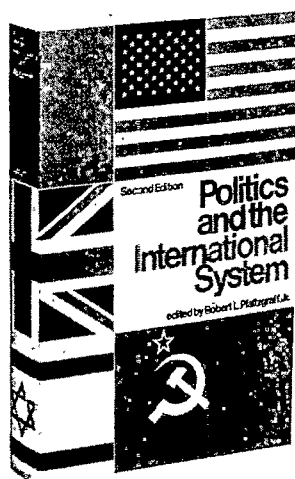
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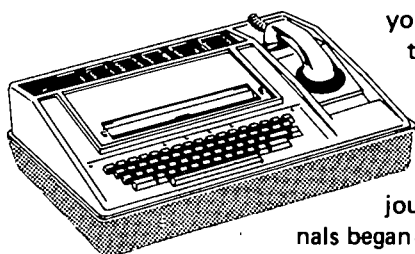
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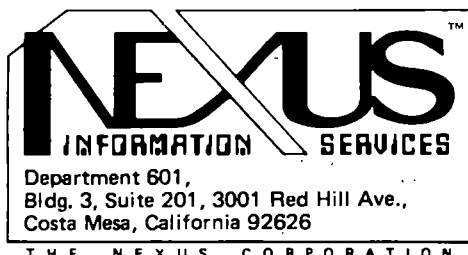
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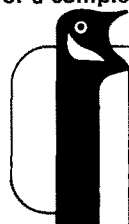
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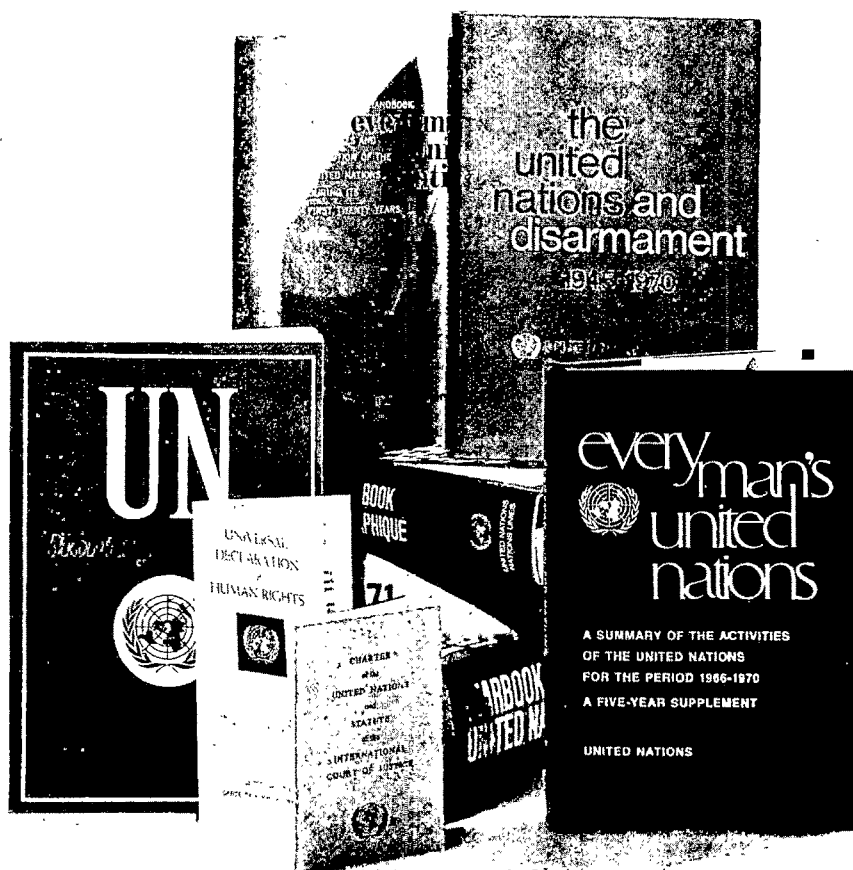
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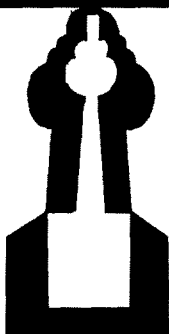
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grating the various groups of society, or mediating among them); or (4) the supposedly unique instrumentality it uses to perform its tasks (legitimate coercion). A certain view of history, or perhaps cultural arrogance, also lay behind the emphasis on the formal organizations of sovereign states. This was the view that sovereign state-organization, as developed in the modern West, was the natural destiny of all societies, and not a remote end but one achieved as soon as a modicum of "civilization" was attained—so that the kinds of politics described by early anthropologists like Maine and Fraser could be dismissed as simply "prepolitical."³

It is true that political scientists have long been concerned with all sorts of organizations and activities that impinge upon states but are not, formally, parts of them. However, their concerns with such phenomena initially derived from and were circumscribed by their very identification of politics with state-organizations. Attacks on that identification can be traced back only to about 1930 and became common only in the 'fifties.⁴ The chief factor that made the objections common then, and now, involves the broadening geographic interests of political scientists: their new and growing interest in the politics of "developing" societies. In such societies, a search for specialized formal organizations exercising the attributes of sovereignty, and displaying the supposedly unique characteristics of states, sometimes reveals nothing at all or, more commonly, a facade behind which political realities lie rather ineffectively hidden. This is most obvious, of course, in still largely tribal societies, where functionally diffuse structures based on primordial identifications remain the focus of social regulation, whether or not imitation-states (or rather capitals) have been superimposed on them; and between these and modern Western state-organizations runs an extensive continuum on which different approximations to what we know as statehood can be discerned. The narrow traditional notion of political phenomena did not serve subjective interests in such societies, nor prevent the feeling that they had, in some sense, a "political" life like other polities, even at the most primitive levels of social organization.⁵

³For a choice example of this view, see William Archibald Dunning, *A History of Political Theories* (New York: Macmillan, 1902).

⁴I am ignoring here certain much earlier normative political philosophers who are discussed below. The reason for ignoring them at this point is stated in the later passages.

⁵See the very large literature on "primitive" political systems: e.g., M. Fortes and E. E. Evans-Pritchard,

Doubts about the narrow conventional conception of politics having thus been engendered, it became manifest that the conception was unserviceable even for an extensive interest in the politics of Western societies. The focus on state-organization restricts study to Western societies roughly after the Peace of Westphalia (no exact date can be given, of course). If we go back much farther we find the work of sovereign state-organizations performed by a hodgepodge of diffuse, imperfectly differentiated, more or less autonomous institutions: royal dynasties, ecclesiastical structures, municipal councils, guilds, merchant companies, land-owning nobles, and universities. We find no "constitutions" as formal organizational blueprints, only charters, compacts, bills (and sometimes so-called Constitutions, like those of Clarendon) as contractual agreements or explicit statements of usage among competitive and overlapping *potentias*. We find no monopolies, actual or pretended, of legitimate coercion. We find, instead, a long and slow fashioning of sovereignty and its instruments as a gradual process of "political expropriation"⁶ that seems more tied to special historical circumstances than to the universal attributes of civilized life. Yet one has the sense that "politics" antedates that process and went on throughout its unfolding; and political scientists anxious for light on political development in non-Western societies turn, not unnaturally, to the earlier histories of Western polities themselves.

As the boundaries between state-organizations and other structures for carrying on government and politics weakened, another objection against the focus on state-organizations also emerged. This criticism involved a view occasionally advanced earlier (e.g., by Merriam);⁷ that, apart from possibly unimportant variables like size, there is not all that much difference between "public" governments and "private" ones—not enough, at any rate, to warrant a generic distinction on which to rest disciplinary divisions and all they entail.⁸ This view is quite different from one admitting the study of "private" groups into the field as influ-

African Political Systems (London: Oxford University Press, 1940); I. Schapera, *Government and Politics in Tribal Societies* (London: Watts, 1956); A. I. Richards, editor, *East African Chiefs* (London: Faber, 1960); and Lucy Mair, *Primitive Government* (Harmondsworth: Penguin Books, 1962).

⁶Max Weber, *From Max Weber: Essays in Sociology* (New York: Oxford, 1946), p. 83.

⁷Charles Merriam, *Public and Private Governments* (New Haven: Yale, 1944).

⁸See especially Robert A. Dahl, "Business and Politics: An Appraisal of Political Science," *American Political Science Review*, 53 (March 1959), 1-35.

ences on public power and policy. The latter view treats the groups as related to political systems through boundary-exchange; the former admits them as political systems in their own right. It thus implies that, in excluding private governments, political science deprives itself of much material useful for building empirical theories while, at the same time, failing to make an optimum contribution to the general understanding of social life.⁹

The Functional Conception of Politics

The principal response to the difficulties raised by the notion of politics as centered on state-organizations has been a tendency to define politics in terms of certain social functions rather than social structures.¹⁰ The essential purpose of the functional conceptions has been to raise the traditional focus of political study to a somewhat more general level, at which equivalence between sovereign state organizations and other structural modes of carrying on what is sensed to be political activity becomes apparent.

The logic behind the functional conceptions seems impeccable. Political structures differ widely in space and time, so that the identification of politics with any special structural arrangement unduly restricts and handicaps the field. But different structures can perform the same functions (i.e., can be functional "equivalents"). Hence, the field's scope can be broadened and yet remain clearly defined in relation to nonpolitical phenomena if one identifies the functions of state-organizations and then studies whatever performs those functions where such organizations do not exist or are ill-developed.

Neat as this may seem, the functional revision creates large difficulties of its own. The foremost is that the functions of political structures seem hard to identify, as shown by the fact that practically no functional conception of what is "political" coincides exactly with any other. To be sure, we can discern a sort of latent consensus among the conceptions, but it is on a notion of the "political" function that seems, in the final analysis, to identify nothing in particular at all—to make politics into the

study of "the whole of social life in all its facets," as Bentley put it. Some people may not object to this. But it is hardly what the functionalists intend; it may impose intolerable burdens in political research; it probably leads to theories so general (because concerned with such a diversity of phenomena) as to be trivial and uninformative; and if politics becomes equated with social life we will no doubt want to carve it into separate fields, as we now carve up social studies, thus returning to where we started.

The function on which the various functional conceptions converge is that of "maintaining social order"—not in the narrow sense of "law and order" but in the very broad sense of assuring the continuity (viability) of society through its harmonious and efficient operation. That function, as functional sociologists like Parsons make clear, is really a large and diverse (indeed all-encompassing) complex of functions. Parsons lists them under four categories, which will do about as well as any: (a) pattern-maintenance (roughly, imparting to members the society's general norms of behavior and special norms pertaining to its special roles, and getting commitment to them); (b) integration (coordinating the elements of social life to make them mutually supportive, or at least not mutually destructive); (c) goal-attainment (defining and achieving social ends and purposes); and (d) adaptation (generating an appropriate level of social resources, distributing them in ways perceived as just, and mobilizing them to the attainment of social goals). It should be apparent that all institutions of society, in one way or another, to one degree or another, are involved in the performance of these functions. It is also evident that all the functions must be performed in all social institutions if these are to be "viable"—a point that functionalists gleefully emphasize rather than conceal.

Functional definitions of politics thus converge on a view that broadens the scope of the field at the cost of virtually eliminating its boundaries altogether. For this reason, they hardly define the political scientist's subjective interests, however much broader now than formerly; and while all social phenomena might be usefully analyzed in terms of the order-maintaining function at a high level of generality, it hardly follows that disciplinary distinctions beneath that level are unimportant. One might note, in this connection, that functional ap-

⁹ For a more extensive critique of the traditional conception of politics, see David Easton, *The Political System* (New York: Knopf, 1953).

¹⁰ Examples are Gabriel A. Almond, "Comparative Political Systems," *Journal of Politics*, 18 (August 1956), 391-409; Almond and James S. Coleman, editors, *The Politics of the Developing Areas* (Princeton: Princeton University Press, 1960); Talcott Parsons, "Voting and the Equilibrium of the American Political System," in *American Voting Behavior*, ed. Eugene Burdick and Arthur J. Brodbeck (Glencoe: Free Press,

1960); and David E. Apter, "A Comparative Method for the Study of Politics," *American Journal of Sociology*, 64 (1958), 221-237.

proaches to special subjects, like politics and economics, have been mainly advanced by sociologists in quest of overarching theories,¹¹ and that such approaches in politics have, over a span of about a decade and a half, hardly gone beyond the initial definitional level of conceptualization.¹²

The obvious way to save the functional alternative from this difficulty is to identify politics with a single "political" function in performing which modern state-organizations specialize. But this also raises large problems. One, as stated, is to identify the function. Two views on this are common (and others are possible or occasionally advanced): Politics is concerned with "goal-attainment" (Parsons), or politics is concerned with "integration" (Almond). That difficulty might be resolved, in the manner of Bourricaud and Spiro, by a synthesis: Politics is concerned with "integrating conflicting views about social goals."¹³ However, that would still leave a major research difficulty. Functions are abstractions, not concrete entities. The first task of any political analysis would therefore be to identify the structures—or, worse, aspects of structures—that have something to do with the performance of the functions, which might be all or any in a society. Parsons puts his finger on the intricacy of that task in a rather offhand footnote: "The polity in (my) sense is *not* identical with government, which we interpret to be a complex of *organizations*. Government has other than political functions, and other organizations participate in the polity."¹⁴ No sweat for people like Parsons, who steer clear of empirical work on concrete phenomena. But what does one do when descending from the

rarefied air of conceptualization? What aspects of American government, for instance, are, or are not, political? What aspects of American business, trade unions, schools, professions? The critical point is that, on such a basis, we still wind up studying "the whole of social life in all its facets," quite as much as with the broader functional alternative. In one case, we do so because everything is political, in the other because everything has, or might have, political aspects. And the burden this entails is magnified by having to identify the political aspects without structural guidelines to the task—no guidelines other than that they must have something to do with goal-attainment or integration. No one has ever done this or anything like it; and no wonder.

The moral is that functional conceptions of a subject matter are, in their very nature, to be avoided if satisfactory structural ones, directing attention immediately to concrete phenomena, are available. But can one really devise a structural conception of politics that avoids the main shortcomings of the traditional conception of the field—a conception that does not define the subject in narrowly culture-bound terms, nor make too sharp and rigid the distinction between public governments and private ones?

Politics as Asymmetric Relationships

Attempts to redefine the subject matter of politics antedate recent interests in underdeveloped, developing, and historic polities by quite a bit. The earlier attempts (mainly of the 'thirties) were in fact structural, not in the least culture-bound, and virtually oblivious to distinctions between public and private governments. If nothing else, they demonstrate that the functionalist's way of delimiting the field is not the only alternative to the conventional structural conception.

The object of the earlier redefinitions was to make politics a "discipline" rather than to accommodate expanding subjective interests—save perhaps for a still embryonic interest in private groups as political orders in themselves (rather than because of their boundary-exchanges with "polities"). In gist, their object was to make it possible for political science to be scientific in some serious sense of the term, and those who advanced the redefinitions were, in that respect among others, precursors of contemporary "behavioralism" in the field. The prevalent conception of the scope of politics seemed to them the first and foremost impediment to the achievement of a "science" of politics, in part because of its very narrowness, in part because the phenomena considered political seemed too

¹¹ E.g., Talcott Parsons and Neil J. Smelser, *Economy and Society* (Glencoe: Free Press, 1956).

¹² This criticism does not apply to works that do not define politics functionally but urge a functional approach to the subject otherwise defined. For example, Almond's widely influential functional approach to the study of political systems is mainly tied to a conception of politics that stresses, à la Weber, an instrument of action rather than a social function: legitimate coercion. In his definition of 1966, legitimate coercion (plus territorial jurisdiction) is all that is advanced; in the definition of 1960, functional language is merely added to the original notion—probably to make the definition of the subject look as functional as the approach to its study. (See note 10, above, for references.)

¹³ Bourricaud: Political structures have the task of forging a common will out of conflicting wills in a society. Spiro: Political structures are those institutions of a society that "process" their "issues." See François Bourricaud, "Science politique et sociologie," *Revue française de science politique*, 8 (June 1958), and Herbert J. Spiro, "Comparative Politics: A Comprehensive Approach," *American Political Science Review*, 25 (September 1962), 577–596.

¹⁴ Parsons, *Structure and Process in Modern Societies* (Glencoe: Free Press, 1960), p. 42.

complex and various to serve as the subject matter of a science. In a sense, the very "uniqueness" used to justify the autonomous study of state-organizations furnished the main ground for their attacks on the conventional delimitation of politics as a discipline.

A typical argument to this effect is Catlin's.¹⁵ Science, Catlin argues, induces generalizations from "simple" phenomena that are "commonplace and frequent of occurrence"; states have been and are few in number, and are also quite "uniquely" constituted (i.e., complex and variable on many dimensions); hence nothing can safely be induced from their study. The problem is readily translatable into statistical language: it is difficult or impossible to generalize if one has a small n and a large variance. The solution must be to find a far simpler subject matter embodying many more instances. That subject matter must certainly be of the essence in state-organizations, but, just as certainly, it must also be found in many other contexts.

Note that the problem discerned was not that of finding "equivalents" of state-organizations in premodern cultures. The problem was to find something to replace the focus on the state in all cultures.¹⁶

Despite differences in language, the solutions proposed, like those of the functionalists, converged on a common point. In essence, it was proposed that politics be equated with what might be called "asymmetric" human relationships. All the early revisionists started with the same question: what are (in Catlin's chemical metaphor) the "elements" from which all political complexes, as thought of conventionally, are compounded—or, in different metaphors, the particles or cells of "bodies" politic? The answer was derived from what seemed (and no doubt is) an obvious truth: governments are *complexes* of rule and hierarchy (of *κράτος* and *-αρχος*). Hence the elementary relations constituting them must be *items* of power, or control, by some men over others. In essence, these items consist of interactions in which abilities to produce intended effects and derive benefits are unequally distributed. This is precisely what is meant by "asymmetric" relations; as Lasswell might put it in his pungent way, they are relations in which someone affects more than he is affected, controls more than he is controlled, and/or gets more of what is allocated. Such asymmetries manifestly occur in

virtually all human relationships, everywhere and on all social levels. They certainly are intrinsic to state-organizations, but states are just particularly large, complex, well-patterned, persistent, and (more questionably) "pure"¹⁷ complexes of them. Asymmetric relations thus provide just the sort of simple, frequent, and commonplace units of analysis that inductive science seems to presuppose.

Anyway, so the argument went—and it surely is not, *prima facie*, unpersuasive. It would be hard to argue that asymmetry is not inherent in governments of all types, at all social levels, or that anything of great consequence about the governments of states would be missed by considering them large and complex networks of power and control relations. Easton does point out that "goals"—the purposes to which power and control are put—also matter.¹⁸ He is right; but nothing precludes analysis of the substance as well as process of politics, no matter how politics is conceived. After all, nothing prevented those concerned with state-organization from analyzing their policies. Nor does the conception of politics as asymmetric relations (*pace* moral critics like Franz Neumann) necessarily have misanthropic implications. It does not imply, as Neumann argued it did, that all human collectivities are ultimately controlled by brute coercion, or that there are no absolute moral laws, or that democracy and constitutional government are impossible, or that no important qualitative differences of any consequence exist among polities, or even that asymmetry is always strongly weighted in favor of nominal rulers.

One is tempted to conclude, therefore, that when political scientists embarked on the search for a less restrictive conception of politics in the 'fifties, they leaped into functional definitions through insufficient consideration of available alternatives or because of moral biases without foundation. Nevertheless, one may have important reservations about the early structural redefinition. It seems better regarded as a large first step toward a serviceable conception of the field's subject than as a definitive delimitation of its scope.

Most important, the equation of politics with asymmetric relations delimits very little. Like the functional conception of politics as the order-maintaining aspects of society, it broadens the field by virtually eliminating its boundaries altogether. To be sure, there are social relationships that are symmetric, or as nearly so as

¹⁵ George E. G. Catlin, *A Study of the Principles of Politics* (London: Allen and Unwin, 1930).

¹⁶ Catlin was by no means the only man to perceive the problem, or to try to solve it. His contributions were in fact overshadowed by those along similar lines of Harold Lasswell.

¹⁷ "Pure" means "unmixed with symmetric interactions" here. For the reasons that this is questionable, see below.

¹⁸ Easton, *The Political System*, p. 117.

makes no difference. One thinks of buyer-seller exchange relations (under free-market conditions), affective relations among friends or spouses, or interactions on collegial decision-making bodies that are not just nominally collegial. But while such relations frequently occur, they "characterize" few social units; they do so less and less as the notion of symmetry is more strictly defined; they are virtually nonexistent in many societies and subsidiary units; and they are more likely to occur in ephemeral interactions than in organized or institutional collectivities. Certainly, it would be difficult to say that any concrete institution or relationship in any society is automatically excluded from the scope of politics defined as the universe of asymmetric relations.¹⁹

From this difficulty others follow. One involves our subjective interests. Easton (see Appendix) rejects the equation of politics with asymmetric relations mainly because of this consideration: political scientists are simply interested in "political" asymmetries, not all social asymmetries. A general theory of power, he argues, would help them, but would still have to be shaped to the special phenomena of political power, conceived in some narrower sense.²⁰ More important is the question whether conceiving politics more narrowly (but not in the very narrow sense of state-organization) is advisable also on "disciplinary" grounds—that is to say, whether a general theory of asymmetric relations, even if helpful, would help much. A strong argument to the effect that it would not can certainly be made.

The crux of this argument is that the world of asymmetric relations varies extremely on many dimensions, so that the classification of human relations into symmetric and asymmetric ones would not be very informative descriptively, hence also not in regard to theory. Asymmetric relations vary, for example, on the basis of numbers of people involved in them (from two to just about all human beings). They vary in regard to continuity and regularity of interaction—asymmetries exist in continuous and regular interactions in a persistent so-

cial collectivity (teacher and pupil, in school), or in regular but hardly continuous interaction (doctor and patient), or in ephemeral interaction (criminal and victim, or the slow driver and the drivers he slows down). They vary in regard to the nature of the "actors" involved in asymmetric interactions, who may be single individuals or more or less complex collective individuals. They vary in regard to degree and direction of asymmetries—i.e., they may, in varying degrees, involve unidirectional or multidirectional flows of power and control (in certain, probably most, respects, bosses control their secretaries; in others, secretaries run their bosses). And so on, along many more criteria of discrimination, any of which might be consequential for one theoretical purpose or another.

It is possible to cover all this diversity, and more, with generalizations of some sort, and no doubt some people's interests will run to such an extreme level of generality, or beyond. But the distinction between symmetric and asymmetric relations surely discriminates no more than the first and simplest distinction biologists make among members of that extremely heterogeneous class, the animal "kingdom": protozoa (one-celled animals) and metazoa (many-celled animals). For most purposes, a field so broadly delimited would have to be subdivided into many virtually autonomous subdisciplines, so that, as with the functional alternative, we come back again to our starting-point: trying to find some delimitation of "political" asymmetries that seems defensible on disciplinary grounds and at least approximates special subjective interests.

Delimiting the Scope of "Politics" as a Problem in Classification

Working out a reasonable delimitation of the scope of political phenomena is, of course, a matter of classification: arriving at the defining traits of a special class of cases to which the label "political" is sensibly applied. What we have uncovered to this point is a rather familiar problem in the construction of taxonomies (familiar, at any rate, in fields that take classifications more seriously, and construct them less arbitrarily, than political scientists do). To clarify that problem and point the way to a reasoned solution of it, we now go off on a brief tangent on the subject of constructing classificatory categories.

Taxonomies that do not just amount to arbitrary labeling can be constructed by numerous methods, which could only be treated fully in a separate paper. However, much the best of these methods for most purposes, as well as the most widely used where classification is re-

¹⁹ Catlin was quite aware of this problem. (See *A Study of the Principles of Politics*, p. 66.) He specifically counsels that politics, as a discipline, must not concern itself with "all the life of the community," only with those activities in it that are, somehow, political. On the other hand, recognizing that not much is left clearly nonpolitical by his definition, he also counsels Aristotelian wholism in holding that politics is present in all social relationships. Easton rightly extends this view to Lasswell's conception of politics, although not to his substantive work. (See *The Political System*, pp. 119-124.)

²⁰ Easton, p. 123.

garded as an important intellectual enterprise (e.g., in biology), is a procedure that might be labeled "progressive differentiation" (my term). That method happens to be especially useful if one is caught in the dilemma of being either excessively restrictive or excessively extensive in one's conception of a class of cases—precisely the difficulty uncovered here.

"Progressive differentiation" proceeds, in gist, as follows: (a) One begins with a very broad and heterogeneous subject matter, perhaps having nothing but one attribute in common, such as "organisms": material entities capable of reproducing themselves. (b) One then asks on what basis that subject matter can be subdivided into two (or at any rate a very small number of) very populous subsets, so that (1) all instances of the subject matter fall into one subset or the other, and so that (2) all cases in a subset share one characteristic additional to that of the whole subject matter, which (3) is not found in any case in the other subset(s). (That initial cut, in biology, is made between plants and animals, on the basis of ability to

produce "food" by processing inorganic matter.) (c) One then asks the same question of the subsets (e.g., animals, as stated, are divided into protozoa and metazoa) and continues thus to differentiate until one arrives at a compelling stopping point—the most compelling being the point where differentiations can only be made sensibly by singling out particular individuals, not sets of them.

The process involves the systematic use of "branching." Each offshoot, at every level, is a member of a more inclusive branch, but has at least one characteristic different from all other offshoots from the same branch. Figure 1 portrays the process graphically (using biological categories—somewhat simplified—to identify levels of differentiation).

Classificatory schemes constructed by progressive differentiation have several characteristics that such schemes ought to have if used as bases of general propositions. They leave nothing "residual" (unclassifiable), preclude ambivalence (i.e., having equally good reasons for classifying a case under two or more sets),

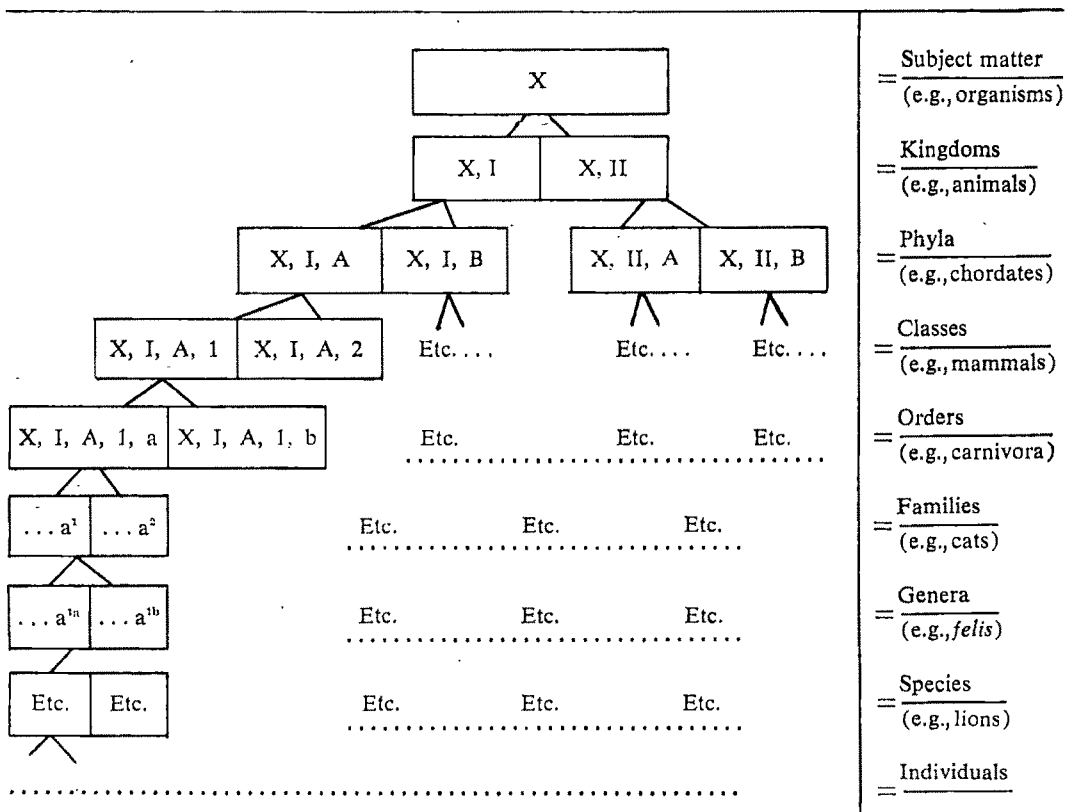


Figure 1. The method of progressive differentiation.

and also preclude ambiguity (difficulty in deciding to what set a case belongs). Even more important here, it should be evident that at the upper levels of a classificatory ladder thus constructed, cases will be very numerous and very heterogeneous, while at the lower levels they will become progressively more homogeneous—similar or identical on many dimensions—and more limited in number. Consequently, while generalizations are possible at any level, those that cover the more inclusive (less differentiated) categories will, of necessity, provide only limited information about the less inclusive classes, and sometimes hardly any of the information one subjectively wants about them. In that sense, they will be “vacuous.” On the other hand, a class may be so narrowly defined that subjectively sought information becomes available only at a disproportionate price in regard to the possibilities of induction, or validation of theories arrived at by any means whatever. In such a case, one is compelled to seek a trade-off between the extensiveness and homogeneity of the classes. Even if that compulsion is not absolute, any large gain in extensiveness is to be preferred if the cost in loss of homogeneity is small—not just because of the requisites of inductive methods but because of the parsimony of theories about a subject, in the sense of ratio between cases covered and information provided.

My criticism of others' ways of delimiting the scope of political study can now be restated in a more compact manner. In addition, the tack to take in order to solve the problem we have uncovered seems clearly to emerge from this note on methodical taxonomy.

(a) *The conventional delimitation of the scope of the field sacrifices too much extensiveness for the sake of homogeneity.* It does so not just because state-organizations are rather few in number, but also because they are not so different from other structures as to make the gain in homogeneity large. This is not to mention the nonmethodological fact, certainly admissible as a supplementary argument, that our subjective interests no longer are confined to so exclusive a subject matter, although they certainly continue to include it.

(b) *The early revisionist conception of politics as the study of asymmetric relations sacrifices too much homogeneity for the sake of extensiveness.* It does so chiefly because Catlin and his ilk seem to assume that typological categories should have only a single defining characteristic, “commonplace and frequent of occurrence.” Obviously they may have any number of them, so long as the mix between subjective interests and disciplinary imperatives re-

mains reasonable. (In fact, the broad structural conception of politics itself has at least three: (1) homo sapiens, (2) in relationships, that (3) are asymmetric.)

(c) *The later “functional” revision has the same result, compounded by difficulties that always arise when one emphasizes functions rather than structures.* It proceeds, implicitly, from the right question: to what more numerous, if less heterogeneous, class can “states” be considered to belong? But the answers provided lead to the same overextensiveness and excessive heterogeneity as do the earlier revisions and, still worse, direct attention to elusive abstractions from concrete observables.

(d) *Our problem then is to find a reasonable trade-off between the homogeneity of the conventional conception of the scope of the field and the inclusiveness of proposed revisions, without incurring avoidable costs in ability to identify pertinent phenomena.* To avoid vagueness, we want a “structural” conception of political phenomena. For the sake of homogeneity, we do not want to include all asymmetric relations, although we do want political phenomena to be a distinct subset of such relations. For inclusiveness, we do not want the conception to be limited to state-organizations, although we do want such organizations to be a distinct subset of the class.

This clearly calls for adding structural criteria of discrimination to that of “asymmetry.” The full set of traits must be characteristic of state-organizations and also of much else, and yet still delimit a rather homogeneous and precisely identifiable set of cases.

Here we can leave criticism, and turn to the constructive part of the argument.

Political Relations as a Subset of Asymmetric Relations

My position is that three criteria additional to asymmetry can be used to define the nature of political phenomena in a manner that reconciles our subjective interests with disciplinary requirements. Stepwise, they delimit successively narrower “populations” suitable to increasingly informative empirical theory—populations that increasingly resemble the phenomena central to the traditional subjective interests of political scientists, but that are not in the least culture-bound, nor so intrinsically limited in number or widely various as to rule out rigorous inductive procedures.²¹

²¹ It is probable that the criteria will seem “obvious” when stated. If so, all the better—and all the more reason for making them explicit. Points obvious when stated, after all, are not always obvious when not stated. The unfortunate tacks others have taken in exercises of this kind attest to that.

(1) *Asymmetric relations may or may not occur in social units, among people acting in their capacities as members of the units. Those that do thus occur may be considered political, others not.*

This criterion subtracts from all power and control relations a large number and variety that are especially far removed from the commonsense conception of politics, often quite amorphous, and likely, if not subtracted or distinguished, to make theories vacuous. Examples are the power of a criminal over his victim or the slow driver over the queues in his wake. Such relations are clearly asymmetric and satisfy formal definitions of control (or power), such as that of Dahl and Lindblom:²² "B is controlled by A to the extent that B's responses are dependent on A's acts in an immediate and direct functional relationship." But they do not occur in social units, or do so only in a trivial sense (e.g., in that they occur among members of the same nation).

To avoid ambiguity, and the kind of definitional carping it begets, we had better be precise about what a "social unit" is (although readers no doubt have an immediate feel for the term).

(a) Social units are not mere aggregates of persons, but, so to speak, collective individuals. That is to say, they have a certain identity of their own, apart from the identities of their members—a fact generally ratified by special proper names (the United States, New Jersey, Princeton University, the Woodrow Wilson School, etc.). They thus persist despite turnover in members, and often have legal status quite apart from that of the individuals that constitute them. They are, in the medieval sense, "corporations," i.e., bodies or entities in their own right.

(b) In such collective individuals, interactions are recurrent and patterned: i.e., they occur continuously, or at least very frequently, and can be generalized about with rather high predictability. They are not sporadic or isolated encounters—not, for instance, the sort of relationship that exists between buyer and customer but the sort that employers have with their employees.

(c) However much the unit may be identified by its members, the members also identify themselves in terms of the unit; in one degree or another their personal identity is perceived by themselves as that of the larger entity. One does not "identify" oneself by saying: "I am a person held up at gunpoint last January

24"; one does think of oneself as "a professor at Princeton University," and the like. The strengths of such personal identifications with collectivities vary, but some tendency to define the self by the collectivity can always be found in "social units."

(d) As corporate entities, social units have goals distinct from those of individual members *qua* individuals. Indeed, individual satisfaction, like individual identity, is itself defined, in some degree, by collective goal-attainment. In the case of families like those Banfield studied in Southern Italy,²³ that definition is extreme, but it exists in virtually all families. As societies "develop," however, the identification of self-satisfaction with collective goals seems to become focused increasingly on larger-scale units, including the "state" and organizations like political parties. Old equations between self and collectivity, by and large, remain, but others (hence other social "units") are superadded. (I leave aside the vexing question of whether there are "rational" collective goods. Empirically detectable nonrational ones suffice.)

(e) As in other "bodies" there exists in social units a more or less specific, or differentiated, allocation of roles or division of labor attuned to the unit's goals, together with definitions of tasks, duties, responsibilities, and rights appropriate to the roles. This, again, is as manifest in small-scale units like families as in large-scale ones, like states. Someone is charged with the shopping, the cooking, the cleaning-up, the bill-paying, much as roles are devised to legislate, administer, adjudicate, push interests, or aggregate votes. The allocation may vary among members almost daily, and also may be quite uninvincible, but generally it both exists and remains fairly fixed, if only because of variations in capacity to perform tasks or expertise acquired in performing them.

We clearly eliminate many of the problems, subjective and disciplinary, raised by the broad conception of politics as social asymmetry by confining the field to social units thus conceived. Most of all, we avoid that of arriving at uninformative generalizations which cover both asymmetry in social units and in encounters among individuals outside of them—generalizations perhaps as vacuous as that the controller tends to command a greater stock of "pertinent values" (force, position on a highway, etc.) than the controlled.

(2) *Asymmetric relations may, or may not, occur among hierarchically ordered segments or members of a social unit, or manifestly affect relations among such segments. Those that*

²² Robert A. Dahl and Charles E. Lindblom, *Politics, Economics, and Welfare* (New York: Harper and Row, 1953).

²³ Edward Banfield, *The Moral Basis of a Backward Society* (Glencoe: Free Press, 1958).

occur among hierarchically ordered segments, or manifestly affect their relations, may be considered political, others not.

Segments of a unit are hierarchically ordered if any of them are thought of as "superior" to others—literally, at a higher level—so that the unit appears to have "stratiformity." (The term is borrowed from geology, to avoid the probably invidious connotations of the more familiar term stratification). In formal organizations, such stratiformity appears on organization charts and, often, through ranking by title: permanent secretary, undersecretary, assistant secretary, etc. In other cases, it is simply a matter of implicit perceptions: e.g., of parents or teachers by children and by themselves.

Stratiformity, in the above sense, is surely a characteristic of governments (as conventionally conceived) and of most other social units. On the other hand, there (probably) are social units in which avoidance of stratiformity is deliberately sought. More important, there are many interactions within a social unit that occur on the same level of its hierarchy and do not manifestly affect relations among the hierarchic levels. Examples are social relations among legislators or bureaucrats, or, in a small-scale unit, certain interactions, unnecessary to specify, between spouses or children in families. Very indirectly, these may, of course, affect relations among the levels (cocktail parties in Washington may promote voting coalitions, or the children's quarrels may turn mild parents into temporary tyrants). But these are usually remote and highly unpredictable effects, like those that encounters outside of social units might cumulatively have on more comprehensive units in which they occur. They certainly do not "manifestly" affect between-level relations in the sociologists's sense of the term, i.e., in the sense of having intended and recognized effects on social relations.²⁴ They are thus not in the least like interactions among nominal peers, such as members of a board of directors, that are intended to make rules for a social unit and recognized to do so.

The second criterion of discrimination thus narrows the field by subtracting interactions within social units that most resemble encounters outside of them, which seems desirable from the standpoint of disciplinary considerations. It also narrows it in a direction dictated by the focus of our subjective interests. But it hardly narrows it extremely.

(3) *Asymmetric relations within social units, occurring among or manifestly affecting between-level relations, may or may not be*

concerned with the direction of the units. Those concerned with their direction may be considered political, others not.

The "direction" of a social unit refers to four matters that are indivisible but distinguishable, and, in any event, have to be discussed serially. One involves a cluster of activities, the second a set of role-incumbents, the third a set of instruments by which the role-incumbents perform the activities, the fourth the activation of a set of sentiments toward the activities, instruments and role-incumbents.

(a) Direction as *activity* involves the general management of the unit: defining its goals and means to attain them; specifying proper conduct within it and, in a sense, "policing" members to assure that their conduct conforms to specification; allocating roles within the unit, together with special tasks, duties, or rights associated with the roles; and coordinating roles and conduct to achieve effective performance in regard to goals or, in a more diffuse sense, to minimize conflict and confusion.

(b) Conceivably all members of a social unit might participate equally in performing these activities on a continuing basis. However, such utopian "communal" arrangements rarely, if ever, exist, nor are they even closely approximated. (In any case, they would not be "asymmetrical.") In virtually all cases, one finds special *role-incumbents* who have assigned or assumed rights or responsibilities to see to the unit's "management," in the various senses of the term. Such role-incumbents may be called the unit's "directors." Their activities in the unit are partly analogous to those mechanisms of control in organic "systems" that keep the organism in equilibrium under stress or prevent stresses from arising in the first place. They are also partly analogous to the activities of engineers who define the objectives of mechanical systems and generate or manipulate structural capabilities and other resources to attain them.

Incumbents of such special roles are always, by virtue of their special rights or responsibilities, superiors of others. However, the term "superordinates" seems more apt a label for them. The obvious reason is that a superior may simply be thought of as higher in status than other unit-members without in any sense being a director in the unit—general status levels and levels of direction need not coincide, and ought not to be made to do so by definition. A superior becomes a superordinate when, in addition to nominal superiority, he exercises a considerable amount of actual control over the unit. Here too, however, a modification, especially stressed by Dahl and Lindblom,²⁵ is in

²⁴ Marion J. Levy, Jr., *The Structure of Society* (Princeton: Princeton University Press, 1952).

²⁵ *Politics, Economics, and Welfare*, p. 94.

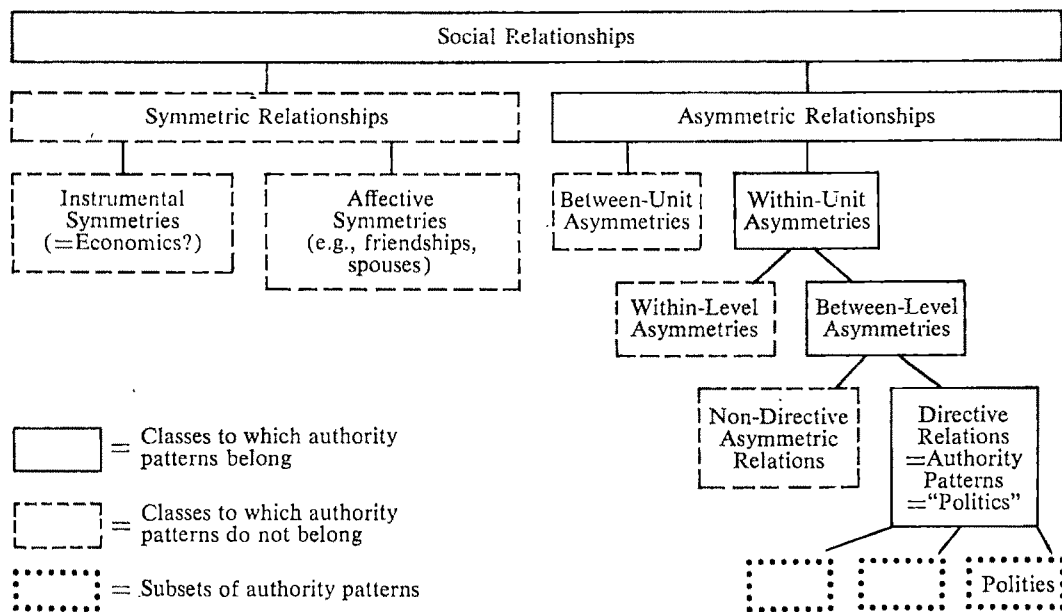


Figure 2. The Place of Authority Patterns in a Taxonomy of Social Relationships.

order. Flows of control between directors and those who are directed need not be, and in fact rarely are, entirely unidirectional. By any formal definition of control, the actions of subordinates can control those of superordinates no less than vice versa, either in latent effect or because manifestly meant to do so—i.e., meant to influence the superordinates' actions. Subordinate actions concerned with directives other than those that merely carry out what superordinates ordain are thus also part of a unit's structure of direction. Such actions may, of course, matter more or less. But even where they are of great import, superordinates have a special role in the determination of the unit's network of direction, either in the sense of quite autonomous power inherent in their roles, or in the sense of aggregating, selecting, or otherwise processing inputs from subordinates, or both.

(c) Direction is carried out by means of *directives*: prescriptions potentially backed by sanctions, negative or positive, if disregarded by unit-members. Directives may take the form of petty commands, broad rules, or implanting in members internalized norms reinforced by the use of deprivations when not acted out. The term "directive" is sometimes used by sociologists and psychologists to denote any guide to action that compels particular choices among alternatives or limits the range of perceived alternatives, and thus reduces the possibility of "anomie" as a psychic condition.²⁶ Here the sense of the term is narrower: not any guide to

action, including all habits and sentiments of morality and propriety, but explicit guides to action within social units, or sometimes interactions with other units in one's capacity as member of a unit.

(d) However, an important aspect of direction, the roles of directors, and the perception of directives is precisely that they do evoke special sentiments of morality or propriety; more precisely, they activate perceptions of *legitimacy*. Such perceptions can be either diffuse or specific. In the diffuse sense, they involve general perceptions of the rightfulness, or justice, of the overall constitution of a pattern of direction, hence its worthiness of being supported. In the specific sense, they involve sentiments of obligation to comply with directives because invested with a special moral quality derived from that diffuse sense of rightful constitution, and therefore of the propriety of punitive sanctions in cases of noncompliance which, otherwise employed, would be perceived as immoral. The perceptions need not be positive. All that is stated here is that, positive or negative, they are evoked. It is inconceivable that such perceptions might not be evoked where obligations are prescribed, where the ability to prescribe them is differentially allocated, and where the behavior used to police and enforce prescriptions is not regarded as morally rightful in any relationship, under all circumstances.

The third criterion of discrimination thus focuses attention on a special cluster of between-level relations within social units. It subtracts relations that are not concerned with their direction in the above senses, which are probably

²⁶ Sebastian De Grazia, *The Political Community* (Chicago: University of Chicago Press, 1948).

the great majority. The reasons for the subtraction again are both disciplinary and subjective. From the disciplinary standpoint, the important points are that "rule" in any form must have to do with rules or their obvious equivalents, and interactions that are not concerned with rules are both extremely heterogeneous (even within units, between levels of stratiformity) and lack attributes of those that are. The most manifestly lacking attribute is legitimacy: they do not activate perceptions of it, but only arouse general sentiments of morality which, in one degree or another, are modified when the quality of legitimacy is discerned. As for subjective interests, the essential point is that "direction," in all the senses given that term here—the management of a social unit, the existence of super-ordinate roles, the prescription and policing of directives, and the perception of special moral qualities that justify rights, privileges, and punitive behavior otherwise deemed immoral or amoral—is intrinsic to commonsense notions of government.

The Nature of Authority Patterns: A Summary

The special class of asymmetric relations delimited by these criteria of discrimination may be called *authority patterns*.

The accompanying figure portrays the way the class was worked out as an exercise in progressive differentiation.²⁷ As should be evident, it implies that some generalizations about social relationships as such should also pertain to "politics," as should some generalizations about all asymmetric relationships, as well as within-unit and between-level asymmetries. Thus, as we pass down the branch-points, an increasing number of generalizations about more inclusive subjects become pertinent to politics; however, the theoretical understanding of politics should also require a good many generalizations about the less inclusive class alone.

The case for delimiting the scope of politics at the indicated level of discrimination still has to be made; so far only groundwork for it has been provided. Before we proceed to that most critical part of the argument, however, two brief sets of remarks are still required. To prevent confusion, my notion of the nature of authority patterns should be contrasted with other uses of the term "authority" and, to make an extensive argument succinct, it should be more directly and summarily stated.

(1) The concept of "authority," like others

²⁷ I am aware that, as a taxonomy of social relationships, the figure (and the argument it represents) is incomplete. The reason is that I am not, at this point, concerned with all social relationships, only with the place of political ones among them. No doubt, however, this involves a weakness: a full, methodical taxonomy of social relationships conceivably might, but probably will not, look different.

of its ilk (power, control, influence) has had a checkered definitional history in social studies.²⁸ The term is probably used most widely as Weber meant it.²⁹ Weber, to my knowledge, never offered a detailed formal definition of authority, and his use of the concept is ambivalent in certain senses.³⁰ He does, however, consistently use it in one way different from the way it is used here. He uses authority as a special quality that an exercise of "imperative control" (in my terminology, direction) might or might not have: *positive* legitimacy. I use it as a label for a structure in which activities, roles, and instruments evoke legitimacy sentiments, *positive or negative*. This avoids some knotty problems eminently worth avoiding: how to measure levels of positive legitimacy; at what level of positive legitimacy "power" (or control, or what have you) becomes authority; whether directors can be "authorities" to some people but not others in the same unit, in regard to some directives but not others, at one point in time but not another; whether the absence of negative legitimacy perceptions equals the presence of positive ones—and many others with which no one has yet managed to come to grips. It is surely enough to know that perceptions of legitimacy are evoked to know also that one is in a special universe of asymmetric relations different from others (for which terms like power or control are quite serviceable).

(2) The proposed conception of an authority pattern can be summarized in a general formal definition, elaborated in more specific definitions that are needed to make sense of the general conception:

General definition:

An *authority pattern* is a set of asymmetric relations among hierarchically ordered members of a social unit that involves the direction of the unit.

Subsidiary and additional definitions:

1. *Social units* are collectivities of individuals that may be regarded as collective individuals (as discussed above).

²⁸ For a sense of the confusion surrounding the term, see Robert L. Peabody, *Organizational Authority* (New York: Atherton, 1964), chaps. 1–2, and Carl J. Friedrich, ed., *Authority* (Cambridge, Mass.: Harvard University Press, 1958).

²⁹ Max Weber, *The Theory of Social and Economic Organization* (New York: Oxford University Press, 1947), p. 153.

³⁰ Note, for example, the following ambivalence in Weber's use of the concept: (1) Authority is held to exist in many types of "corporate groups" (i.e., in our terms, social units) and to involve "the legitimate exercise of imperative control." An aspect of such control is the use of coercion regarded as rightfully exercised. (2) The *state* is defined by its "monopoly of legitimate coercion." Clearly (2) contradicts (1) unless (1) is confined to "stateless" societies. The ambivalence arises, in my view, from the wish—more typical of political scientists than of sociologists—to make too rigid distinctions between politics and other social units.

2. *Hierarchical order* exists where members of a unit are perceived as ranked in levels of superiority and inferiority.
3. *The direction* of a social unit involves the definition of its goals, the regulation of conduct of its members, and the allocation and coordination of roles within it. Direction is carried out through directives prescribed by incumbents of superordinate roles in the unit.
 - 3.1 *Directives* are prescriptions of conduct that evoke legitimacy sentiments. Such sentiments raise the question of whether prescriptions of conduct ought to be regarded as obligatory and rightfully backed by sanctions.
 - 3.2 *Superordinates* are any unit superiors who are assigned, or assume, special responsibility for arriving at directives and seeing to it that they are carried out. (In Dahl and Lindblom's terminology, they are both superiors in the unit and, to a substantial extent at least, its actual "controllers.")
 - 3.3 *Subordinates* are, residually, any other unit members. It should be noted, however, that in cases of complex stratiformity a member may be both superordinated and subordinated vis-à-vis different other members. Sometimes, in cases of complex differentiation along functional lines, a member may be both a "super" and a "sub" vis-à-vis the same other members, in regard to different activities—an ambiguity that disappears if role-incumbents rather than concrete individuals in all their aspects are treated as a unit's "members."
4. *Super-sub relations* (a self-explanatory term) are at the core of any authority pattern: there is no direction of a unit without a flow of directives and actions enforcing them from supers to subs, and cases in which subs do not try to influence supers in some degree and manner are probably very rare. But since supers generally interact in arriving at directives, either as peers or in a special stratiform structure of superordination, *super-super relations*, insofar as they "manifestly" concern a unit's direction, are also an aspect of authority patterns. And since subs may interact in trying to influence the directives of the superordinates, those *sub-sub relations* manifestly concerned with doing so may be considered a further aspect.

The Case for Equating "Politics" with Authority Patterns

My argument is that authority patterns, as delimited and defined in the two preceding sections, are a sensible point at which a decent trade-off between inclusiveness and homogeneity can be struck, and subjective interests reconciled with disciplinary considerations. That argument rests on three main bases. Each of these is addressed to particular difficulties and advantages of conceptions of the field's scope that others have proposed—plus some supplementary considerations that emphasize the sig-

nal importance of studying authority patterns, whether or not taken as defining the subject matter of the field.

(1) *The conception of political study as the study of authority patterns is sufficiently broad (deals with phenomena sufficiently commonplace and frequent of occurrence) to serve as a proper basis for inductive science.* That is to say, the conception serves the purpose of those who first attacked the narrow traditional delimitation of the field's scope, without incurring the disadvantages of the alternatives they proposed.

"Societies"—a term that refers simply to the most inclusive "collective individuals"—consist of very large numbers of such patterns in every case, some very simple and small-scale (e.g., nuclear families), others much more complex and extensive. True, authority patterns at their simplest are not so simple as "acts of control," being clusters of such acts. But nothing prevents "analyzing" them into simpler component units, in any of the ways by which any entity can be analyzed into components clustered in a certain manner: by distinguishing "dimensions" on which they vary, or "elements" of which they consist, or smaller-scale "particles" to which they are reducible. It clearly is unnecessary for inductive science to deal always with the most simple and irreducible phenomena that cluster in entities. In fact, virtually all the very "simple" phenomena of science (cells, chemical elements, molecules, atoms) turn out to be clusters of still simpler phenomena, and failure to arrive at the end of the chain of simplicity—if indeed there is such an end—has not handicapped other fields of inquiry. The main requirement surely is that numbers be sufficient to offset problems arising from range of variability (as argued above), and it is hard to see, *a priori*, why in the study of authority any serious difficulties should arise on that score.

(2) *Defining the scope of the field as the study of authority patterns does not make it so broad as to take us far beyond the range of subjective interests, nor confront us with phenomena so heterogeneous as to make informative theory unlikely or to require much in the way of special disciplinary tools for special types of authority patterns.* That is to say, the definition retains the principal advantages of the traditional narrow conception of the field's scope, without incurring its manifest liabilities.

The methodological reasons for including far more within the scope of the field than state-organizations (and very near equivalents) are important, but by no means definitive. If such organizations were in fact as unique as some consider them, it would be self-defeating to subsume them under a more numerous set of

cases: what is gained methodologically would perforce be lost in knowledge of the subject matter. But state-organizations, and near-equivalents, are not all that unique. They do differ from other authority patterns but only on a very limited number of variables, the import of which for methods and products of inquiry is unlikely to be large.

For one thing, the idea of sovereignty is just that: an idea—and an idea more normative than empirical. The idea characterized well the pretensions of sovereigns as expropriators of authority, and sometimes justified their successes; it also served the longings for peace and harmony of those caught in struggles between competitive, overlapping authorities. But even if indivisible in principle (we leave that to political philosophers), it has never really been undivided in practice—certainly not if taken to involve the autonomous power to make laws perceived as binding and to enforce them with coercion perceived as morally permissible. Being on the official statute books or civil and criminal codes is not all that makes directives appear obligatory. Virtually every social unit has its own set of directives, mostly unchecked by more inclusive authority, and enforces them with more or less severe punitive sanctions, often of a kind considered impermissible among individuals outside of the confines of the unit. And even if there may be appeal to higher authority, that does not much change the nature of the activity. Normative theorists, like the “pluralists,” were the first to perceive this, wishing to deny any unique normative position to the State; empirical theorists ought to follow suit, to deny it any claim to autonomy as subject matter for a discipline.

When comparing state-organizations and other authority patterns subsumed under them by the idea of sovereignty, one finds that the dimensions on which they vary largely coincide. An “analytic scheme” for the less inclusive patterns serves well as an analytic scheme for the more inclusive; a typology of them is serviceable as a typology of “states” as well. We shall really only see this clearly in a set of subsequent papers (now in preparation) devoted to the analysis of authority patterns into constituent dimensions and an attempt to synthesize the analytic elements into clusters serviceable as typology. But we already know from the way the conception of authority pat-

terns—and the probably related variable of complexity. Whether these factors make much difference in regard to disciplinary considerations (concepts, methods, problems, theories) is at least an open question that ought not to be settled by definition, especially by a definition that incurs manifest handicaps for inductive science. Besides, states and near-equivalents vary considerably in extensiveness and complexity, and some “private” governments are certainly larger in size and more complex than many public ones. Size and complexity thus seem more useful as potential explanatory variables (as in a forthcoming study by Dahl and Tufte) than as defining characteristics of a field.

If this general argument is correct, then both parts of the general argument above follow: we do not go far beyond the focus of subjective interests in dealing generally with authority patterns, nor define a subject that is vastly heterogeneous, or even significantly more so than the original basis of the field—taking heterogeneity in the literal sense: not “different” (any concrete individual differs from all others) but “belonging to different genera.”

(3) *The conception of political phenomena as authority patterns helps to identify the equivalents of state-organizations where such organizations do not exist or are not highly developed.* That is to say, the conception serves the purpose of the functional alternatives to the traditional structural conception of the scope of the field, while avoiding the liabilities of purely functional conceptions.

In a sense, all authority patterns are “equivalents” of state-organizations, i.e., belong to a class to which the latter also belong and which, although numerous, discriminates a great deal, since it does not include most human relationships, symmetric or asymmetric. Authority patterns are equivalents of state-organizations even in the various functional senses that have been proposed: they maintain order, integrate, define, and try to attain goals. Moreover, if scale as such is the critical variable that distinguishes “states” from social units included in them, no difficulty arises in answering the question that the functionalists take as their point of departure: “What is the nearest equivalent of state-organization in societies where it does not exist—in, say, Nuer society, or among the Mayas?” No need at all to resort to “functional” equiva-

avoid dubious elements of their definition, such as that they are sovereign wielders of authority, or monopolize legitimate coercion, or alone perform order-maintaining functions in societies.³¹

There is nothing wrong with making a specialty of the study of politics in that sense, or of other subsets of the more inclusive class. Definitions of scope are intended for disciplines as a whole, not for any and all who work in them. What is regrettable is not specialization within a field but equating the whole of the field with the specialty, so that avoidable liabilities are incurred and closely similar subjects neglected.

(4) *Neglecting the study of nongovernmental authority patterns has the consequence not only of leaving unstudied much that is interesting and important in itself, but also of leaving the study of "polities" (however defined) uninformed by much that can be extremely useful for understanding them.* That is to say: even if authority patterns were not taken to define and delimit politics as a discipline, there would be weighty reasons for political scientists to extend their scope of attention to such patterns in all forms. Some of these reasons, however, would not be as weighty as they seem if governments as a special category were vastly more different from other authority patterns than I consider them.

It has often been argued that "private governments" should be far more widely and intensively studied than they are. The reason generally given is not that they are sufficiently like "public governments" to form a subject matter for a single discipline. It is simply pointed out that they exist; that they are interlaced with public authority; and that they are highly consequential—not least for the network of governmental directives that constrain individual lives, provide privileges and rights, and impose obligations, duties, deprivations. The argument, unfortunately, has not been widely acted upon in practice. There are exceptions in political

science, but they are out of the ordinary.³² There are more in other social sciences, particularly in family and industrial sociology, but they too are hardly commensurate with the extensiveness and import of the subject.³³ The consequence, needless to say, is conceptual, methodological, and theoretical retardation. As to reasons for the neglect, there seems to be a growing tendency to consider it part and parcel of a general conspiracy to conceal the networks of power and privilege that dominate supposedly "liberal" societies. Maybe so; but no demons are needed to explain the void. Just as likely is another possibility, i.e., that areas of disciplinary overlap may engender voids as well as duplications. On this hypothesis, political scientists neglect private governments because they are concerned with public ones and do not like to wander far outside their boundaries; other social scientists neglect them because their interests are broader, because they have no specialized competence to analyze them, or because they consider them to fall within the political scientist's domain. One can expect that the current sense of a "crisis of authority" in all institutions in all advanced societies—plus the sense that all sorts of nebulous powers dominate them in uncharted ways—will engender studies that decrease the void. But, on the evidence, studies with normative axes to grind are as likely to retard, or divert, disciplinary advances as to promote them, and they are most fruitful when informed by study conducted from a more neutral basis.

As long as systematic studies of nongovernmental authority patterns are few and crude, we are deprived also of knowledge potentially very useful for making sense of governmental patterns—in at least three ways: (a) Despite pitfalls when one extrapolates from one level of analysis to another, there is much to be learned about macrocosms from microcosms.³⁴ The smaller units, moreover, often accommodate better methods of study, and are more accessible to study than the larger ones. One would suppose that the more the microcosm resembles the macrocosm, the less dangerous the pitfalls and the more directly pertinent the things to be

³¹ The notion of extensiveness in fact figures in some definitions of political systems. The most explicit example is Apter's: *Polities are the most generalized membership units of societies with defined responsibility for maintaining the societies and enjoying a monopoly of coercive power.* Other definitions imply the same more awkwardly: e.g., one of Almond's definitions which includes the notion of an "autonomous society"; or the much earlier one of MacIver which includes the notion of a "territorially demarcated community" among numerous other definitional attributes; or Dahl and Lindblom's notion of government as having "the last word" over other organizations in an "area." See Apter, "A Comparative Method for the Study of Politics"; Almond and Coleman, *The Politics of the Developing Areas*; Robert M. MacIver, *The Modern State* (London: Oxford, 1926), p. 22; Dahl and Lindblom, *Politics, Economics, and Welfare*, p. 42.

³² See Dahl, "Business and Politics," for documentation in one area of private authority.

³³ I refer here to studies mainly concerned with the systematic study of authority patterns—such as that of Peabody. Authority being ubiquitous, references to authority patterns, or potentially relevant materials, may be found in virtually all studies of social institutions, but chiefly in their interstices, between the lines, in connection with other matters, without special emphasis or systematic treatment.

³⁴ A useful work to consult on both points is Sidney Verba, *Small Groups and Political Behavior: A Study of Leadership* (Princeton: Princeton University Press, 1961).

learned by extrapolation. (b) If Easton is right, as he clearly is, in saying that political study (in his sense, discussed briefly in an appendix to this paper) could profit from a general theory of power, then it could manifestly profit even more from a general theory of authority patterns—not, in this case, by extrapolation from one level to another, but by the partial application to less general phenomena of theories about more extensive ones. (c) Even if governmental authority patterns are separated from less extensive ones, the latter are likely to be important explanatory variables for aspects of governmental authority, e.g., on the basis of learning processes, or because they provide links in other senses (mediate between) governmental and strictly nonpolitical variables, such as economic ones, social status or mobility, and so on. I have made an extensive case for using nongovernmental authority patterns as explanatory variables (in theories of stability in democracies or, in a more general sense, performance levels of all kinds of government) in other publications; interested readers should consult these for a full statement of the argument. The general point, however, does not rest only on my arguments. Nongovernmental authority patterns have been used, implicitly, as explanatory variables for governmental ones in hosts of work, and have been so used explicitly at least as far back as Montesquieu, or indeed Confucius.

Note that this delimitation of "politics" is not wholly unprecedented. One can find at least one (implicit) anticipation of it in the modern history of political thought. It is mentioned at this point partly to avoid misimpressions about claims to originality and partly because, indirectly, it adds to the force of my argument. The anticipation (briefly alluded to earlier) occurs in the works of the so-called "pluralists" (Figgis, Laski, Cole) of an earlier period. The pluralists argued, in gist, that the state's claim to sovereignty did not really give it a special *moral* status relative to other social structures. It is true that they were not concerned with the scope of politics as a field of study. They were normative philosophers. Besides, political science hardly existed as a special "field" when they first wrote. However, their arguments did rest on doubt about the very existence of something properly considered "sovereign," or, better, doubt that the special attributes of structures claiming sovereignty made much, or any, moral difference.

How does this reinforce my argument? Simply in that, if the same moral problems (e.g., of rights, obligations, liberty) are raised in a variety of social units, it is probable that descriptive similarities among them are also great. Put differently: if apparently diverse phenomena

pose largely the same problems, moral or otherwise, we may take this as a powerful indicator of their being, despite appearances, members of a common taxonomic class.

Politics, International Relations, and "Political Economy"

Objections to my position are less likely to concern what it adds to the conventional scope of political study than what it seems to subtract. Two apparently subtracted subjects are especially likely to engender demurrals: (1) relations among polities and (2) the nonauthoritative, in general symmetric, processes central to "economic" analyses of politics, such as political bargaining, political coalition-formation, and the adjustment of governmental programs and party positions to voters' preferences.³⁵

These are certainly among the more common subjective interests in political science departments. Moreover, a reasoned case for including both in the political scientist's field of attention can certainly be made. It seems to me, however, that a stronger case can be made to the effect that their elimination from "politics" here is either more apparent than real or else inconsequential. This needs elaboration.

(1) We all know that the study of *international relations* has long played a major role in political science departments. Reasons that this should be the case, as long as polities are central to our concerns, are not hard to find; above all, it is arguable that linkages between the internal affairs of polities and relations among them are such that neither can be properly understood without the other.³⁶ That argument is somewhat intricate, but the basic position is obvious enough to need no spelling out.

The first point to note by way of rebuttal is that nothing said here rules out the study of international relations. Anyone interested in that special subject can still pursue it.

Beyond this, aspects of international relations, as conventionally conceived, are not excluded by the proposed delimitation of Politics—particularly two in which "linkages" are ignored at most obvious peril. One is the study of supranational integration. Authority patterns

³⁵ Much of the recent literature on this subject is summarized in R. L. Curry, Jr., and L. L. Wade, *A Theory of Political Exchange* (Englewood Cliffs, N.J.: Prentice-Hall, 1968), and some of its anticipations are summarized in James M. Buchanan and Gordon Tullock, *The Calculus of Consent* (Ann Arbor: University of Michigan Press, 1962), pp. 323–340. In a sense, most of the literature on political competition, anyway in democracies, is concerned with the subject, even if economic postulates or methods of reasoning are not explicitly used.

³⁶ See, for example, James N. Rosenau, *Linkage Politics: Essays on the Convergence of National and International Systems* (New York: Free Press, 1969).

above the national level fall within the field's scope no less than those below it. In addition, the study of authority patterns can certainly accommodate studies of how they are formed. Every field theorizes about statics and dynamics, and the study of political dynamics is most fundamentally concerned with the processes by which political units come into, or pass out of, being. Processes of international integration surely are processes that develop directive, asymmetrical relations, within embryonic social units, out of relations falling into other taxonomic categories. (That applies to the formation of nation-states themselves no less than processes that, some argue, are now taking us "beyond the nation-state.") The second aspect still manifestly included in politics is, of course, foreign policy, especially insofar as such policy has directive consequences for members of polities.

Nevertheless, other aspects of international relations clearly cannot be squeezed into the proposed delimitation, without procrustean distortion. They fall outside it because they are symmetric in character—more akin to economic market relations than to authority relations; or because they involve between-unit asymmetries; or, farthest removed from our conception of politics, because they involve only indirect impingements rather than "relationships" in the sense of interactions. These aspects of international relations are also "linked" to within-unit affairs, and vice versa; and it is in regard to them that potential disciplinary losses (not losses in status and the like) become serious.

There is reason to think that the losses would not be large, on either side. The disciplinary boundaries between politics and economics, after all, have not prevented studies that link political and economic factors or political studies using economic postulates and methods. Neither have those between politics and sociology, politics and psychology, politics and anthropology, prevented the development of political sociology, studies of political socialization, or studies of primitive political systems. Nor has the fact that economists study markets and sociologists study corporations had very deleterious consequences. Serious scholars who feel compelled to cross disciplinary boundaries do so, paying the price in time and effort. In the case of studies of politics and their relations, the price will certainly not be as great as in many others: neither presently requires mastery of particularly exotic skills or peculiar talents. Moreover, on the evidence, interdisciplinary programs can be useful substitutes for departmental identities, and have the advantage

of allowing one to choose by reasoning rather than brute convention among the disciplines to be related.

Here we come to gains that may offset any losses if departmental divorces occur. It is by no means certain that specialists in international relations are best off as general political scientists. At the student, or even faculty, levels they might be better off devoting more time and other resources to studying international relations as such. They might also learn more that would help them make sense of their subject from other fields (e.g., economics, anthropology) than they presently do from various subdivisions of political science—many of which departmental rules and teaching loads force upon undergraduates, graduate students, and teachers. Given the manifest taxonomic differences between politics and international systems, and the resemblances of the latter to other phenomena, this is, in fact, very likely to be the case. By the same token, as already argued, those who study politics may learn more from studying authority patterns at lesser levels than from the study of international relations: (I say "may" because I do not know enough to say "will." Just for that reason, however, choices should be open, not foreclosed.)

One is struck, in this regard, by the paucity of theories, or even mere theoretical approaches or frameworks, that apply both to politics and to their relations (as distinguished from their linkages), except at very general and uninformative levels. This is precisely what one should expect if taxonomic differences are considerable. And yet, simply because scholarly convention and administrative habit identify international relations and politics, people continue to devote sweat to distracting exercises that might justify the habit. They search for definitions of politics that might cover both international and intranational affairs (not hard to do, but also not very useful). They look for any signs in international activities indicating that they are not absolutely different from intranational activities (also easy, since, of course, they are not). They try to develop perspectives of inquiry, theoretical approaches, and theories that might apply to both sets of phenomena (more difficult to bring off, but certainly also possible at a sufficiently inclusive, and heterogeneous, level). The results are often trivial or embarrassing. It would seem less wasteful, on the face of it simply to work with one's materials, allowing any correspondences to emerge naturally from the work, than to strain after commonalities simply so that habit might appear as reason.

(2) Objections to the possible elimination

from "politics" of *symmetric relations that figure in the making of directives* (as well as in processes of recruitment to superordinate roles) should be taken more seriously. For one thing, such relations are what "politics" is often taken to refer to in general usage—e.g., when distinguished from government or administration. More important, the relations involved do not just play a momentous role in state-organizations; they can occur in any and all authority patterns, and probably do in most.

To disarm objections on this score, it should suffice to say that the study of the direction of social units clearly includes the study of processes by which directives are arrived at, carried out, observed or defied, as well as processes by which those who carry out direction are recruited to their roles. The notion of asymmetry merely provides the first of several criteria of discrimination by which the concrete phenomena of the field are identified. If symmetric relations occur among members of concrete units identified by these criteria, and if these relations are manifestly concerned with the direction of the units (in short, intrinsic to authority), they fall within the boundaries here delimited—just as asymmetric relations among members that are "nonauthoritative" fall outside them.

The elaboration of the general definition of authority patterns (above) points to two aspects of the patterns likely to involve symmetries. One is relations among superordinates (*super-super relations*). Superordinates may work out directives by vote-trading, "side-payments," and other such modes of bargaining to form winning coalitions.³⁷ Such collaboration is not always corrupt or cynical, or even just utilitarian. It would seem that there are cultures in which particular value is attached to collegial behavior and consensual decisions. In cultures such as these, symmetry among superordinates is a general end rather than merely a means for optimizing particular utilities. (It is manifest also that the more a social unit's tacit or explicit decision-rules require assent by numerous participants, the greater the number and significance of symmetries in decision-processes will be.) Symmetries may, of course, also occur in *sub-sub relations* intended to "influence" direction. This does not refer to equal power to influence superordinates. It refers chiefly to exchanges and compromises among subordinates for the sake of forming cohesive influential groups.

Despite all this, it must be granted that the

³⁷ For a list of such possible "exchanges," see William H. Riker, *The Theory of Political Coalitions* (New Haven: Yale, 1962), pp. 108–114.

inclusion of symmetric relations in "political" study introduces an element of dissonance, or impurity, into the proposed conception of politics. Symmetries remain symmetries, no matter how justified their inclusion in studies of asymmetric relations and no matter how often one says that they must manifestly affect asymmetries of a particular kind. The conceptual dissonance can be tolerated. But it should be realized that relations in authority patterns which are not themselves authoritative will at least require theories different from those that are.³⁸

It is altogether likely that the theories required will substantially resemble, or coincide with, those of economics—the field preeminently concerned with "instrumental symmetries" in social relations. (They may also resemble certain theories of international relations, if and when properly formulated.)³⁹ The adaptations of economic theories to political relations are, in fact, already well-developed in a new "political economy" that better merits the label than the old (see note 35). These theories link two fundamentally different, but intertwined, aspects of organized or institutionalized human relations: those in which humans are equals and exchange and those in which they are hierarchically ordered and direct or are directed.

Proponents of economic theories of politics often justify their work on grounds that beg questions one need not beg. They claim that "human nature" is inherently Hobbesian or in some more benign sense value-maximizing, and hence that politics and economics must be governed by the same laws. Or they rest their case on the claim that methodological imperatives (parsimony, the deductive derivation of propositions, predictive or explanatory power) somehow compel working with economists' methods on economists' postulates. All this is probably untrue, and surely unnecessary. The political economists need only claim that concrete political structures and processes contain relations of the sort economic theories are designed to handle, so that such theories are a useful basis (prefabricated, as it were) for partial accounts of politics. How partial or comprehensive these accounts can be is best left to be settled, *a posteriori*, by competition between contending approaches.

This argument has several further implications of consequence.

³⁸ This, in turn, should not really bother anyone. No one has ever held that theories of politics can avoid all nonpolitical factors.

³⁹ If theories resemble both those of economics and international relations, these two bodies of theory should resemble one another. In this connection, note that "entrepreneurial," i.e., economic, theories of international relations are now beginning to emerge. (A good

It manifestly implies that the contemporary political economists' own fixation with state-organization and processes intimately associated with the state (e.g., party competition) is ill advised. Their theories pertain to processes that may occur in any and all authority patterns. Many of these, moreover, are far more suitable to, and accessible for, testing their models—an activity for which they, like other political scientists, are not notable—than are political structures as conventionally conceived.

A second implication is that we should start to develop theories of asymmetric relations—particularly those that occur within social units, between levels, and involve “direction”—as rigorous and fertile as those of instrumental symmetry. Such a counterpart to economic theory certainly does not presently exist even in embryonic form, and the failure of political scientists to define their subject matter surely explains, in large part, why this is the case. In turn, the lack of a powerful “pure” theory of politics helps explain why contemporary political economy is so much more economic than political.

A third implication is related to this point. My argument implies that one should think of “political economy” in two ways, and work toward much greater symmetry of emphasis between them. There is, on one hand, the economics of politics: the study of symmetries in authority patterns. There is, on the other hand, the politics of economics: the study of authority in symmetric relations. I am not referring here to Dahl's still largely unheeded call for the political analysis of business firms as “political orders.”⁴⁰ The subject is much larger and more subtle. It entails study of the ways authority impinges upon symmetry in any and all social units and relations—not excluding economic markets themselves. Except for economists' theories of monopoly and oligopoly, that subject is much less explored than are economic theories of politics. Assuming that the necessary theoretical tools are developed, a political Downs roaming through the economists' domain might, just conceivably, bring off some eyebrow-raising results.

Appendix:

A Note on Easton's Conception of the Political System

The point of this paper, that authority patterns should define and delimit political study, is (as stated in the text) not claimed to be

example is Norman Frohlich and Joe A. Oppenheimer, “Entrepreneurial Politics and Foreign Policy,” *World Politics*, 24 (Supplement, 1972), 151–178.

⁴⁰ Dahl, “Business and Politics,” pp. 3–9.

wholly original—only persuasive. As well as being anticipated by normative philosophers, and following up arguments for studies of “private” governments as important influences on public ones or as not greatly dissimilar from the latter in nature or impact on individuals, it is also in line with points made by many contemporary critics of liberal democracies—especially their argument that liberal governmental institutions conceal oppressive networks of power that act through them or directly constrain individuals. In effect, it simply spells out explicitly what is largely implicit in all these views and rests the case for equating political inquiry with the study of all authority patterns less on normative grounds than on “disciplinary” considerations.

Even in this regard, the argument might appear already to have been developed by David Easton in *The Political System*. Some comments on points of difference between Easton's position and mine are therefore in order.

Easton defines “political acts” as those that “authoritatively allocate values in a society.”⁴¹ A social structure, therefore, is political if it engages in the authoritative allocation of values. The operative term “authoritative” corresponds to mine, and Easton's position, like mine, was worked out by criticism of the very narrow and very broad structural conceptions of the subject matter of the field, to some extent on similar grounds. (The “functional” alternative was not considered by Easton, since it had not yet become current when he wrote—although he also ignores it in his later work.)

If my argument only restated, in a somewhat different way, Easton's position, it could still be justified on the ground that Easton's influence on the conception of the field has hardly been great—except for the mouthing of definitions—and can stand repetition and reinforcement. Actually, however, resemblances between Easton's position and mine are illusory. Easton's conception of politics (as distinguished from his systems approach to political study) has had no discernible impact on what political scientists do, for the good and simple reason that it really only presents a new verbal way to characterize the conventional conception of the field.

That Easton's position remains closely tied to the notion of political structures as state-organizations, or very near equivalents, is evident in this crucial passage:

... in spite of the fact that for the members of [organizations that are not society-wide their] policies carry the weight of authority, it is at once ap-

⁴¹ Easton, *The Political System*, p. 134 (my italics).

parent that political science does not undertake to study these policies for their own sake. Political science is concerned rather with the relation of the authoritative policies, made in such groups as associations, to other kinds of policies, those that are considered authoritative for the whole society. In other words, political research seeks first and foremost to understand the way in which values are authoritatively allocated . . . for the whole society.⁴²

Processes of direction, directive roles, and internal directives in less comprehensive groups thus remain beyond the disciplinary pale. They are considered only as substantive inputs into "polities" (as defined above)—as parts of the "super-sub" relationships of the most inclusive authority patterns, or matters of boundary-exchange between politics and nonpolitical structures.

Why does Easton delimit the field in a manner so closely tied to a conception of it that he attacks? He does it, in the final analysis, because he only takes into account our conventional subjective interests, not any disciplinary considerations—except when he attacks alternatives. At all points where his restrictive conception is stated he invokes as support for it only that political scientists are "not concerned" with a more broadly defined subject matter. This is an explanation of what they do, but hardly a reason for doing nothing else. Besides, it is manifestly untrue, because some of them actually are concerned with more, and it says nothing about what their concerns ought to be. Values are allocated by the "policies" of all groups;⁴³ indeed one can go further and hold that values are allocated in some sense not just by "policies" but in all social relations (asymmetrically or otherwise). Therefore, that part of Easton's definition of the subject of the field which refers to value-allocation is clearly gratuitous. Since all policies of all groups may be "authoritative," that part of the definition of politics also delimits nothing specifically political—a point that should obviously have given Easton pause about making "society-wide" relations the only attribute that discriminates between political and nonpolitical phenomena.

It is not widely realized that the operative part of Easton's delimitation of politics is society-wide relations, nothing else. More attention has been paid to the redundant parts—which appear to say more than the crucial part of the definition. But Easton's own subsequent work confirms my argument: nowhere has he concerned himself with anything other than governments in the conventional sense. My posi-

tion can thus be taken as one to which much of Easton's preliminary argument about the nature of political phenomena leads but that Easton himself never reaches. In that sense, it does not reiterate his views but extends them over a rather similar route toward a different destination.

(Readers might note, in this connection, a curious point about Easton's use of sources. In two places, he cites Sir George Cornewall Lewis's *A Treatise on the Methods of Observation and Reasoning in Politics*, published in 1852—a book that precociously anticipates many current methodological issues in political science. He does not cite Lewis's *The Influence of Authority in Matters of Opinion*, published three years earlier, which attempts to demonstrate that authority pervades all of social life, all groups (as DeGrazia puts it)⁴⁴ having their own "small common goods" and "small authorities" that "envelop us at birth and clothe us throughout life." Easton may simply have missed that work. It is not hard to miss it, for its influence has been virtually nil. The fact remains that it comes closer to what I argue than does Easton's argument itself.)⁴⁵

⁴⁴ De Grazia, *The Political Community*, p. 330.

⁴⁵ Ten years ago I wrote an essay on the development of "comparative politics" from its remote ancestry in Aristotle to the present. (See "A Perspective on Comparative Politics," in *Comparative Politics: A Reader*, ed. Eckstein and Apter [New York: Free Press, 1963], pp. 3–32.) The essay concluded with a problem to which, at the time, no solution had been worked out. As long as political studies emphasized formal-legal aspects of state-organizations they had a certain focus and simplicity. The tendency in the field, for good reasons, was to deemphasize that subject matter. But the result of doing so was loss of focus and simplicity and, as a result, a loss of bearings in regard to what the field was all about, especially as a field of specialization. Political scientists often did not practice what they preached: they continued, in their work, to deal chiefly with state-organizations, while disavowing the reasons for doing so (in the manner of Easton). Or, if they did follow their own prescriptions, they seemed to equate politics with "the whole of social life in all its facets." The problem, then, was to find a different, but still focused and relatively simple, conception of the subject of the field.

The present essay presents my answer to that problem. It also implies that there are two fundamental sciences of society: that dealing with symmetrical social relationships and that dealing with asymmetric ones in social units—"economics" and "politics." Between these, a comprehensive science of social interaction must build bridges, of course, as must many particular explanations of particular social occurrences. Since the science of symmetrical interaction is well-developed (and suitable to what are generally considered "hard" methods of thought and inquiry) there has been a tendency recently to develop "economic" theories applicable to political phenomena. This is a useful tendency. But it would be more useful still if a concomitant effort were made to develop "political" theories of economics—and of politics.

⁴² Easton, p. 134.

⁴³ Easton, p. 130.

National and Local Forces in State Politics: The Implications of Multi-Level Policy Analysis

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American national and state governments influence each other within a federal arrangement. This process often dominates state politics but is largely ignored in the study of states and their policies. The first part of this article points up some of the deficiencies in the current knowledge of states which stem from the ignoring of federalism; the second part suggests a refocusing of our studies around the federal arrangement.

Level Problems in the Study of State Politics

The variety of levels of American governments presents some difficulties in studying any one level, especially the state level. Substantively, these difficulties revolve around the political processes associated with the relationships between governments. Technical problems derive from the generic difficulties of level-of-analysis alternatives, of which the most familiar are ecological fallacies. For the main elements of the contemporary study of state politics, the level difficulties are considerable.

A noticeable quirk of most state studies is the treatment of states and only states as policy units. For almost any state policy, quite a number of influences go into and out of policy, and it is doubtful that most of these can be attributed to "states." For instance, most highway building and welfare giving involves the federal government's determining basic parameters and local agents' influencing the applied policy; neither of these factors can be treated in a state-only analysis of the policy, though the actions of state governments are not particularly meaningful abstracted from federal-state-local interaction.

The use of states as units implies that some attribute of the state determines or causes policy, or at least policy differences, among the states. It is not enough that the policy merely "pass through" the state or that state differences are statistically related to policy. The "necessary condition" of having states or state differences for policy does not make states the determinants of policy. If Lyndon Johnson as president pours federal money into Texas or a Washington bureau decides to allocate grants-in-aid on a per capita basis, state policy differences cannot legitimately be traced to the states themselves. Because less than one-fifth of the

money passing through the hands of state governments starts and stays at the state level, state differences are *ceteris paribus* not likely to be tied mainly to state governments. Conclusions about policy or even about state policy based on a simple state-level of analysis have a substantial chance of being wrong.

Furthermore, the type of systems model used in state analysis often interacts with the single-level approach to produce a problem of spurious correlation. The systems model usually examines the interrelations of some subset of past government policy, social and economic parameters, political representation processes, bureaucratic attributes, policy, and policy impact. The beginning assumption is that these factors are independent; actually, nonstate-level phenomena often cause several of the factors at once. The proposed revenue-sharing act, for instance, includes a "tax effort" provision, so that the act would affect both state tax law and state expenditures, producing a correlation which may depend on federal, not state, policy. Poverty funds allocated on a "per poor person" basis have been shown to produce a relationship between state income and state welfare expenditures.¹ Similarly, the lack of relationships can be traced to nonstate factors, as when the federal courts stop states from correlating the black portion of their population with low election turnout.

The systems approach can introduce Galton's problem into relationships: diffusion produces similarity on many variables but there is really only one event occurring—the diffusion itself.² Walker found a host of socioeconomic variables reflecting perhaps one or two actually different factors—related to 88 policies (largely reflecting a single policy innovation factor) for 48 states, which for policy purposes were equivalent to five regions.³ The thousands

¹ See Andrew T. Cowart, "Anti-Poverty Expenditures in the American States," in *State and Urban Politics, Readings in Comparative Public Policy*, ed. Richard I. Hofferbert and Ira Sharkansky (Boston: Little, Brown, 1971), p. 415.

² For a concise discussion of this problem, see Adam Przeworski and Henry Teune, *The Logic of Comparative Social Inquiry* (New York: Wiley, 1970), pp. 51–53.

³ Jack L. Walker, "The Diffusion of Innovations Among the American States," in Hofferbert and Sharkansky, pp. 377–412; a somewhat revised version of this

of relations among these variables strongly reflect a single relation of five items—the association of region and innovation.

The diffusion process itself complicates the understanding of relationships. Nearly 100 national and regional associations of governmental officials diffuse not only policies but also relations between policy and other variables, as between population and form of local government, between taxes and economic structure, and between population density and fire prevention policy. It is usually unclear whether strong relations would exist without the associations, without other state governments as pacesetters, or without bureaucrats oriented to out-of-state events, but it appears that interstate correlations depend on interstate communication in addition to the within-state factors presented in the systems approach.

The information employed in a state analysis characteristically is a single parameter for a single variable. That is, a single score represents the state for each item. Thus, AFDC welfare expenditures in a state, which comprise innumerable payments more or less closely conforming to a multitude of rules, are often indicated by the average payment per recipient. It is rarely clear whether this score is a policy, if so, a state policy, if so, indicative of the entire AFDC state policy. There is considerable room for argument over what constitutes a policy: the rules, the application of the rules, the government actions, the intended consequences, etc. The payments are measured from actions; because in different states the same rules can lead to different actions or different rules to the same actions, the payments do not indicate directly the policy rules, nor would the rules measure policy actions. If analysis is limited to policy actions, it is not always clear that the actions can be attributed to the states. Local governments do most of the welfare paying, for instance, so that the measure of state policy might be best described as the average payment by the average local government. The averages themselves may be misleading. Relatively little

article, found in *Politics in the American States*, ed. Herbert Jacob and Kenneth N. Vines, 2nd ed. (Boston: Little, Brown, 1971), pp. 354–387, indicates (Tables 7, 8, 9) that about half the diffusion depends on four states, one in each of four innovation regions, which provide innovations to states. It would appear, though it is not entirely clear, that California and New York both provide policy models for virtually all other states, that the regionalism largely occurs in accepting innovation from what might be called second-order innovators—Wisconsin, Florida, Illinois—and in the sequence of diffusion (rather than the ultimate source of innovation).

policy action consists in paying the average amount, and the average is indicative of all action only when the distribution of actions around the average is known and is the same everywhere. The within-state distributions are rarely reported or even calculated; where they have been reported, the distributions vary with the policy and differ considerably among the states.⁴ For example, the county average monthly general assistance payments vary around the state average 75 times as much in Massachusetts as in South Carolina, while the state averages show only about 6 per cent as much policy difference. The averages do not index the policy, and the averages may not be the important source of policy differences.

The type of score representing state policy presents statistical problems. In one major study, 88 of 94 variables used were means, percentages, per capita, or otherwise severely altered scores, most commonly derived by dividing one variable by another.⁵ Statisticians are leery of using such scores in a complex analysis, especially one involving powers, roots, and differences, as state policy analyses normally do. The intention of the alteration is to control for some influences, such as size of population, in order to examine other relations more clearly. The division is an inappropriate control: standard least-squares regression should be used, and it should be used on *all* the variables influenced, not just a select few.⁶

The general result of using automatic controls for the impact of population size on policy without a discussion of that impact is to hide a major source of policy differences. California and New York, for instance, are conceded by state administrators to be the primary sources of diffusion, providing models for over one-third of the important, new, and better state

⁴ See Sharkansky and Hofferbert, "Dimensions of State Policy," in Jacob and Vines, p. 346, and Sharkansky, "State Administrators in the Political Process," in Jacob and Vines, p. 266.

⁵ Thomas R. Dye, *Politics, Economics, and the Public* (Chicago: Rand McNally, 1966), pp. 303–309.

⁶ It is standard statistical practice to control for the influence of a third variable on both variables in a relationship. Controlling for effects on just one of the variables produces anomalies, especially in the correlation coefficients. Controls reduce the variance in the "dependent" and "independent" variables by the amount of covariance (weighed) with the "control" variable. The reduction in the variances properly adjusts the base—the estimated maximum covariance—against which the independent-dependent covariance is measured in a correlation coefficient. When only one of the variables is controlled, only one of the variances is properly reduced, leaving an established maximum covariance which is artificially high, so the correlation coefficient is estimated to be lower than it is.

programs.⁷ The reason for their policy leadership lies first in the simple scale of government: they are the largest states. While other forces also influence innovation and diffusion, any explanation leaving out the effects of population size on scale of government and thus on innovation and the other variables is inadequate. Using a population-based percentage measure, as in percentage urban, is not a good control for population: large states are disproportionately urban. The percentage measure ends up partly measuring population size, and the interpretation of its relation with policy becomes difficult. When other variables are not controlled for population—the favored value-added-by-manufacturing is virtually never controlled for population although it is an excellent correlate—population size is reintroduced into analysis without being understood as a size effect, again obscuring interpretation.

Some state analyses, while conceding much of the above, argue that important policy differences among the states can be explained with a state-only systems approach and per capita types of measures. States certainly differ on some policies, but it is not immediately clear that these differences are large or important. One straightforward method of examining the magnitude of state policy differences is to compare them with the differences for the entire U.S. population. The population as a whole varies on policies, such as the amount of state and local taxes paid by persons, and this variation can be measured by the conventional variance formula from statistics. The population variance can then be partitioned into two parts: differences among individuals within a state (deviation from the state mean) and differences among states (deviation of the state average from the national mean). The partitioning is done by a standard one-way analysis of variance formula commonly employed in psychology and sociology and available in standard statistical textbooks. The results of the analysis of variance are directly equivalent to a multiple regression using dummy variables (each state is a variable): the variation among states is the variance “explained” by state differences, the variation within states is “unexplained.” The states’ share of the total variance is directly equivalent to the multiple R^2 (proportion of variance explained, measured by “between sum of squares” divided by “total sum of squares”) using fifty independent variables. Table 1 shows the share of the national population variance on typical policies which is accounted for by differences among the states. If anything,

the table overrepresents the state share of the variance, yet states are not the locus of the main national policy differences. While ultimately political science ought to explain nearly all policy, a sense of priorities should lead us to deal first with the 98–100 per cent of the policy variance which is not accounted for by state differences. Similarly, statistical theory and practice indicate that we can have little confidence in our variables that account for only 1 per cent or even 10 per cent of the variance.

Table 1. State Share of National Policy Variance, Selected Policies, One-Way Analysis of Variance

Policy	Proportion of Variance Explained By States, in Percentages
Selective Service Exam Failures, per examinee ^a	2.13% ^b
Car Registrations, per capita	1.47%
State and Local Taxes, per capita	2.34%
State and Local Employees, per capita	0.10%
Divorces Granted, per capita	0.11%
Policemen, per capita	0.02%
Old Age Assistance Recipients, per capita	0.74%
AFDC Recipients, per capita	0.27%
Aid to the Blind Recipients, per capita	0.01%
Disabled Assistance Recipients, per capita	0.12%
General Assistance Recipients, per capita	0.12%
Unemployment Compensation Recipients, per capita	0.11%

^a The data sources for Tables 1, 2 and 3 are *The Statistical Abstract of the U.S.*, prepared by the Bureau of the Census, U.S. Department of Commerce, 91st and 93rd editions (New York: Grosset & Dunlap, 1970 and 1972). The number of cases ranges from 590,000 selective service examinees to over 200 million total U.S. population.

^b Entry indicates that .0213 (1/48) of national policy differences are reflected in state average policy differences.

On none of the policies do states come close to accounting for even 5 per cent of the policy action differences. The differences among states are not large;⁸ if the differences are to be im-

⁸ A number of authors use the coefficient of variation to establish that there are large differences among states. The statistic is not convincing. The coefficient of variation is meaningful only with a normal distribution and a true zero point, in which case the coefficient of variation for the American population of 200 million (where the lowest case is expected to be over 5.6 standard deviations from the mean) should be less than .18 at the maximum, when the lowest case is zero; if the lowest case is more than zero, then the coefficient of variation should decrease. The coefficient of variation for fifty states, however, is expected to have a maximum

⁷ Walker, “Diffusion of Innovations,” p. 384.

portant, then they must have consequences out of proportion to their size. Table 2 indicates state differences as a proportion of national differences, for some measures of policy consequences. The impacts are indeed out of proportion to the policy differences: they are even smaller. The small state policy differences correspond to virtually invisible policy consequences differences.

Table 2. State Share of National Policy Consequences, Selected Variables, One-way ANOVA

Policy Impact	Proportion of Variance Explained, in Per cent Average
Infant Deaths, per live births	0.02% ^a
Rape, per capita	0.00%
Doctors, per capita	0.01%
Hospital Beds, per capita	0.03%
Mental Hospital Patients, per capita	0.03%

^a Entry indicates that .0002 (1/50 of 1%) of national differences are reflected in state average differences.

It is less than clear that the policy impact variable differences can be easily traced to the policy differences. For instance, while policemen and incarceration are supposed to reduce rape, they all increase together, indicating that the "impact" has more influence on the policy than vice versa. The health impacts, while interrelated, are not strongly tied to state policies. Dye finds little or no relation between differences in educational policy action and dropout rates or mental failures.⁹

In comparison to policy variation, we find that social, economic, and representational differences abound in the states, as indicated in Table 3. State differences in these background factors exceed differences on any policy or impact variable. Several caveats are in order, however. On none of the variables is 10 per cent of the national variance explained by state differences, so states are hardly the key to American social organization. Further, Galton's problem reappears because states' scores are not independent of each other.

of .429 (the lowest case is expected to be about 2.33 standard deviations from the mean). We might expect, then, to find fairly high coefficients of variation in the states. The coefficient of variation is useful only as a scale device for comparing standard deviations on variables in which everything is similar except the magnitude of the scores. Any other type of difference (number of cases, modality of cases, absence of a ratio scale, etc.) will make coefficients of variation non-comparable.

⁹ Dye, *Politics, Economics and the Public*, p. 98.

About half or more of the state variance in industrialization, education, urbanization, percentage black, etc., traces to the census bureau's nine geographical regions. Indeed, socioeconomic and political scores allow a near perfect prediction of what region a state is in.¹⁰ Of course, the differences within states—at, say, the county level—are also considerable.

Table 3. State Share of National Social, Economic, and Representational Variance, One-way ANOVA, selected variables

Variable	Proportion of Variance Explained, in Percentages
Urbanization, per capita	7.65%
Black population, per capita	7.56%
Industrial occupations, per employee	4.32%
Income, dollars per family	2.48%
Education, years per person over 25 years	2.46%
Congressional Turnout, per potential voter	4.28%
Congressional Democratic votes, per voter	2.91%

Almost all major state policy studies use measures of association and relationship based on manipulations of Pearsonian product-moment correlations. In view of the great similarity on policy among states, it is unfortunate that correlations do not indicate the sources of similarity. Rather, correlations assume *a priori* that states should be identical, and they attempt to explain violations of the assumptions. Using correlations in a state-level analysis, there is no simple way to check whether similarities are greater than expected. If the major forces involved in state policy are forces which produce similar policies across states, then the standard correlational analysis can seriously mislead our explanations of policy. The national government, functional associations of state officials, and institutionalized policy diffusion patterns among the states are but some of the forces operating to produce similar policies in the states; correlational analysis does not illuminate their policy role. The magnitude of that role can be partly estimated by the difference between the states' share of national social, economic, and political differences (Table 3) and the states' share of policy differences (Table 1). If states were even random groupings, we would expect the variance shares to be about equal; if the

¹⁰ Robert B. Dishman and Robert F. Craig, "Regionalism, State Policy Outputs, and General Explanatory Models," a paper delivered at the Annual Meeting of the American Political Science Association, New York, September 2-6, 1969; tables between pages 27 and 28.

states are primarily political groupings producing different policies, we would expect the policy variance to be considerably greater than the social and economic variance.¹¹ Because the policy variance among states is much less than that which could be expected from the states' social, economic, and representational diversity, the major task of state policy analysis should be to explain the *similarities* among state policies; correlations are not easily applied to this task.

The combination of individual level data (education, income) and aggregate or macro-level analysis (using states) commonly leads to serious level problems.¹² Often, strong forces at the individual level influence our estimates of higher level relationships. With only a macro-analysis, it is extremely difficult to avoid this contamination. The best estimates of the effects of contamination depend on an understanding of the relationship of the aggregate units to the variables.¹³

Though the grouping of individuals into state units is not an intentional act of analysis, the effects of the grouping are the same regardless of why or by whom it is done. The appropriate universe to measure these effects is the set of relationships at the individual level across the country. If there is a state-level set of effects, it will show up in the total individual relationship in a manner first specified by Robinson and since elaborated.¹⁴ The impact of state-level effects depends on the share of the total variance occurring at the state level, relationships at the state level, and "aggregation bias" relationships across levels which depend on the grouping.¹⁵ All three of these may vary considerably with the grouping method. As Blalock has pointed out, "in shifting to larger units we affect the amount of variation in all variables,"¹⁶ and "contiguity is often the major criterion [for grouping]. But such combinations may . . . maximize variations in either the independent

or dependent variables. If so, the slopes and correlation coefficients will be affected correspondingly." Aggregation bias disappears only with exact equations of one sort or another, which are not the rule in empirical political science.¹⁷ Our conclusions about states as policy units depend on the type of grouping which states represent for policy-relevant variables.

Grouping by states minimizes variance on the dependent variable—policy itself. This is clear both from the absolute share of national variance which occurs between states (Table 1) and from the relative share of national policy compared to social, economic, and representational variance (Table 1 vs. Table 3). Policies are the same across states: they differ almost entirely within states.

For an example, we take the main business of both state and local governments: formal education, through attendance at certified schools. Here the federal government has little control, while states explicitly control education in the localities. If states have a significant policy role, it should show up in education. State differences in the proportion of 5- to 34-year-olds enrolled in school in 1960 account for 0.38 per cent of the national variance. In the age group where the national variance is the least (9-year-olds), state differences account for 0.19 per cent of the national variance. These policy differences are far less than the states' differences on some relevant social variables: states account for 1.42 per cent of the national variance in age and nearly 7.65 per cent of the national variance in urbanization. For all practical purposes, the explanation of the state policies is accomplished by a single national explanation, as for instance in Louisiana, where the enrollment rates for eight age-blocs mirror ($r = .994$) the national rates. If the purpose of state analysis is to examine national or universal patterns, then a single within-state analysis is more representative than a between-state analysis; if the purpose is merely to examine state differences, then the study is trivial. For both purposes, the between-state results are likely to be unreliable.

In using states and grouping so as to minimize variance for policy, the relationship of other variables to policy is changed. Particularly, in minimizing policy variance, the use of states as units controls for most of the valid causes of policy differences, while expanding the scope for random and error disturbances. One signal that this is occurring is that as we move from the individual to the state level, we observe a greater reduction of variance in pol-

¹¹ Hubert M. Blalock, Jr., *Causal Inferences in Non-experimental Research* (Chapel Hill: University of North Carolina Press, 1964), pp. 103-114.

¹² The literature on level fallacies is considerable. For a basic discussion, see Hayward R. Alker, Jr., "A Typology of Ecological Fallacies," in *Quantitative Ecological Analysis in the Social Sciences*, ed. Mattei Dogan and Stein Rokkan (Cambridge, Mass.: The MIT Press, 1969), pp. 69-86.

¹³ For summary discussions, see Blalock, *Causal Inferences*, pp. 180-181 and Michael T. Hannan, "Problems of Aggregation," in *Causal Models in the Social Sciences*, ed. Hubert M. Blalock, Jr. (Chicago, Aldine, Atherton, 1971), pp. 491-496.

¹⁴ Hannan, "Problems of Aggregation," pp. 487-489, 496-505.

¹⁵ *Ibid.*, pp. 499-500.

¹⁶ *Causal Inferences*, p. 180.

¹⁷ Hannan, p. 501 and note.

icy than in the variables supposed to produce state policy. If a constellation of variables largely determines policy, a control for policy should control for the portion of the variance in other variables which is reliably related to policy; the remaining variance on these "independent" items should be unrelated to policy and should be small if the variable is a good predictor of policy. For example, urbanization typically implies greater school enrollment for the national sample; in aggregating by age and state, the valid relationship between urbanization and enrollment disappears, leaving only a small and unreliable ($r = .003$) correspondence. Similarly, age—a good negative predictor of enrollment rates (it is mostly young people who go to school), becomes a weak, positive ($r = .118$) correlate of enrollment when ages and states are aggregated. Because the policy variances involved are so small, however, the aggregate relationship may easily appear strong owing to random influences, as for instance in the strong positive relationship ($r = .454$) of age and 9-year-old school attendance at the state level. Explaining this spurious correlation is neither a major nor a rewarding task. "In grouping by Y [policy], we are confounding the effects of all these variables, this being the very opposite of the direction in which we usually wish to move," according to Blalock.¹⁸ Almost no variable strongly related to policy remains a good predictor of policy when grouping by states because most of the valid covariance disappears, and the remaining covariance is confounded with nuisance variables irregularly related to policy. Variables which are but randomly related to policy increase considerably in potential importance, and a constellation of such variables, randomly related to policy but strongly related to each other, may easily show a series of high correlations with policy at the state level without being in any way truly related to policy. In shifting from a universe of 200 million cases to a group of 50 cases, we invite error, an effect magnified many times when the 50 cases are virtually identical on the dependent variable.

The relevance of correlation coefficients in the analysis of policy at the state level is virtually nil: "Correlation coefficients merely measure the proportion of unexplained variation, which is a function of the degree to which outside variables vary as compared with variation in the independent variable,"¹⁹ a comparison not sensible when the independent variable scarcely varies. Some analysts have argued,

however, that half of the slopes of the regression may be reliable for areal groupings such as states if the grouping effect is known.²⁰ Generally, this is true only for simple cases; when the number of variables and relations exceeds three, and when grouping affects all relations and variables somewhat differently—the normal situation for state policy analysis—the regressions are not likely to be reliable; also, if there is a true system factor at work, its effects can be hopelessly confounded with individual level factors.

The Federal Impact on State Politics

The difficulties discussed in the previous section involved in ignoring level phenomena when studying state politics are so considerable as to suggest not only that level factors need be included in the analysis but also that the salient characteristics of state politics derive from the level arrangements. The study of state politics might well be organized around the federal relation.

Consider elections. State majorities are represented in U.S. Senate and gubernatorial outcomes; to the degree that national party rather than state is the important political dimension, this system can seriously misrepresent political opinion in the country. For instance, in the 1968 gubernatorial elections, Democrats with 51 per cent of the national vote won only 38 per cent of the offices; only 54 per cent of the voters were governed by a man for whom they had voted; while 66 per cent of the Republican voters had a Republican governor, only 43 per cent of the Democrats had Democratic governors. If the election priorities were reversed, with parties being represented first and states second,²¹ overall voter satisfaction would decrease less than one-tenth of one per cent, but party satisfaction would change to 48 per cent of the Democrats satisfied and 60 per cent of the Republicans satisfied. But states do not correspond to a major dimension of voter interest—namely, parties; therefore, elections can seriously misrepresent input through malapportionment.

States as policy-making bodies are justified largely by unique local problems or interests, which a theory supporting both majority rule and minority rights might suggest should lead to different state policies based on differing state interests. Frank Munger's study of public

²⁰ *Ibid.*, p. 180.

²¹ Under such a scheme, the party share of offices would be decided by the party share (to the nearest even percentage) of the national vote, with the N Democratic governors going to the N most Democratic states.

¹⁸ *Causal Inferences*, p. 108.

¹⁹ *Ibid.*, p. 180.

policy and public opinion in the states throws light on this justification.²² For a series of issues, Munger measured both the majority public opinion and the law two years later for each state and each issue. These data, when aggregated, yield two additional measures: the national law (the majority of state laws) and the national opinion (majority of state opinions) for each issue. Munger's original measures show state democracy when a state's laws match its citizens' opinions. The additional measures allow us to examine the relations between a state's opinion and the national opinion, between a state's law and the national law, and between national opinion and national law. When states differ from the national pattern on both opinion and law, we might infer that they are working democratically to accommodate unique state interests. Only on three questions—segregation, aid to private schools, and right-to-work laws—do state interests produce state policy in a democratic manner. On these questions, 39 per cent of the states have publics which, in the majority, disagree with majority national opinion, and 37 per cent of the states have laws which disagree with majority of state laws; state opinion is matched by state law 71 per cent of the time, which is 20 per cent more than expected from the marginals; thus, while the translation of opinion into law is considerably less than perfect, it does approximate democracy.²³ These issues tap directly into the major political symbols of national racial, religious, and class division. The federal government prohibited states from segregating by state law, strictly regulated where it has not prohibited state aid to private schools and encouraged state right-to-work splits as a tactical ploy in a national party conflict. The ability of states to legislate democratically on the basis of state interests on fundamental issues depends on the national government.

The federal role in the fundamental issues is critical because these are the only issues on which states divide or approximate democracy. On 53 other questions, state public opinions agree with national opinion more than 90 per cent of the time, state laws agree with each other 87 per cent of the time, state opinion agrees with state law 57 per cent of the time, which exceeds the agreement expected from the marginals by less than 1 per cent.²⁴ On these

issues—divorce, gun control, abortion, police strikes, gambling, etc.—only ¼ of 1 state can characteristically be considered democratic. Slightly more democracy is produced by states following national trends rather than within-state events (state opinion agreeing with national opinion and state law diffusing from other states). Contranational state public opinion corresponds to state law only 37 per cent of the time—well below chance (which is 50 per cent for these dichotomous issues). States are controlled by the federal government on divisive issues and are not operationally democratic on other issues. Democracy in the states would be considerably improved if policy making were nationalized (by passing a uniform law whenever ¾ of the states' statutes agree), so the states are not particularly effective even at diffusing national trends democratically.²⁵ Autonomous states do not protect unique state publics from disagreeable national trends; to the degree states differ from substantial national trends, they tend to frustrate their citizens.

In spite of their critical constitutional position, states are not well received by the public, which pays less attention to them and trusts them less than other levels of government. By a considerable margin, citizens think they get less for their tax dollars from states than from other levels and that for comparable policies states are more unfair.²⁶ In their politics, citizens are nationally and locally oriented. Party identification, for instance, is national. For House of Representative elections in the 'fifties, states characteristically contributed only 1½ to 2 per cent variation in turnout and party vote.²⁷ With any disconnecting of state politics from national politics—through nonpartisan or odd-year elections, for instance—citizen involvement and understanding tend to drop drastically. For the governed, states have a secondary and largely derivative role.

The increasing gap between the nominal constitutional role and the actual role of the states poses some problems for traditional justifica-

²² The uniform national law, while improving opinion-law democracy, removes the possibility of further innovation on that policy. Although a variety of mechanisms (such as time limitations on the uniform law) could remove this disability, the whole suggestion is an academic standard rather than a practical proposal.

²³ "Public Opinion and Taxes," United States Advisory Commission on Inter-governmental Relations, (Washington, May, 1972), Table A-3, p. 15, Table A-1, p. 13, Table A-4, p. 16.

²⁴ Expected deviation, calculated from the state variance components reported in Donald E. Stokes, "Parties and the Nationalization of Electoral Forces," in *The American Party Systems*, ed. William Nisbet and Walter Dean Burnham (London: Oxford University Press, 1967), Table 1, p. 186.

²⁵ Frank Munger, "Opinions, Elections, Parties, and Policies," a paper delivered at the Annual Meeting of the American Political Science Association, New York, September 2-6, 1969.

²⁶ Munger, from Table 1, pp. 7-12. The marginals used here to generate expectations are those for an entire class of questions. Results are similar if marginal expectations are generated separately for each question.

²⁷ *Ibid.*, Table 1.

tion of state politics. The nationalizing of politics includes an individualizing of politics and an increased role for non-areal groups, while diminishing the significance of states. The Democratic party has decreed that individual Democrats, not areal party organizations, comprise the national party and should get represented at national conventions. The increased orientation to policy for voters and parties increases the party role while decreasing areal factors.²⁸ Public opinion differs little among the states, and so do laws. In this situation, the traditional argument for states as representatives of local interests and democratic units carries little weight.

Across time, states have been diminishing in their importance and diversity. Table 4 presents the average standard deviations for states in partisanship in the presidential elections. States have declined as locales for national political controversy.

Table 4. Average Standard Deviations of Presidential Percentages of Popular Vote in States, 1900-1960*

Decade Midpoint	Average Standard Deviation
1900	16.8
1910	15.3
1920	15.2
1930	13.7
1940	12.7
1950	9.1
1960	8.1

* Adapted from Frank Sorauf, *Party Politics in America*, 2nd ed. (Boston: Little, Brown, 1972), p. 52.

Hofferbert and Sharkansky report steadily decreasing socioeconomic and policy variation among states since 1890 and indicate that the connection between these factors has been weak, as with the correlation between per capita income and state and local spending, which decreased from .93 in 1903 to .56 in 1964-5.²⁹ Although the differences diminish, the states

²⁸ For discussions of the changes over time in voters' orientations to parties, see John C. Pierce, "Party Identification and the Changing Role of Ideology in American Politics," *Midwest Journal of Political Science*, 14 (February, 1970) 25-42; David E. RePass, "Issue Salience and Party Choice," *American Political Science Review*, 65 (June, 1971), 389-400; John O. Field and Ronald E. Anderson, "Ideology in the Public's Conceptualization of the 1964 Election," *The Public Opinion Quarterly*, 33 (Fall, 1969), 380-398; and Gerald M. Pomper, "From Confusion to Clarity," *American Political Science Review*, 66 (June, 1972), 415-428.

²⁹ "The Nationalization of State Politics," in *State and Urban Politics*, pp. 469, 472 and "Dimensions of State Policy," p. 322.

largely maintain their respective positions; for instance, the rank-order correlation between Hofferbert's industrialization factor in 1890 and in 1960 is .72.³⁰ Similarly, state policy innovation patterns during the long period of Republican dominance are largely the same as during the current period, though total policy differences among states have been diminishing.³¹ Thus, the nationalization of American society has not particularly implied any major redistribution among states.

The increasing similarity among states over time runs counter to several common models of integration. Changes are not randomly distributed around earlier differences, so the process of change is directional. The similarity does not follow a model of clustering and complementary location which individuals, corporations, etc., left to their own devices tend to follow. Nor do the changes follow the trends of state policy differences, which on the whole would lead to increased dissimilarity over time. The increasing similarity most closely resembles the pattern which would be produced by strong, uniform policies, either in the form of national policies or in the form of similar state policies. The similar state policies could derive from the imposition of national norms or from cooperation and imitation among states.³² The policy similarity cannot derive from similar internal policy-making politics, for these would decrease the resulting similarity. Thus the broad trend is for national patterns to override state differences in policy making and the consequence is increasing similarity among states in the effects of policy.

The formal similarities between state and federal governments are misleading. Despite the nominal commonalities including single chief executive, bicameral legislature, plural-member supreme courts, formal constitutions, Democratic and Republican party cleavages, formal areal representation, civil service, and the obvious similarities in political culture and socioeconomic environment, states do not resemble the federal government in their actual operation. Indeed, for all the formal dissimilarities, local governments are closer to the national government than are state governments. Primarily, states cannot usefully be viewed as political systems (or economic systems or social systems), while the nation can be.³³ Briefly,

³⁰ "Socioeconomic Dimensions in the American States," in *State and Urban Politics*, pp. 456-457.

³¹ Walker, "Diffusion of Innovations," pp. 360-361, and column 1, Table 6, p. 377.

³² Competition among states might also be a source of convergence. The states are unequal competitors, however, so highly similar policies might not result from competition.

³³ For a similar suggestion, see Herbert Jacob and

this means that virtually all parts of state politics are more closely connected to factors outside the state than to other political elements within the state. States are collections, on an areal basis, of assorted subsystems of national and local politics. The preceding presentation of data on social, economic, representational, and policy patterns illustrates the close connection of states to a national pattern. The connections within states are weaker and more variable, partly of necessity—what in Louisiana could be correlated with school enrollment rates more than $r = .994$, which is the connection with national enrollment rates? The correlation of state “industrialization” with state Democratic presidential vote varies from $-.38$ to $.35$ during the period 1890–1960, for instance.³⁴ Mostly, the correlation of items for states follows the national correspondence. State policies are not, therefore, the “outputs” of a state system with state “inputs.” Rather, state policies are mainly the product of national policy trends, between-state communications and imitations, local within-state interests, and some state level political and socioeconomic considerations.

Any adequate model of state policy making cannot build in the predominance of state forces. Rather, the importance of state forces is one of the main characteristics of a state or of a policy. For instance, the highest correlation ($r = .425$) of state democracy for Munger’s data is with states’ absolute deviation from the average innovations pattern. That is, states which are democratic *either* lead in innovating or refuse to follow policy diffusion; in both cases the rejection of the national state policy norm produces more cohesion between policy and opinion within the state. For particular policies, national norms may allow more or less room for state influence. Differences among states’ school enrollment rates for five-, six-, and nine-year-olds account for 12.1 per cent, 7.1 per cent, and 0.2 per cent, respectively, of the national variance, so national norms are less strong as an influence on states’ policies for the younger age groups. For the states with the lowest overall enrollment (Alaska) and the highest (Utah), the 5- to 16-year-old age deviations are strongly correlated ($r = .80, .79$) with the level of national norms; states differ most in the tangential aspects of enrollment policy and differ little when enrollment is critical. Though deviations for the most variable enrollment group—five-year-olds—are related to state

characteristics such as innovation and urbanization ($r = .62, .56$), these relations largely occur as a result of between-state interaction; for instance, 73 per cent of the enrollment-innovation relation results from the six state regional innovation patterns, and 67 per cent matches simple geographical contiguity as indicated by the Census Bureau’s four regions. Most of the remaining relation between urbanization and enrollment validly indicates that, within states, urban areas have a higher enrollment rate for six age groups than do rural areas, a relatively uniform national pattern. The state policies thus depend on a variety of levels of influences, as well as a variety of influences within levels. There might well be a general zero-sum relationship between the impact of national forces and every other influence.

A final salient characteristic of states is the existence of two separate major dimensions in the politics of policy making. Analysts differ on the exact nature of these dimensions. Hofferbert and Sharkansky suggest that they consist of citizen representation (“competition-turn-out”) on the one hand and bureaucratic performance (“professionalism–local reliance”) on the other hand.³⁵ The dimensions involve different demographic factors and operate largely on different policy areas.³⁶ Somewhat equivalently, Walker’s innovation and Munger’s opinion-law democracy are major, separate policy-making dimensions which suggest a bureaucratic and a citizen representation factor. While the analyses are not completely comparable, both suggest a largely “internal” and a largely “external” dimension operating in policy making, each tied to its own sector and reflecting different, rather autonomous, national forces.

Some of the consequences of the dimensions are suggested by a detailed study of four states.³⁷ Munger’s opinion democracy relates directly to the policy accountability of incumbents in elections, and Walker’s innovation scores match the behavioral index for governmental legitimacy (see Table 5). Innovation seems to produce legitimacy (or perhaps legitimacy allows faster diffusion), while electoral accountability for policy seems to produce opinion-law democracy.³⁸ As with innovation and democracy, policy accountability and government le-

³⁴ Sharkansky and Hofferbert, “Dimensions of State Policy,” pp. 331–333.

³⁵ *Ibid.*, pp. 338–339.

³⁶ Douglas Rose, “State Election Systems,” (Ph.D. dissertation, University of Minnesota, 1970), chapters 4 and 5.

³⁷ Accomplished mainly through strong party organization and at the expense of individual demagogic and coalition-building skills. See Rose, “State Election Systems,” Chapter 5.

Michael Lipsky, “Outputs, Structure, and Power,” in *State and Urban Politics*, p. 21.

³⁸ Hofferbert, “Socioeconomic Dimensions in the American States,” Table 6, p. 460.

Table 5. Innovation, Legitimacy, Democracy and Accountability in Four States

State	Innovation ^a	Legitimacy ^c	Democracy ^d	Policy Accountability ^e
Oregon	.544 ^b	1.5	.621	1.1
Wisconsin	.532	1.1	.517	.4
Vermont	.414	.0	.509	-.3
Mississippi	.298	-.4	.621	.7

^a From J. Walker, "Innovation in State Politics," in *Politics in the American States*, ed. Jacob and Vines, 2nd ed. (Boston: Little, Brown, 1971), p. 358.

^b All scores have true zero points; innovation and democracy can reach a maximum of 1.0; legitimacy and accountability absolute scores do not have a precise interpretation outside the state voting context, but a score of 1.0 would indicate that about 1 per cent of a state's eligible voters will turn out and cast their ballots as if voting on this factor alone.

^c From D. Rose, "State Election Systems," (Ph.D. dissertation, University of Minnesota, 1970), pp. 238, 242, 247, 251.

^d From F. Munger, "Opinions, Elections, Parties, and Policies," delivered at the 65th (1969) Annual Meeting of the American Political Science Association, p. 18.

gitimacy are linearly unrelated, but increases in policy accountability produce deviant legitimacy scores. That is, accountability and democracy lead away from the normal pattern of mild innovation and mild legitimacy.

The striking characteristics of state politics appear to derive from level effects. The level effects may produce interaction within states, leading to some broad differences in the types of politics occurring in different states.

The basic elements of state politics from constitutions to family structure to traffic laws are national, both in the sense of being fairly uniform across states and in the sense of national change occurring. These national changes do not result from similar intrastate processes but rather occur with similar interstate and state-nation processes. The basic pattern for state politics is for each element to be largely unrelated to any other element except through their relations with national patterns, which themselves may be causally connected. States are not political systems, though they are parts of a political system.

Within this basic structure, state departures from national patterns reflect past departures minus the relatively uniform impact of national policy. The total amount of differences across states on any element mostly indicates the impact of national policy on that element: policy itself shows small differences, while largely unregulated elements, such as urbanization or religious preference, show noticeable differences.

For most states, political activity is regular but not strenuous. The electorate selects potential national officials, holds taxes down somewhat and supports education; bureaucrats suggest and implement standard laws; legislators adopt the standard laws and cut budget re-

quests. The connection between one activity and another is tangential or small.

As long as the national pattern is functional and the state differences from national patterns on any element are not important, or not related to differences on other elements, then states can function as rote conduits of national patterns. But if the national pattern becomes dysfunctional, then states need to become systems. The 1929 crash and the contemporary school busing issue are perhaps instances. There is little evidence that states do become systems in these instances, and there are compelling reasons why they cannot do so. States do not control their borders, or the large part of their commerce which is interstate, or their basic political arrangements, or the host of other factors which they would have to control to be political systems. It is unlikely that even if they could control these, states could institute major change in the required short period of time. The national government, of course, is unlikely to allow states to control such matters, for to do so would eradicate the national system (whose priority was more or less established by the Civil War).

State differences from the national pattern on any element are likely to be important only when the differences are large and the item is fundamental. Aid to parochial schools in Catholic states, the closed shop in industrial states, segregated schools in Southern states are illustrations of such differences, as are the severance taxes in states heavily dependent economically on mineral extraction. Because states are not normally systems, it takes an enormous amount of unified political power to get them to operate as such. Mass coercion from a relatively homogeneous electorate is likely to be a

required ingredient in the application of power to produce deviant policy. Aside from the considerable difficulties involved within the state in producing deviant policy, two additional constraints operate. First, the national government may not allow the policy. Second, the basis on which the power and policy rest will, in the normal course of events, erode over time as national policy on other matters decreases dissimilarities among states. It is quite possible to have a mildly coherent state mobilization system which cannot govern in accord with the politics, or a deviant policy whose political justification has eroded, or both occurring in separate policy areas.

Interrelated state differences from the nation may produce consistent differences across a range of policies. While it is possible for a state to operate systematically to produce the consistency, this would require that the differences be of major importance. What is more likely is that marginal consistency is built in at some stage of policy making. Regional policy diffusion appears to occur because administrators take account of patterned differences; the matching of law and public opinion to produce consistent deviation tends to occur as legislators perceive consistent interrelated policy preferences (ideology) in the electorate.³⁹ Such consistency amounts to being above or below, faster or slower than the national pattern; it is limited by the pattern itself and by the lack of completely interrelated state differences from the nation. Because the national pattern is a moving pattern which carries states ever closer to itself, even policy deviancies based on built-in accommodation of interrelated differences are likely to be unevenly and generally decreasingly deviant.

Most states are not systems but reflectors of national patterns. Some states which regularly precede or lag behind the national pattern on many elements may behave as if they were systems in part, but the operating consistency resolves to mechanisms in the bureaucracy or representation process for adjusting the national pattern. Some states with important differences from the nation may have a systematic politics or basically deviant policies, but the system arises in response to the national pattern, and the existence of deviant policies depends on the national government. In all these cases, whether or not the state is or appears to be consistent, the national pattern is the dominant influence. The national influence operates most massively in a direct causal fashion, but

even differences among the states' patterns of politics arise in reaction to state-national differences. In understanding the politics of any state, the most important information to know is the national pattern.

States whose electorates' preferences differ either consistently or importantly from the national pattern are more likely, as in Mississippi and Oregon, to exercise policy control over incumbents through elections, producing a closer-than-average (though hardly perfect) correspondence between public opinion and state law—democracy. Such states are better able to withstand national electoral trends—the pro-Democratic 1958 and 1964 forces, for instance—than are states such as Vermont and Wisconsin.⁴⁰

The existence of a relatively coherent state political system like those in Oregon and Mississippi, based on differences from the nation, tends to produce deviant policy, reflected in Oregon's high innovation score and Mississippi's low score, as legislators and administrators either build in the preference differences or build in factors which are related to preference differences.⁴¹ Because the systems are not complete and because national patterns continually change, neither state is thoroughly deviant. It seems likely, however, that all states are more deviant on policy than could be expected simply from the strength of the representational arrangements; this suggestion is consonant with the notion of relatively autonomous forces in administration which operate as though there were a coherent democracy.

State differences from national policy patterns have quite divergent consequences depending on their direction (i.e., whether they are ahead of or behind the national pattern). Because the national pattern over time tends to be unidirectional, the continuance of that trend requires the national government to allow states to innovate in the direction of the anticipated pattern, and this requirement is usually met. The national government allows innovative de-

³⁹ Rose, "State Elections Systems," p. 215, pp. 158-159.

⁴¹ Wisconsin's innovation and progressive policies on welfare-education appear rooted in earlier state-national differences associated with the Progressive movement and continued through the state bureaucracy which is one of the major monuments of progressivism. The bureaucracy itself can continue as "too" innovative for the state because, as discussed in the text, it is highly functional for a state to be innovative. The origins of the Wisconsin progressive deviancy can speculatively be traced to the deviant "administrative" culture of the large German population, the availability of the progressive movement, the alienation from the Democratic party in World War I, and the feudal electoral politics associated with the La Follettes.

³⁹ Munger, "Opinions, Elections, Parties, and Policies," pp. 30-31.

viancies (such as segregation in the 1890s, primary elections, divorce leniency, permissive abortion, etc.) when they are first initiated and if they represent a trend which fits with the rest of national policy. State policy deviancy which lags behind the national pattern, however, eventually gets revoked or undercut by national policy. Thus the long-term policy consequences of state deviancy depend on the direction of deviancy with regard to the national pattern. States in advance of the nation are supported by national policy, and even where the policy is in advance of state citizen preferences, the effects of national patterns will eventually make the policy appropriate for the state. States which lag behind the nation get undercut by national policy, either directly or through the overtime workings of national policy impacts. It is this asymmetry which appears to produce the close relation between policy innovation and the legitimacy of state governments to their citizens.⁴² Over time, either directly or indirectly, laggard states will have a high proportion of ineffective or inappropriate policies—a

⁴² One possibility is that the legitimacy that the national government has for a state's citizens may correspond (partially) to the legitimacy of the state government, for the same conflictual process affects both of them.

tendency which is likely to lower state legitimacy. Similarly, innovative states receive a legitimacy bonus over time.

The above is a hypothesis about why most states are mildly democratic and legitimate and others are moderately democratic and illegitimate; the democracy and legitimacy are related in all states, but the relation in addition to the scores depends on the state-national contrast for a variety of elements. It should be re-emphasized that the differences among states are marginal—that is, no state is very systematic, democratic, or innovative. The forces of nationalization are so strong that they dwarf as well as create differences among states.

Summary

Studies of state politics which ignore events and relations at other levels are severely limited in their usefulness for both methodological and substantive reasons. The salient elements of state politics do not derive from relations within states, so it is necessary to examine external relations of states, especially relations to national patterns, to understand state politics. The basic dynamics of state politics may be rooted in the impact of national patterns on states and the reactions of states to those patterns.

Innovation in the States: A Diffusion Study*

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In the past decade political scientists have witnessed an outpouring of literature on state policy outputs. Although policy is generally acknowledged to be a multidimensional phenomenon, comparative state policy research, with a few exceptions, has had expenditures as its primary focus. One of the few nonmonetary dimensions investigated previously was innovation by states.¹ This article seeks to extend in a more rigorous fashion the investigation of innovation by states.

Introduction

An innovation is generally defined as an idea perceived as new by an individual; the perception takes place after invention of the idea and prior to the decision to adopt or reject the new idea.² In this study, as in Walker's, an innovation is more specifically defined as a law which is new to the state adopting it, i.e., it is equivalent to a single adoption.

The observable data are the adoptions by states of particular laws. When states first learned of the idea is unknown. The data do not encompass new ideas or practices adopted by the state's bureaucracy, nor do they include "disadoptions" by means of a state court's declaring a law unconstitutional. In practice, innovation may stimulate huge appropriations or it may have little monetary impact if the program is adopted but never funded. Consequently, there is good reason to study the process by which states adopt new ideas as well as the process by which they maintain existing programs, i.e., expenditures.

The laws under consideration here are from issue areas central to the "have have-not" struggle, described as the essence of politics by V. O. Key, Jr.³ It is more likely that a political explanation, not an economic one, can account for differences in selected "have-not"-oriented policy areas than it can for the broad range of

policy areas included in some studies.

The policy areas selected are education, welfare, and civil rights. Education was singled out by Key as an arena of "have have-not" conflict;⁴ welfare was the focus of Cnudde and McCrone's study specifically because of its centrality to the "have have-not" struggle;⁵ the adoption of civil rights was the focus of a study by Lockard, one of Key's former students.⁶ Within the areas selected there is some potentially interesting variation: Education is probably less "have-not"-oriented than the other two fields; civil rights innovations ordinarily would require less funding than public welfare or education programs.

Insofar as possible, the laws selected were ones whose adoption was free of federal influence because in expenditure analyses it has been found that federal spending often has a substantial impact on state and state-local spending patterns.⁷ Some of the laws considered here were enacted by states prior to the federal government's entry into the field, e.g., public welfare laws before 1935. Others deal with subjects exclusively in the state's jurisdiction, e.g., teacher certification. This distinction between federal stimulation and state initiative is not made in Walker's exploratory work. Walker treats the dates on which states began to participate in federal grant-in-aid programs the same as the dates states adopted laws independently. Hence, the results of the two efforts may vary somewhat.

The laws also were selected for the durability of the issue; it took a long time for all states to adopt any one law. Indeed, the data extend back as far as the 1780s in one case. In some instances the process of adoption by all states is not yet complete. All laws in the three policy areas which meet these criteria and whose dates of adoption were summarized in available sources were selected. Table 1 lists the laws, the time periods during which they were adopted,

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¹ Jack L. Walker, "The Diffusion of Innovations Among the American States," *American Political Science Review*, 63 (September, 1969), 880-899.

² Everett M. Rogers, *Diffusion of Innovations* (New York: Free Press of Glencoe, 1962), p. 13.

³ V. O. Key, Jr., *Southern Politics in State and Nation* (New York: Alfred A. Knopf, 1949), p. 307.

⁴ Key, p. 307.

⁵ Charles F. Cnudde and Donald J. McCrone, "Party Competition and Welfare Policies in the American States," *American Political Science Review*, 63 (September, 1969), 858-866.

⁶ Duane Lockard, *Toward Equal Opportunity: A Study of State and Local Anti-Discrimination Laws* (New York: Macmillan, 1968).

⁷ Richard E. Dawson and Virginia Gray, "State Welfare Policies," in *Politics in the American States*, 2nd ed.; ed. Herbert Jacob and Kenneth N. Vines (Boston: Little, Brown and Co., 1971), p. 459.

and the number of states adopting them.

Using this data base, we will consider three questions: (1) How do new ideas diffuse or spread among the states? They may diffuse like many other new ideas, through user interaction. If this diffusion process is regular, i.e., predictable, a dynamic model of the process can be constructed. Comparison can be made between policy areas to ascertain how diffusion differs according to the issue involved. (2) Why are some states more innovative than others? The common hypotheses relating competition and economic resources with public policy will be tested and the results compared by issue area. (3) Are there identifiable patterns of innovation? An effort will be made to determine if the same states are innovative in all three policy areas.

Diffusion of Innovations

The process by which an innovation spreads is called diffusion; it consists of the communication of a new idea in a social system over time.⁸ Diffusion research has been carried out in the disciplines of: anthropology (e.g., the diffusion of cultural traits among primitive tribes); rural sociology (e.g., the diffusion of hybrid seed corn among farmers);⁹ medical sociology (e.g., drug adoptions by physicians);¹⁰ education (e.g., some 150 studies made at Columbia University under the direction of Paul Mort); industry (e.g., the diffusion of a new product among consumers); political science (e.g., the diffusion of city manager governments in the 48 states);¹¹ and medicine (e.g., the contagion of a disease).¹²

Empirical investigation in rural sociology, sociology, and education has demonstrated that for a wide variety of innovations, the frequency of their adoption over time is normally distributed; their cumulative distribution over time has the "S"-shape of the cumulative normal curve.¹³ Three explanations are offered in the literature for this repeated finding:

(1) The time of adoption for any given case "is determined by the interplay of an infinitely

⁸ Rogers, *Diffusion of Innovations*, p. 13.

⁹ Bryce Ryan and Neal C. Gross, "The Diffusion of Hybrid Seed Corn in Two Iowa Communities," *Rural Sociology*, 13 (March, 1943), 15-24.

¹⁰ James S. Coleman, Elihu Katz, and Herbert Menzel, *Medical Innovation: A Diffusion Study* (Indianapolis: Bobbs-Merrill Co., 1966).

¹¹ Edgar C. McVoy, "Patterns of Diffusion in the United States," *American Sociological Review*, 5 (April, 1940), 219-227.

¹² Norman T. J. Bailey, *The Mathematical Theory of Epidemics* (New York: Hafner, 1957). For sources of 1500 other diffusion studies see: Everett M. Rogers and F. Floyd Shoemaker, *Communication of Innovations: A Cross Cultural Approach*, 2nd ed. (New York: Free Press of Glencoe, 1971), pp. 388-466.

¹³ Rogers, *Diffusion of Innovations*, chap. 2, *passim*.

Table 1. Innovations in Three Policy Areas, Duration of Adoption Process, and Number of Adopters

Innovation	Time Period	No. of Adopters
<i>Education</i>		
State Boards of Education	1784-1949	40
Chief State School Officer	1835-1912	37
Compulsory School Attendance	1852-1918	48
Degree Requirement for Teaching in Elementary School	1930-1969	46
Degree Requirement for Teaching in High School	1896-1966	44
<i>Welfare</i>		
Merit System for State Welfare Dept.	1883-1942	48
Old Age Assistance	1923-1938	48
Aid to the Blind	1898-1945	48
Aid to Families with Dependent Children	1911-1937	48
<i>Civil Rights</i>		
Anti-Discrimination in Public Accommodations	1947-1966	31
Fair Housing (Public or Private)	1937-1965	19
Fair Employment	1945-1966	28

Sources: Fair Employment, Housing, Public Accommodations: Duane Lockard, *Toward Equal Opportunity: A Study of State and Local Antidiscrimination Laws* (New York: Macmillan Co., 1968), p. 24; U.S., Civil Rights Commission, *Voting*, Book 1 (Washington: Government Printing Office, 1961), pp. 208-210; U.S., Housing and Home Finance Agency, *Fair Housing Laws* (Washington: Government Printing Office, 1964), p. 10.

Merit System: Council of State Governments, *Book of the States, 1952-53*, 9 (Chicago: Council of State Governments, 1952), 179.

OAA, AB, AFDC: Anne E. Geddes, *Trends in Relief Expenditures, 1910-1935*, U.S., Works Progress Administration, Division of Social Research, Research Monograph 10 (Washington: Government Printing Office, 1937), pp. 91-92.

Degree for Elementary and High School: T. M. Stinnett, *A Manual on Certification Requirements for School Personnel in the United States* (1967 ed.; Washington: National Education Association, 1967), p. 80.

Compulsory School Attendance: August W. Steinhilber and Carol J. Sokolowski, *State Law on Compulsory Attendance*, U.S., Department of Health, Education, and Welfare, Office of Education, Circular #793 (Washington: Government Printing Office, 1966), p. 3.

Chief State School Officers and State Boards of Education: U.S., Federal Security Agency, Office of Education, *State Boards of Education and Chief State School Officers*, Bulletin #12 (Washington: Government Printing Office, 1950), p. 27 and p. 8.

large number of elements in the social milieu," thereby fitting the requirement of the normal distribution that "the value of each event is the

result of the chance combination of a great many minute and relatively equal factors."¹⁴

(2) The cumulative normal curve is similar to an individual's learning curve which is "S"-shape in its cumulative form. Adoption by a state is then equivalent to a learning trial by an individual.¹⁵

(3) There is an interaction effect, i.e., adopters influence those in the social system who have not yet adopted. As more persons adopt, the effect on nonadopters increases.¹⁶ Actually, the normality or non-normality of the adopter distributions is independent of the theoretical assumption that ideas spread because adopters somehow influence nonadopters.¹⁷ Other curves, particularly the logistic curve of population growth, have been widely used to fit the same kind of data (innovations) with the same goodness of fit.¹⁸ The interaction explanation is more appealing on substantive grounds; observers of state governments point out that decisionmakers emulate or take cues from legislation passed by other states. Indeed, this function is institutionalized in the Council of State Governments, financed largely by states. Walker argues that this competition of ideas largely determines "the pace and direction of social and political change in the American states."¹⁹ Thus, in formal diffusion theory and in Walker's application of it to state governments, one assumption is that gain in adoptions is due to nonadopters' emulation of adopters. In the following section a simple interaction diffusion model is developed and then evaluated using the twelve laws being studied here.

A Diffusion Model Based on Interaction. The rate of spread of adoptions can be denoted by ΔA_t and is some function of those already adopting:

$$(1) \Delta A_t = f(A_t),$$

where A_t is the cumulative proportion of adopters in the t th year and ΔA_t is the difference in the cumulative proportion of adopters at t and $t + 1$, defined as $\Delta A_t = A_{t+1} - A_t$.²⁰

¹⁴ H. Earl Pemberton, "The Curve of Culture Diffusion Rate," *American Sociological Review*, 1 (August, 1936), 550, 549.

¹⁵ Rogers, p. 153.

¹⁶ Rogers, p. 154-155.

¹⁷ Rogers, p. 154-155.

¹⁸ Coleman, Katz, and Menzel, pp. 100-103; however, for a warning on the futility of curve fitting as a satisfactory test of theoretical relevance see: William Feller, *An Introduction to Probability Theory and Its Applications*, II (New York: John Wiley & Sons, 1966), 52.

¹⁹ Walker, "Diffusion of Innovations among American States," p. 890.

²⁰ For explanation of the application of difference equations to social data see: Samuel Goldberg, *Introduc-*

The most elementary way to consider the function f is as the number of pair relations between adopters and nonadopters or simply their product. This interpretation assumes that the population is completely intermixed, i.e., that "leaders" from each adopter state come in contact with "leaders" from each nonadopting state. Furthermore, the model omits diffusion from other sources, e.g., innovation stimulated by the minimum standards attached to federal grants-in-aid.²¹

If the proportion of adoptions is proportional to the interaction between those who have adopted and the potential adopters, then f could be expressed as:

$$(2) f = bA_t(L - A_t),$$

where b is the coefficient of diffusion from interaction,

L is the maximum possible proportion of adopters for a particular law, and

$L - A_t$ is the pool of potential adopters in year t .

If $L = 1$, all states have the potential for adoption. There are compelling reasons, however, why every state will not be susceptible to adopting a particular law: Hard-to-amend limitations in the state's constitution or values of the political subculture might cause a state's leaders to be practically immune to diffusion from interaction. Therefore, L , the limit on the pool of eligible adopters, should be treated as a parameter to be estimated for each law.

By substitution,

(3) $\Delta A_t = bA_t(L - A_t)$ and, after rearranging and decrementing the index t ,

$$(4) A_t = A_{t-1} + bA_{t-1}(L - A_{t-1}).$$

The cumulative proportion of states having adopted any law at year t depends upon the proportion of states retaining the law plus some proportion b of the interaction between previous adopters and eligible adopters. The coefficient of the first term, A_{t-1} , is set equal to 1, implying that all states which adopt a law keep it. Although this assumption is not strictly true in all cases (e.g., states may retain a law but

tion to Difference Equations (New York: John Wiley & Sons, 1958).

²¹ More elaborate models could be constructed in which there is diffusion from a constant source like the federal government, or in which there is incomplete mixing of the population, e.g., regional or professional communication networks may produce distinctive diffusion patterns. For these more elaborate models see: James S. Coleman, *Introduction to Mathematical Sociology* (New York: Free Press of Glencoe, 1964), chap. 17.

not enforce it or states may revoke laws, as Mississippi, South Carolina, and Virginia have done for compulsory school attendance), the data sources are not systematically informative on this point. Hence, this model does not allow the process to go "backward."

By rearrangement, the equation becomes

$$(5) A_t = 1A_{t-1} + bLA_{t-1} - bA_{t-1}^2,$$

whose parameters can be estimated by the following regression equation:

$$(6) A_t = (1 + bL)A_{t-1} - bA_{t-1}^2 + c + e$$

where c is the intercept and

e is the error term.

If the intercept c is near zero, and if the error, e , meets standard expectations, then regression may furnish useful estimates of b , the contribution from interaction, and L , the maximum proportion of states susceptible to adoption of any one law.²²

The interaction model was evaluated using as observations the cumulative proportion of states having adopted a particular law. Figure 1 below displays an example of the fit of the curve predicted by the model for adoption of the twelve laws. The squared term of the equation causes the regression line to depart from linearity and allows the "S"-shape characteristic of some cumulative adopter distributions.

The fit of the regression line is very good for all twelve laws, as Table 2 shows. Relying upon

²² J. Johnston, *Econometric Methods* (New York: McGraw-Hill Book Co., 1963), p. 7.

R^2 as the goodness-of-fit criterion, one can observe in column 1 that the variance explained ranges from .9380 for AFDC to .9976 for high-school degree requirement. Referring to the graphs, one will note that when adoptions are plotted over time, the high-school degree requirement data (Figure 2) approach a normal curve, but AFDC (Figure 3) presents quite a different picture—a damped oscillatory pattern.

It was stated previously that the error must be small and the regression constant c must be near zero in order for true estimates of the parameters to be given. As Column 2 of Table 2 shows, the intercept is very close to zero except in one case. That case is AFDC, whose intercept is .1320; that figure is sensible when you examine its graph (in Figure 3). The third column of Table 2 reports the error of the dependent variable relative to its standard deviation. The standard errors are rather small; again AFDC is the most aberrant. Therefore, by these criteria the simple model fits fairly well for these twelve laws.

Another basis on which one can evaluate the fruitfulness of the model is to examine the parameter estimates. Columns 4 and 5 of Table 2 allow for comparison of the estimated value for the parameter L and the last observed value. For eight of the twelve cases, the estimated value is close to the observed value; in two cases, the observed value is overestimated; in two cases, underestimation occurs. For each of the latter four cases there are technical reasons to anticipate poor predictions of L : low R^2 's for

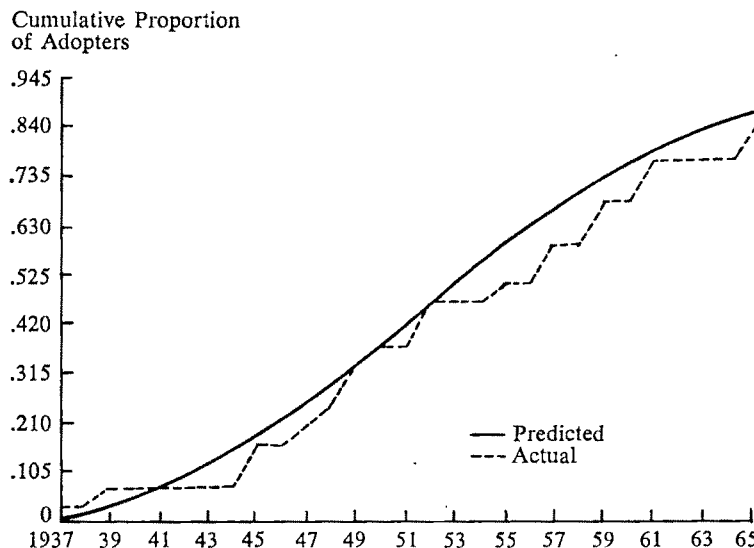


Figure 1. Predicted and actual curves for adoption of fair housing legislation, 1937-1965

Table 2. Evaluation of Interaction Diffusion Model, $A_t = A_{t-1} + bA_{t-1}(L - A_{t-1})$, for 12 Innovations

Laws	(1) R^2	(2) Intercept	(3) Standard Error	(4) Estimated L^*	(5) Observed L^*	(6) b
<i>Civil Rights</i>						
Public Accommodations	.9756	.0250	.0337	.590	.651	.15
Fair Housing	.9841	.0068	.0172	.391	.399	.32
Fair Employment	.9615	.0278	.0341	.468	.588	.06
<i>Welfare</i>						
Merit System	.9453	-.0227	.0483	1.486	1.000	.22
Old Age Assistance	.9772	.0039	.0570	1.079	1.000	.38
Aid to the Blind	.9931	-.0027	.0290	1.145	1.000	.14
AFDC	.9380	.1320	.0690	.769	1.000	.08
<i>Education</i>						
Boards of Education	.9951	.0019	.0229	.791	.840	.07
Chief School Officer	.9962	.0032	.0213	.731	.777	.14
Compulsory Attendance	.9960	.0075	.0216	1.019	1.000	.05
Degree, Elementary	.9931	.0130	.0268	1.014	.966	.07
Degree, High School	.9976	.0013	.0175	.895	.924	.15

* L = limit on the pool of adopters.

merit system, fair employment, and AFDC; negative intercepts for merit system and AB; a large intercept for AFDC; large standard errors for merit system and AFDC. More error seems to occur when attempting to predict the processes which have diffused completely. A more substantive interpretation is that error in prediction occurs for welfare laws; an explanation

for this phenomenon will be offered in the section comparing issue areas.

The estimates of the parameter b in column 6 of Table 2 are also of interest; b can be interpreted as the probability that when "leaders" from two states meet, their interaction results in another adoption. Unfortunately, there is no criterion by which to evaluate the accuracy of

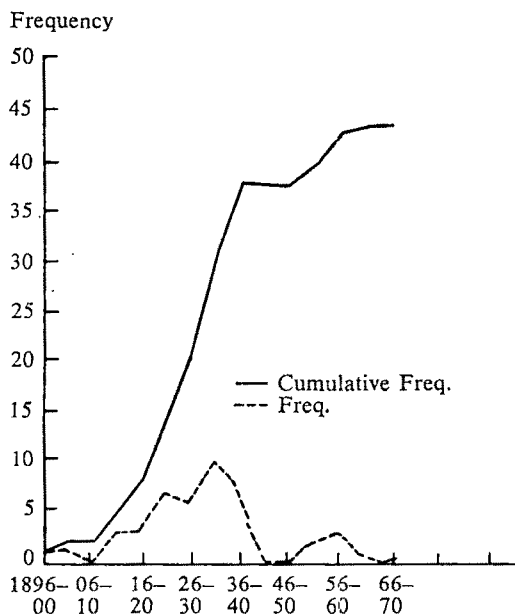


Figure 2. Adoption of degree requirement for teaching in high school by state, 1896-1966

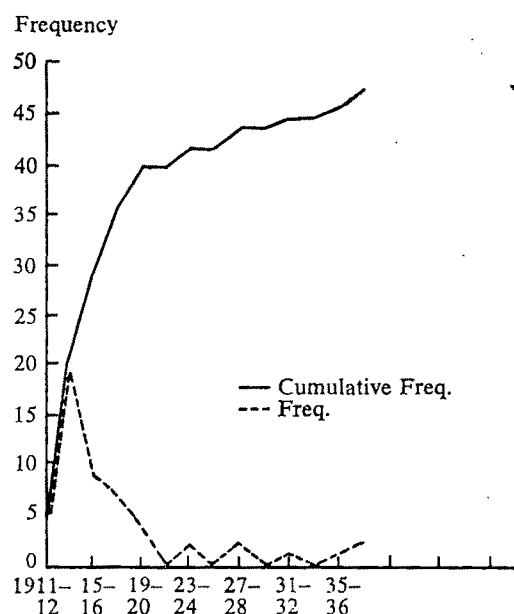


Figure 3. Adoption of aid to families with dependent children legislation by state, 1911-1937

these probabilities.²³ Interaction has the greatest impact in the diffusion of old-age assistance legislation. The fact that most of the b 's are small is understandable, since for the graphs of most of these twelve laws a straight line with a "tail" at each end would fit. Therefore, a linear model was also evaluated, using the regression equation,

$$(7) A_t = bA_{t-1} + c + e,$$

where the symbols are those defined earlier.

The R^2 's of the quadratic model can be compared to the R 's of the linear model, using the F -statistic as the test for significant difference.²⁴ As shown in Table 3, for 6 of the 12 models there is a significant increase in the proportion of variance explained when the interaction term is added. Consequently, in half of the cases studied, innovations seem to diffuse through interaction.

Table 3. Comparison of Quadratic and Linear Models for Twelve Innovations

Laws	Quadratic R^2	Linear R^2	F
<i>Civil Rights</i>			
Public Accommodations	.9756	.9750	.45
Fair Housing	.9841	.9831	1.56
Fair Employment	.9615	.9614	.04
<i>Welfare</i>			
Merit System	.9453	.9415	3.89*
Old Age Assistance	.9772	.9695	4.37*
Aid to the Blind	.9931	.9918	8.66*
AFDC	.9380	.9376	.18
<i>Education</i>			
Boards of Education	.9951	.9950	4.66*
Chief School Officer	.9962	.9958	12.33*
Compulsory Attendance	.9960	.9953	2.82
Degree, Elementary	.9931	.9923	1.63
Degree, High School	.9976	.9967	22.00*

* Significant at .05 level.

This simple interaction model holds up fairly well under evaluation on the basis of the crite-

²³ In general, the accuracy of b may depend upon the degree to which a social structure is completely intermixed. In this case, bias may be introduced by structural characteristics, such as regionalism, which reduce the number of relations across regional boundaries and increase the state contacts within regional communication networks. For an excellent discussion of communication networks at the state level see: Walker, "The Diffusion of Innovations Among the States," pp. 891-897.

²⁴ John Cohen, "Multiple Regression as a General Data-Analytic System," *Psychological Bulletin*, 70 (1968), 435.

ria used above. The results generally confirm that some of these innovations diffuse as do others—through the interaction of users and nonusers. The next section compares the results by issue area. Certainly, one might want to refine the model, perhaps by adding a term for constant source diffusion and by relaxing the assumption of a completely intermixed population.²⁵

Diffusion Patterns by Issue Area. Results from evaluating the diffusion interaction model can be put to a further use: comparing the three policy areas—education, welfare, and civil rights—with respect to diffusion from interaction. Several analyses of state expenditures, broken down by functional area, report that states vary in their level of support for each function, sometimes in response to the varied nature of the stimulus from federal grants-in-aid.²⁶

Table 2 contained several points at which the issue areas differ. The average R^2 for civil rights is .9637; for welfare, .9634; for education, .9956. Innovations in education appear to occur with more regularity, possibly because the process took place over a longer period than in the other issue areas. Furthermore, within the area of education, there is little variation in R^2 's among the various laws, while in the other two areas, there is marked variation in the amount of variance explained. Thus, one

²⁵ It should also be noted that several sources of error are possible in this model. First of all, the data are recorded as proportions based on 48 states; obviously, there were less than 48 states in existence for many years studied. Nevertheless, the model fits best for the longer processes (i.e., education) in which more states are absent; hence, this possible source of error did not have deleterious effect on the goodness of fit.

Another kind of error could arise from using a lagged dependent variable on the right-hand side of the regression equation; the assumption that the stochastic or disturbance term is normally and independently distributed may be violated in such a situation. A common test statistic for the presence of serial correlation (i.e., the disturbance at t is highly correlated with the disturbance at $t-1$) is the Durbin-Watson d . When proper adjustments are made for using a lagged dependent variable, the d -test indicates no serial correlation. (See Carl F. Christ, *Econometric Models and Methods* [New York: John Wiley & Sons, 1966], p. 522). Another test for normality is to collect the residuals into a frequency distribution and graph them. (See Christ, pp. 526-530). If the residuals are approximately normal, the graph will be bell-shaped. When this test was applied to three of the laws, the residuals did not appear by inspection to be normally distributed. Thus, the results of the two tests are conflicting concerning the error terms.

²⁶ See Virginia Hickman Gray, "Theories of Party Leader Strategy and Public Policies in the American States." (Doctoral dissertation, Washington University, 1972).

could say that education innovations diffuse in a regular and similar manner, for whatever reasons, whereas civil rights and welfare adoptions do not follow a single diffusion path unique to the subject matter of the law.

It is interesting to note from Table 3 that the interaction term makes a difference for 3 of 5 education laws, 3 of 4 welfare laws, and no civil rights laws. This tendency fits in with the other findings, all generally pointing to the conclusion that the normal diffusion of civil rights laws is met by resistance from particularly immune states.

Although the simple diffusion model does not allow for it, one might also differentiate patterns of adoption according to the amount of diffusion from a constant source relative to diffusion from interaction. For instance, one might hypothesize that program adoptions tied to federal grants-in-aid will diverge from the pattern of normality exhibited by programs adopted independently by states. For this reason, wherever possible, laws were chosen for this study from areas relatively untouched by the federal government. In the welfare area, programs were selected which began independently, though the last few observations (those after 1935) are of programs falling under federal aegis.

Walker in his landmark article does not anticipate much difference:

In a later work I will report the results of comparisons of the diffusion patterns of issues from different subject matter areas. Preliminary efforts at such comparisons, however, have not revealed significant variations. There does not seem to be much difference in the diffusion patterns of issues of different types.²⁷

Walker makes up his composite innovation score from 88 programs which represent a mixture of federal grant-in-aid and independent state programs. He includes five state programs which are used in this study: compulsory school attendance, fair housing, teacher certification at both levels, and superintendence of public instruction. Their graphs are displayed in Figures 4-7 and in Figure 2. In the welfare area he uses program adoptions under the Social Security Act. The course of their adoption over time is displayed in Figure 8. There is a large initial enactment, and then adoptions drop off rapidly. Their diffusion pattern is radically different from that in any graph previ-

²⁷ Walker, "Diffusion of Innovations Among the States," pp. 882, n. 9, or Jack L. Walker, "Innovation in State Politics," in *Politics in the American States*, 2nd ed.; ed. Herbert Jacob and Kenneth N. Vines (Boston: Little, Brown and Co., 1971), p. 591, n. 9.

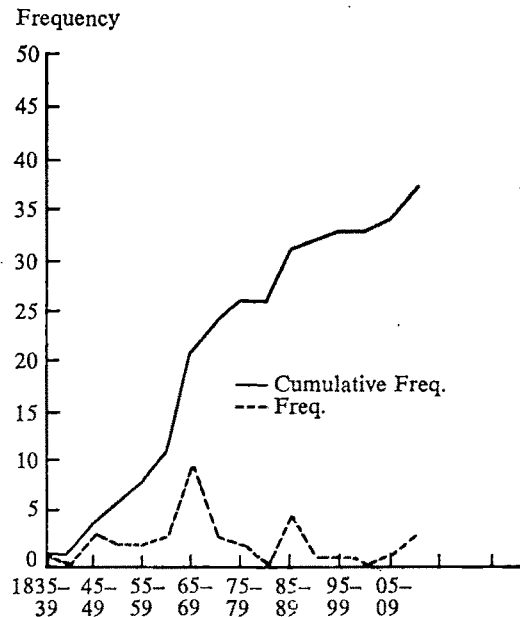


Figure 4. Adoption of chief state school officer legislation by state, 1835-1912

ously shown in this study. It seems, therefore, that diffusion patterns vary, even in Walker's own data, owing, apparently, to federal intervention.

The hypothesis that federal involvement is a source of variation in adoption patterns is also

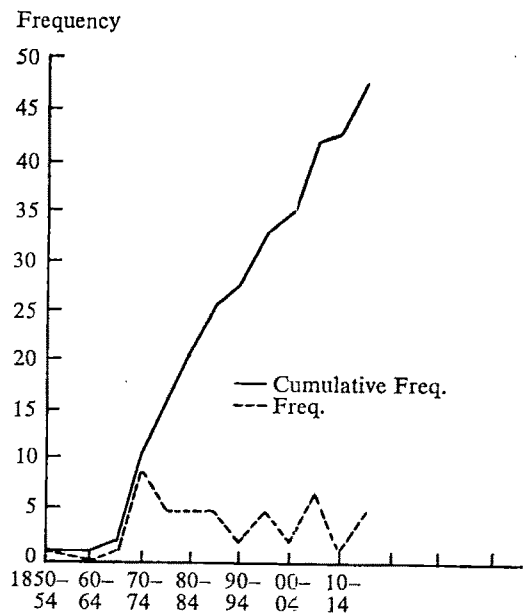


Figure 5. Adoption of compulsory school attendance legislation by state, 1850-1918

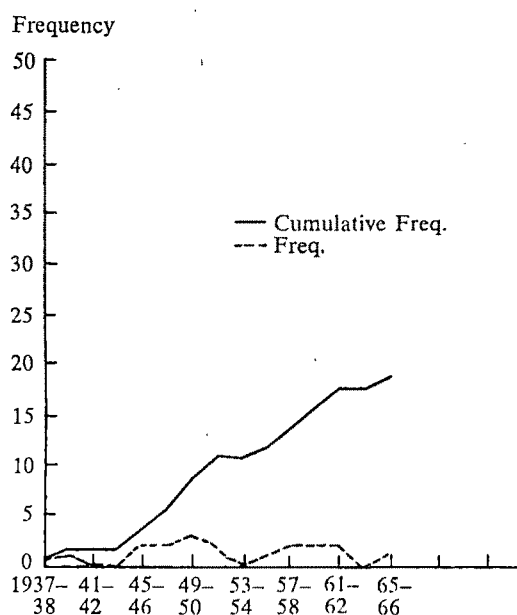


Figure 6. Adoption of fair housing legislation by state, 1937-1965

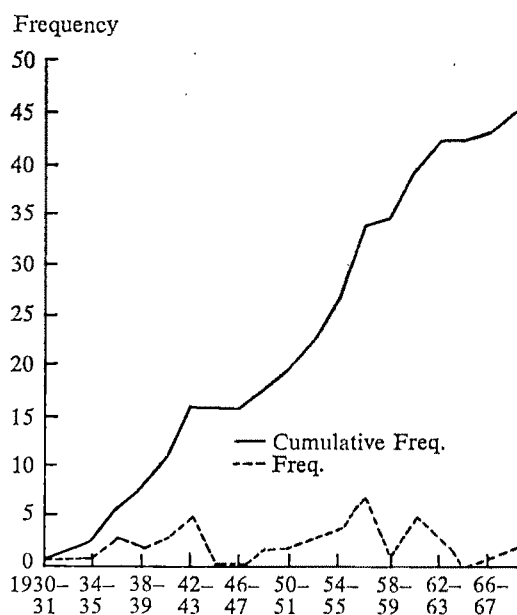


Figure 7. Adoption of degree requirement for teaching in elementary school by state, 1930-1969

borne out by the graph in Figure 9 for state merit systems covering welfare. Only nine states had such coverage before the Social Security Act was passed. After its passage 32 states decided to place their employees under a merit plan. Following the Social Security Board's successful fight to make merit plans a requirement, the remaining seven states followed suit. As a result, the pattern of spread for this innovation is somewhat different from that of other welfare policies. These results indicate the necessity for at least distinguishing

between state and state-federal control in this dimension of policy and perhaps distinguishing among various types of federal aid to states.

Sources of Innovativeness

The second consideration of this study is the question: Why do some states adopt before others? Walker hypothesizes that demographic (socioeconomic) and political factors are among the more important preconditions for innovation just as they are for expenditures; hence, his prediction is that the wealthier and

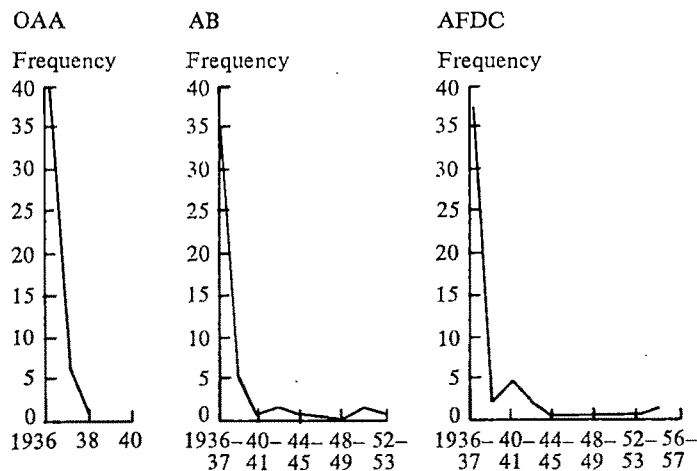


Figure 8. Adoption of Social Security programs by state, 1936-1955

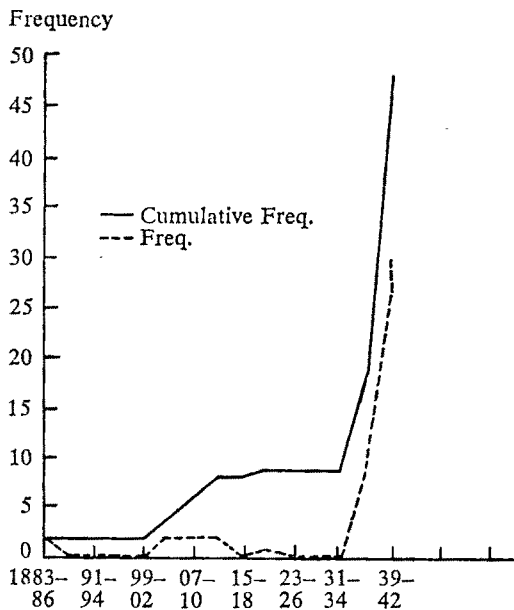


Figure 9. Adoption of welfare merit system legislation by state, 1883-1942

more competitive states ought to be more innovative. He found a strong positive correlation between average income and his innovation score in three time periods and little relationship between party competition and the composite innovation score for 95 years, when controlling for the demographic variable.²⁸

The averaging of innovativeness, party competition, and income over nearly a hundred-year period may serve only to obscure whatever relationships exist between policy and the political system. This investigation of the reasons for innovation will be based on a less rigorous test of the above political and economic hypotheses. The question of interest is why a state adopts a law at a particular point in time. Part of the answer (to why states adopt at all) lies in user interaction, as demonstrated earlier in this article. User interaction, however, is not a full explanation because it cannot account for the first adoption. Political and economic explanations may be more relevant to the most and least innovative states (i.e., why the first is susceptible and why the last is most immune), while user interaction might better account for the order of the states falling in the middle range of innovativeness. Therefore, this part of the inquiry will focus on the political and economic differences between the first adopters and the rest of the states at the time of adoption. A similar kind of analysis could not be

performed for the laggard states because for several laws, diffusion is not yet complete.

Analysis of First Adopters. One might predict that the most innovative states (defined as the first ten states to adopt any particular law) would be above average in wealth and competitiveness in whatever year they adopted the law. The wealth measure used is per capita personal income which is available since 1922. The party competition measure is the governor's electoral margin in the most recent general election.²⁹ An appropriate test is to compare an early adopter's competition (or wealth) score in the year of adoption to the mean competition (or wealth) score for all 48 states in that particular year. The early adopter's score should be above the nationwide mean in each case.

There are 120 possible observations on which to make the test (12 laws times the top 10 states). Unfortunately, much of the pertinent data is missing from standard sources because the adoptions occurred very early for many of these laws. There are only sixteen observations for party competition. Of these, twelve (75 per cent) are in the predicted direction: competition is higher in innovative states. There are 40 observations for per capita personal income. Thirty-four (89 per cent) are in the predicted direction: innovative states are richer.

Thus, it appears that innovative states are both wealthier and more competitive than their sister states at the time of adoption of a particular law. This finding is consistent with the hypotheses derived from studies of state and local expenditures.³⁰ The small amount of data is not sufficient for testing the independence of the two variables. Also the available data were in the fields of education and civil rights only. The relationships discovered were more clear-cut for civil rights laws than for education laws. This finding, along with the fact that education laws diffused in a more regular pattern than civil rights, is evidence that education may be the least politicized of the three policy areas. The following section documents one case in which politics was particularly important.

The Case of Mothers' Aid Legislation. As noted earlier, the user interaction model failed to account for the unique diffusion pattern displayed by AFDC, or Mothers' Aid, as it was

²⁸ For more complete explanation of these measures and their data sources, see Gray, chap. 2.

²⁹ Richard E. Dawson and James A. Robinson, "Inter-Party Competition, Economic Variables and Welfare Policies in the American States," *Journal of Politics*, 25 (May, 1963), 265-289.

²⁸ Walker, "Diffusion of Innovations Among American States," 884, 886.

called at its inception. Also the distribution of adopters is unusual as was shown in Figure 3. For these reasons another explanation of the fact that eighteen states adopted such legislation in 1913, the second year of its diffusion should be sought.

The origin of the idea seems to have come from the "White House Conference on the Care of Dependent Children" called by President Theodore Roosevelt in 1909. The theme of that conference was that home care for dependent children is preferable to their being placed in institutions.³¹ The innovation was opposed by social workers;³² it was popular with legislators, however, because no increased taxes were anticipated.³³ Thus, the presidentially sponsored conference may have been indirectly responsible for the rapid dissemination of the information, though not for rapid adoption of the legislation.

Certainly the outstanding political feature of this era was the Progressive Movement, and it is reasonable to suppose that politicians of Progressive sympathies would support social welfare legislation. Dewitt, writing in 1915 on the Progressive Movement, said that the social phase of the movement was by far the most important at the state level and that Progressive reformers advocated mothers' pensions.³⁴ In fact, a history of Progressivism in Ohio gives credit for Ohio's mothers' pensions to James Cox, a Democratic governor with Progressive sympathies elected in 1912.³⁵

In order to test for Progressive influence on the time of adoption, the states were divided into two groups: the eighteen states which adopted in 1913, and the 28 states which adopted later and for which data are available. For each group of states the mean percentage of Progressive party strength was computed, based on the elections for the governorship and the legislature immediately prior to 1913. This figure should underestimate the strength of Progressivism since politicians running on the regular party ticket also may have had Progressive sympathies.

³¹ Fred S. Hall, ed., *Social Work Yearbook, 1929* (New York: Russell Sage Foundation, 1930), pp. 131, 274.

³² James Leiby, *Charity and Correction in New Jersey* (New Brunswick: Rutgers University Press, 1967), p. 94.

³³ Ada J. Davis, "The Evolution of the Institution of Mothers' Pensions in the United States," *American Journal of Sociology*, 35 (January, 1930), 582.

³⁴ Benjamin Parke DeWitt, *The Progressive Movement* (New York: Macmillan Co., 1915), p. 253.

³⁵ Hoyt Landon Warner, *Progressivism in Ohio, 1897-1917* (Columbus: Ohio State University Press, 1964), p. 404.

The means for each group are displayed in Table 4. States which adopted mothers' aid legislation in 1913 averaged much greater Progressive strength than did states adopting later. Apparently, this is one case where politics played an important part in the timing of an innovation. In the following section the investigation focuses on patterns of innovation.

Patterns of Innovation. Throughout this study each innovation has been studied separately, in contrast to the method used by Walker. The previous sections have demonstrated the utility of disaggregation. Indeed, one might question the fundamental assumption of a "composite innovation score"—namely, that "innovativeness" exists as a single factor among states. Operationally, the question becomes "Do the states which are early to adopt one law also adopt other laws first as well?"

Table 5 shows how states rank in each of the three issue areas, and how they rank overall. The states were ranked from 1 (first) to 48 (last) on time of adoption of each law. Then a state average was computed for each issue area and overall. The overall ranking is similar to Walker's, partly because five of the laws included here were the same as his. There are some notable differences, however; Pennsylvania is 20th on this scale and 7th on Walker's; Nevada is 21st on this scale and 47th on Walker's; Louisiana ranks 19 on his scale and 36.5 here.

The interesting information that is concealed by a simple average ranking is the range for any one state. New York and California are the most innovative states; yet each has failed to adopt one of the laws in question. South Dakota is the most laggard state; nevertheless, it was among the first ten for one law. Thus, any study of policy innovation using averages

Table 4. Extent of Progressive Influence in States, According to Time of Adoption of Mothers' Aid Legislation

	Mean % Progressive Party Strength in Last Election Before 1913	
	Timing of Adoption	
Electoral Contest	1913 Adopters	Later Adopters
Governor	15.2%	6.5%
Lower House	7.0	.5
Upper House	6.0	1.2

Source: *The World Almanac and Encyclopedia, 1914* (New York: Press Publishing Company, 1913), pp. 729-778.

across issue areas may conceal phenomena of great potential interest.

Table 5. Ranks of the States According to Order of Adoption of Laws, Averaged by Issue Area

State	Overall Average Rank	Average Education Rank	Average Welfare Rank	Average Civil Rights Rank
California	13.0	6.1	11.5	26.5
New York	13.0	19.2	12.5	3.3
Massachusetts	14.8	25.0	8.7	6.0
New Jersey	16.0	24.9	13.3	4.8
Wisconsin	16.2	27.7	5.0	12.3
Connecticut	16.2	16.4	24.3	5.1
Washington	16.4	18.0	17.7	11.8
Colorado	16.8	26.3	9.3	11.1
Michigan	16.8	16.0	20.3	13.6
Illinois	17.2	25.0	10.1	13.6
Ohio	17.2	25.7	10.3	15.6
Idaho	19.0	23.6	12.1	20.5
Oregon	19.0	20.9	23.7	9.6
New Hampshire	19.9	22.4	16.3	20.5
Maryland	20.0	15.5	15.7	33.5
Indiana	20.4	16.1	33.5	10.1
Kansas	21.4	14.2	29.5	22.6
Utah	21.5	17.6	22.2	40.3
Minnesota	22.1	39.1	11.3	8.1
Pennsylvania	22.9	27.1	25.5	12.6
Nevada	23.5	24.0	10.5	40.3
Wyoming	25.3	27.6	21.7	39.8
Montana	25.5	29.7	25.7	18.1
Rhode Island	25.5	28.1	35.7	7.8
Kentucky	25.7	21.3	23.8	35.6
Vermont	25.9	18.1	29.2	37.8
Missouri	26.6	21.7	28.3	32.6
Arizona	27.2	22.8	27.6	34.1
Iowa	27.5	30.0	16.8	37.8
Delaware	27.8	25.6	28.1	31.1
Nebraska	28.2	35.7	18.8	28.3
Maine	29.6	34.4	18.8	36.0
New Mexico	29.7	25.2	38.3	26.0
North Carolina	30.2	17.5	42.7	48.0
West Virginia	30.9	25.4	25.1	48.0
Louisiana	31.7	21.1	32.8	48.0
Texas	31.7	23.0	41.1	48.0
Tennessee	31.9	23.2	30.7	48.0
South Carolina	34.3	24.4	36.5	48.0
Arkansas	35.0	30.9	30.3	48.0
Florida	35.0	28.4	33.5	48.0
North Dakota	35.4	38.1	28.7	39.8
Virginia	36.0	25.5	40.2	48.0
Oklahoma	37.1	36.2	30.1	48.0
Alabama	37.1	28.2	35.3	48.0
Mississippi	37.4	32.0	36.3	48.0
Georgia	37.7	31.0	38.5	48.0
South Dakota	38.4	37.9	32.0	48.0

Spearman rank-order correlations can be utilized to answer more directly the question of the stability of "innovativeness." If there exists a stable factor of "innovativeness," then states ranking high (or low) on a scale of time of adopting any one law ought to rank high (or low) on a time of adoption scale for another law. Certainly, within an issue area this principle ought to hold; i.e., the correlation between one education ranking scale and another education ranking scale should be positive. States were ranked from 1 (first) to 48 (last) on each of the twelve laws according to time of adoption.

Table 6 presents the correlation of the rank on one innovation scale with the rank on every other innovation scale. In general, the strength of the correlations is very low. Only eleven of 66 ($\frac{1}{6}$) are .50 or above; sixteen (about 25 per cent) are negative. About half of the stronger correlations are between laws which diffused during roughly the same time period. One might infer that some states are innovative at one point in time, but they are not necessarily innovative at another point in time; hence, "innovativeness" should not be aggregated over long time periods.

If "innovativeness" does not occur as a general timeless phenomenon, perhaps it is issue-specific, i.e., the same states might be innovative in all phases of education but not innovative on other issues. Table 7 shows that only for the issue of civil rights does the phenomenon of "innovativeness" truly appear to exist: the average of states' rank intercorrelations on civil rights laws is .67. States which are leaders (or laggards) in adopting one civil rights law are leaders (or laggards) on other civil rights laws. This finding, juxtaposed with those earlier concerning the civil rights distribution's stronger association with wealth and competition, is further evidence that civil rights laws are more politicized than are the laws in the other two policy areas.

Contrast this with the situation in the education field where not only is the adoption of education laws unrelated to adoption of laws in welfare and civil rights, but adoption of one education law is only slightly related to adoption of another education law; the average rank correlation among education laws is .17. Probably the length of time is one explanation, e.g., adoptions of one education law extend over the period 1784-1969. Also the five education laws are probably more diverse in substance than the set of three civil rights laws which evoke more powerful public reactions.

Another reason for the high intercorrelation among civil rights laws is that they diffused later in a shorter time period; many states have not yet adopted them. Technically, this means that there are a larger number of ties on the civil rights rankings; therefore, a correction was made for ties which has the effect generally of reducing the strength of the correlation, though the magnitude of the reduction was slight for these three laws.³⁶ At any rate, the civil rights rankings reported here are corrected for ties

³⁶ Sidney Siegel, *Nonparametric Statistics for the Behavioral Sciences* (New York: McGraw-Hill Book Co., 1956), p. 210.

Table 6. Spearman Rank-Order Correlations (ρ) For 12 Innovation Rankings

Laws	Education				Welfare				Civil Rights		
	Chief School Officer	Compulsory Attendance	Degree for Elem. School	Degree for High School	OA	AB	AFDC	Merit System	Public Accommodations	FEPC	Fair Housing
State School Boards	.07	.06	.27	.18	-.06	.12	-.22	-.04	-.09	.20	.08
Chief School Officer		-.29	-.16	-.05	.01	.03	-.03	-.16	-.28	-.13	.00
Compulsory Attendance			-.08	.75	.43	.36	.55	.38	.60	.50	.53
Degree for Elem. School				.42	-.11	-.17	-.11	.12	-.08	.30	.27
Degree for High School					.02	.09	.01	.20	.07	.21	.10
OA						.38	.52	.37	.36	.38	.48
AB							.50	.37	.46	.39	.37
AFDC								.37	.37	.46	.55
Merit System									.45	.49	.48
Public Accommodations										.65	.65
FEPC											.71

and are still much different from all other uncorrected rankings.

Welfare occupies an intermediate position in respect to intercorrelation. There is a moderate amount of underlying stability within the welfare dimension but slightly more relationship between the welfare and civil rights areas than within welfare itself.

Table 7. Spearman Rank-Order Correlations for Date of Adoption, Averaged by Issue Area

	Education	Welfare	Civil Rights
Education	.17	.07	.15
Welfare		.41	.44
Civil Rights			.67

Summary

Innovations (adoption of laws by states) were studied in three "have-not" oriented policy areas: education, welfare, and civil rights. A model of the diffusion process based on user interaction was constructed; it performed fairly well under evaluation by several common criteria. Graphical analysis showed, however, that diffusion patterns do differ by issue area and by degree of federal involvement.

Political and economic explanations proved to be useful in determining which states are the first to adopt laws. A brief case history of the adoption of Mothers' Aid legislation pointed up the strong effect of Progressive sympathies on early adopters. Finally, it was shown that "innovativeness" is not a pervasive factor; rather, it is issue-and time-specific at best.

Comment: Problems in Research on the Diffusion of Policy Innovations*

JACK L. WALKER

University of Michigan

Diffusion theory has been an important tool for analysis by geographers, sociologists and economists, but the diffusion of diffusion theory among political scientists, so far, definitely does not approximate an S-shaped curve! In fact, as far as I know, Professor Gray's is only the second article in *The Review* that employs the word "diffusion" in its title.¹

The obstacles to the spread of diffusion theory among political scientists do not exist in the essential nature of political phenomena. The American federal system can usefully be conceived as an elaborate diffusion process, as Gray's article demonstrates, and many other cases where distinctive approaches to policy have evolved within some part of the governmental system might better be understood if seen as examples of diffusion. The reasons for political scientists' neglect of diffusion theory, of course, lie not in the peculiarities of their subject matter, but in the prevailing division of labor among scholarly disciplines in American universities. The various branches of learning stake out their special preserves and claim exclusive jurisdiction over certain variables, research problems, and explanatory theories—diffusion processes have been centrally important for anthropologists but have rarely been employed by students of politics.

There is no good intellectual justification for restricting the use of any explanatory scheme to a limited class of social problems. All the social sciences need as much theoretical help as they can get. Certainly, Gray's article, with its references to material from several disciplines, is a model of the ecumenical spirit and a good example of the progress that can be made through judicious borrowing from other scholarly traditions.

Professor Gray set out in her article to "extend in a more rigorous fashion the investiga-

tion of innovation by states"² that I began four years ago. It would be unduly coy of me not to express satisfaction at this evidence that another political scientist found my work stimulating enough to use it as a guide for further investigation. The mathematical model that is the centerpiece of this article should be useful for analyzing data in several other settings and should stimulate further interest in diffusion theory within our discipline. It is important to see, however, that Gray has not only employed different techniques of analysis than I did, but has also studied a somewhat different aspect of the diffusion process, and has structured the problem in a different way. These crucial variations in our approaches seem to have led to some misunderstanding between us about the use of terms, and they may account for some of the apparent differences in our findings.

Our two articles attack the same general problem, using similar data, but with contrasting assumptions and ultimate aims. Our findings differ in some important ways, but because this is still a largely unexplored area, the most significant aspect of the two presentations is the kind of guidance they offer for future research. In this comment, besides defending my own approach which I believe takes us in more fruitful directions, I shall attempt to provide insights into the principal conceptual and methodological problems that face anyone wishing to investigate the diffusion of public policies among the American states, or among nations or other governmental units. The comment begins with a brief résumé of my own article, and proceeds with an analysis of the way Gray and I have answered some of the more serious questions of measurement and inference that arise in this field.

Curves Versus Spreading Ink Blots

The central problem I dealt with involved

* Thanks are due to James Alt of the University of Essex, Sidney Winter, my colleague at the Institute of Public Policy Studies at the University of Michigan, and my wife, Linda G. Walker, who read an earlier draft of this comment. Their criticisms and suggestions saved me from many mistakes of inference and expression, but what finally emerged is entirely my responsibility.

¹ See Kenneth Janda, ed., *Cumulative Index to the American Political Science Review* (Evanston, Ill.: Northwestern University Press, 1964).

² Virginia Gray, "Innovation in the States: A Diffusion Study," *American Political Science Review*, 67 (December, 1973), pp. 1174-1185. The article to which she refers is: Jack L. Walker, "The Diffusion of Innovations Among the American States," *American Political Science Review*, 63 (September, 1969), 880-899. A revised version of this article appeared in: Herbert Jacob and Kenneth N. Vines, eds., *Politics in the American States*, 2nd edition (Boston: Little, Brown and Co., 1971), pp. 354-387.

the relative speed with which states adopted new ideas. I was interested in whether some states characteristically adopted new programs earlier than other states and if so, how this phenomenon might be explained. I began my research by developing a measure of the average speed of adoption for all the states and then tried to discover its economic, social and political correlates. Gray questions the validity of this index of innovativeness because she doubts that states remain peculiarly innovative over a long period of time or that a quality called "innovativeness" could ever be isolated and independently described. I disagree with her over this question, but more about that below. The important point I wish to make at the outset is that I was primarily concerned with the propensities of states to adopt new programs and attempted to create an empirical index of this characteristic, averaged over many possible adoptions. I was not concerned, as is Gray, with the cumulative frequencies of adoptions over time or whether the pattern of adoptions approximated an "S"-shaped curve.

I did refer in my article to "patterns" of adoption, but did not use the term to refer to cumulative frequencies. I advanced the hypothesis that a typical diffusion pattern existed in which one of the pioneering states adopted first, followed by other pioneers, and finally by other states which tended to take cues from their regional pioneers. This diffusion process forms an essentially geographical pattern, and can be visualized as a succession of spreading ink-blots on a map created by the initial adoptions of new policies by states playing in a national "league" of cue-taking and information exchange, followed by other states whose standards of comparison and measures of aspiration are more parochial and who typically adopt new policies only after others within their regional "league" have done so.

In view of these differences in our use of the phrase "pattern of adoption" I believe Gray is unfair in charging that I did not see differences between the patterns of adoption of innovations that arose from state initiative and those that were launched at the federal level or were subject to federal intervention at some point in the course of their diffusion.³ As I gain experience in research and the distractions of university life I can see that it is a mistake ever to promise further work on a problem, even if one honestly expects to do it, but I did spend some time with the question of federal intervention and found, as I reported, little difference in the rank ordering of states on such issues, or in the es-

entially geographical patterns I was looking for.

Gray's analysis of the shape of diffusion curves clearly leads to the presumption that federal intervention makes a difference in the temporal frequencies of adoption, generally by accelerating the process and encouraging the spread of innovations to all the states. This is an important finding that might eventually allow the classification of issues according to differences in their diffusion patterns. Of course, programs that diffuse to the great majority of states within only three or four years, such as Aid to the Blind or Old Age Assistance, even if they constitute an analytically separate class of issues within some broader theory of diffusion, nevertheless are difficult to study because of their speed of diffusion. Before this aspect of Gray's inquiry can be exploited, a wider selection of programs affected by federal intervention must be analyzed, but this line of research appears to offer many interesting possibilities. It should be understood, however, that Gray and I were interested in different kinds of diffusion patterns, each important in their own right. Even though our data were similar, our discoveries were different because we were asking different questions, as well as using different methods.

Regional Barriers to Interaction

This apparent misunderstanding between us over the meaning of the phrase "diffusion patterns" helps to reveal the differences in our projects and to show how much explanatory power Gray has had to give away in order to achieve the kind of rigor that characterizes her presentation. One central assumption upon which her diffusion model is founded is that all states may be treated alike for the purposes of analysis, or to use her terminology, she assumes that she is working with "a completely intermixed population."⁴ My research specifically was designed to show that the American states are *not* a completely intermixed population, but rather are organized into a complex system of pioneers, regional leaders and laggards. I identified some important developments that were creating pressures for national uniformity, such as the growing cosmopolitan communities of public officials and policy experts, and made some rather oblique efforts to measure these trends, but my conclusion was that regional differences were remarkably persistent and unlikely soon to disappear. Gray certainly recognizes that regional biases in the flow of communications among adopters may make it unreasonable to

³ Gray, p. 1185.

⁴ Gray, p. 1176, 1179.

regard the states as a completely intermixed population and refers several times to the possibility of creating more elaborate models of diffusion by relaxing this fundamental assumption,⁵ but presumably this would be a much more difficult undertaking that would not have the economy or simplicity of her present model. These remarks are not meant to deny the obvious benefits of creating formal models, but merely to underline the limitations of this one as a description of reality.

This questionable basic assumption hinders Gray in explaining the diffusion of civil rights bills, for which she reports that the "normal" diffusion process, based on the interaction among decision makers, "is met by resistance from particularly immune states."⁶ These fourteen resistant states, of course, with one exception (South Dakota), are contiguous and clustered in the South.⁷ It might be an interesting test of Gray's model if she removed the thirteen southern states, or perhaps only the eleven states of the Confederacy usually designated as southern in such studies, and ran the model for the remainder, among which the assumption of complete population intermixture might be more legitimate. If this exercise were performed on the states outside the South, the interaction term in her diffusion model might begin to assume the importance it does for most cases in education and welfare.

Regional peculiarities that produce "interaction effects"⁸ in her data also call into question Gray's assertion that civil rights legislation is more likely to be caught up in partisan disputes than are topics such as teacher certification or AFDC. This inference grows from several findings, including the fact that rank orderings of the states according to the speed of adoption of civil rights bills have stronger associations with measures of relative wealth or political competition than rank orderings based on legislation in the other two categories. These high correlations, however, may result only from the tight clustering of southern states at the bottom of all these scales, a fact that may have arisen merely by coincidence. Findings of this kind may be only a reflection of the great differences between the South and the rest of the country

and may not be an indication of functional relationships among these variables within all the states. If states in the South and those in the rest of the country were treated as independent groups the relationships between the speed of civil rights adoption and wealth or political competition may disappear within each group. In research of this kind, where such distinctive regional clustering appears, correlations should be run with the distinctive region removed before generalizations or further broad inferences are made about functional relationships or causative factors.⁹ A more elaborate model that takes account of regional barriers to interaction among potential adopters would still be needed, even if such tests were made, but these extra pieces of analysis might prevent Gray from drawing misleading conclusions from findings that may only be statistical artifacts.

Differences Between Issues and Adopters

The differences in the fundamental assumptions Gray and I make concerning the nature of the system we are investigating have helped to determine our choices of research strategies as well as the course of our analyses. Gray, for example, argues that each policy innovation should be studied separately so that the peculiarities in each diffusion pattern can be discovered and accounted for. She doubts the usefulness or validity of my efforts to create a composite index of innovation because she does not believe that any quality called "innovativeness" can be attached to any single state government for any significantly long period of time.¹⁰ Her assumption of the interchangeability of states, of course, is consistent with the careful analysis of individual issues. By testing each distribution of cumulative frequencies of adoption against the prediction that it will approximate normality Gray has an unchanging standard for comparison. Using this method her attention usually is directed to the peculiarities of the issues under examination that cause them to diffuse in an "abnormal" way—whether they are presented for adoption with accompanying federal grants-in-aid, for example, or stimulate unusual amounts of partisan controversy. This research strategy is akin to those used in studies of the diffusion of technological artifacts to farmers or small businesses where assumptions about

⁵ Gray, pp. 1176, 1179, and footnotes number 21, 23 and 25.

⁶ Gray, p. 1180.

⁷ These data are provided by Gray in Table 5, p. 1184.

⁸ The phrase "interaction effects" is being used as it was in: Hugh Donald Forbes and Edward R. Tufte, "A Note of Caution in Causal Modelling," *American Political Science Review*, 62 (December, 1968), 1261-1262. For similar commentary see the discussions of the "universal fallacy" and the "contextual fallacy" in Hayward R. Alker, Jr., *Mathematics and Politics* (New York: Macmillan, 1965); pp. 101-106.

⁹ For a more lengthy presentation of this view, see: Dennis Riley and Jack L. Walker, "Communication," *American Political Science Review*, 63 (September, 1969), 900-903. For an interesting discussion of analytical problems created by regionalism, see: John L. Sullivan, "Political Correlates of Social, Economic, and Religious Diversity in the American States," *The Journal of Politics* (February, 1973), pp. 70-84.

¹⁰ Gray, pp. 1183-1185.

the complete intermixture of the populations are appropriate, or at least not so risky as in the case of a vast, continentwide governmental system.¹¹ Gray's method has yielded several important insights, as the case study approach often does, and my own work probably suffered from inadequate sensitivity to the importance of the characteristics of policy innovations as determinants of the course of their diffusion. Nevertheless, as long as Gray takes up issues one at a time and concentrates her attention on the nature of the objects being diffused she is unlikely to discover systematic or recurring differences in the responses of individual state governments to the prospects of change—the problem that was my special concern.

In my own research, the composite innovation score played the same role that Gray's assumptions about normal distributions play in hers. That is, it provided a framework for analysis and a basis for comparison. My efforts were directed at explaining why some states, on the average, adopted new ideas more rapidly than others—why New York tended to adopt most new policies before Mississippi—and in this inquiry I drew most often on insights from theories that emphasize the importance of structural and procedural differences among organizations or business firms in the way they process information, search for new ideas, or measure their achievements.¹² My attention was focused on the agencies adopting the innovations and on those stable aspects of their environment—especially regionalism and the growing professionalization of public bureaucracies—that seemed to be shaping their behavior. Gray touches on most of these factors in her presentation, but her approach leads her to focus most of her attention on the traits of the innovations being diffused, a mode of analysis that is unlikely to uncover fundamental differences among the adopters. My concern was primarily with the decision rules being employed by those to whom proposals for change were

being presented, and not so much with the importance of differences among the policy innovations themselves.

No researcher in this area will ever be presented with a simple choice between studying the nature of the innovations or the behavior of the adopters. Policy innovation and diffusion are much too complicated to allow anyone to ignore one side of the process altogether, although, as I have tried to show, most research designs place greater emphasis on one of the two aspects of the problem. This central analytical distinction is important, however, since it has significant consequences both for research design and for the formulation of generalizations and new hypotheses. Since limited resources usually require that attention be restricted to a small number of problems, most researchers must choose one emphasis over another, but decisions about these problems of research design are more than mere matters of taste. Even after reading Gray's paper, I still believe that efforts to develop a theory of organizational innovativeness and to create a better description of the specialized intergovernmental communication links within the American federal system offer greater potential theoretical payoffs. It is very likely that "innovativeness" is an impermanent condition, and that periods of pioneering innovation by a state government often are followed by periods of consolidation, or even stagnation, but this presents us with the challenging task of explaining these cycles of change. Even though Mississippi was the least innovative state on my composite index, it was the first to adopt the sales tax and recently has pioneered in allowing conjugal overnight visits for prisoners in its jails and penitentiaries.¹³ This may indicate, as Gray's analysis suggests, that within certain classes of issues, innovations may originate on the periphery rather than in one of the system's cosmopolitan centers.¹⁴

We probably can never hope to account for more than a modest amount of the variance, however, in so immense and elaborate a system. The presence of a single aide on a legislative staff who is enthusiastic about a new program, or the chance reading of an article by a political leader can cause states to adopt new

¹¹ For a review of diffusion research that includes an extensive bibliography of more than 1,500 items see: Everett M. Rogers and F. Floyd Shoemaker, *Communication of Innovations: A Cross-Cultural Approach* (New York: Free Press, 1971).

¹² The body of theory upon which I drew is summarized best in Donald W. Taylor, "Decision Making and Problem Solving" in *Handbook of Organizations*, ed. James G. March (Chicago: Rand McNally, 1965), pp. 48–86. Also see: Julia Feldman and Herschel E. Kanter, "Organizational Decision Making," *Ibid.*, pp. 614–649; W. Richard Scott, "Theory of Organizations" in *Handbook of Modern Sociology*, ed. Robert E. L. Faris (Chicago: Rand McNally, 1964), pp. 485–529; and Lawrence B. Mohr, "Determinants of Innovation in Organizations," *American Political Science Review*, 63 (January, 1969), 111–126.

¹³ For a discussion of the genesis of the latter program and reactions to it in other parts of the country, see: Columbus B. Hopper, *Sex in Prison: The Mississippi Experiment with Conjugal Visiting* (Baton Rouge: The Louisiana State University Press, 1971).

¹⁴ For an insightful article that illustrates this point in reference to innovations in public health, see: Marshall H. Becker, "Sociometric Location and Innovativeness: Reformulation and Extension of the Diffusion Model," *American Sociological Review* (April, 1970), pp. 267–282.

programs more rapidly than might normally be expected. We are studying an immensely complicated social system in which random occurrences and chance factors are prominent ingredients, but regardless of these difficulties I am not prepared as yet to give up my emphasis on organizational factors as keys to understanding. nor am I able to accept Gray's alternative notion that New York can be regarded as interchangeable with Mississippi or Arkansas, or that California can be placed, for purposes of analysis, in the same class as Wyoming, Vermont, or Rhode Island.

Innovation Scores and Sampling

Gray's demonstration that the twelve issues she dealt with were not strongly intercorrelated is not sufficient grounds for abandoning efforts at measuring the factor of innovativeness in state governments. She chose programs that were relatively free of federal influence and were objects of the struggle between haves and have-nots, because she wanted to evaluate the importance of political, rather than economic factors, that exist within the states rather than at the national level, as determinants of diffusion. Also, the issues she studied came from only three issue areas, compared with twelve in my study. Yet even though different criteria of selection were used in our two studies, the composite score Gray produced still roughly approximates the one I presented. Gray is certainly justified in claiming that a composite score may cover up interesting phenomena, and her article gives evidence of the insights that can be gained from disaggregation. Some merging of our techniques probably will be necessary to produce a full picture of this complicated diffusion process, but where the composite innovation score is concerned, my hunch is that most new scores based on a relatively broad selection of issues using almost any issue typology as guidance would very closely resemble the one that emerged from my data.

Admittedly, this is little more than an assertion of faith, but it cannot be given a conclusive empirical test until a method can be devised for selecting a representative sample of bills from whatever issue areas or policy arenas one chooses to study. Neither Gray nor I have dealt adequately in our research with the problem of sampling. Hundreds of thousands of programs have diffused through the federal system during the past fifty years alone and we have no satisfactory way of cataloguing or listing them as a basis for sampling. Although each of us has constructed criteria for issue selection that seem reasonable, we have no guarantee that the issues we are studying are not

highly unusual and somehow unrepresentative of their class. This problem is not serious enough, in my opinion, to halt inquiries into diffusion, but until more reliable sampling techniques are devised we must use caution in evaluating descriptive statements of any kind arising from anyone's research.

Reasons for Studying Organizational Innovativeness

Naturally, it would be possible to design a study that concentrated on the traits of the issues being diffused without also making the assumption that the American states can be treated as a completely intermixed population. I believe this assumption is a serious weakness in Gray's approach, as should be plain by now, but I do not want to make too much of it. There are other serious obstacles to building theories of diffusion that employ traits of the innovations as factors determining their diffusion patterns, and principal among these is the fact that the proposals themselves often are constantly being modified during the course of the diffusion process.¹⁵ Can we safely make the assumption that the legislation providing Aid to Families with Dependent Children in the 1930s or 1960s is actually just another name for the Mother's Aid Legislation of the Progressive era? Is it not likely that these proposals have been so thoroughly modified that they are actually different bills and should be treated separately rather than as integral parts of a continuous process of diffusion? Without doing the necessary investigation, I cannot answer these questions, or similar ones that arise about the diffusion of state boards of education that took place over a period of 165 years. It will not be easy, however, to construct an objective method of deciding when an alteration in the details of a program is only a modification of the original or a change of such magnitude that it constitutes an innovation in itself worthy of independent analysis. This problem plagues all diffusion research that deals with phenomena as complex as social programs, and in principle it is not insurmountable, but until it is dealt with, the researcher who concentrates on studying the shape of diffusion curves cannot be sure whether the process being observed is a discrete event or whether it represents the aggregation of two or more independent diffusion processes.

Beyond these serious conceptual problems

¹⁵ For a more extensive discussion of this problem see: Kenneth E. Warner, "The Need for Some Innovative Concepts of Innovation: An Agenda for Research" (Working Paper Prepared for Faculty Seminar on Innovation and Social Change, Institute of Public Policy Studies, University of Michigan, 1972).

facing alternative approaches there also are positive reasons for innovation researchers to concentrate on such questions as the information sources utilized by decision makers, the prevailing decision rules being used by potential adopters, and the determinants of organizational goals or aspirations. It seems more likely that work on this cluster of problems will help us explain the first adoption of new programs by pioneering states before they begin to diffuse throughout the federal system. There have been few systematic attempts by students of diffusion to deal with this important aspect of the problem, just as political scientists have made little effort to discover the origins of public policies. With the notable exception of the dispute over "nonissues"¹⁶ and some mainly speculative discussion of political culture,¹⁷ political scientists generally have taken the issues that appear on the political agenda as given and have defined their task as the study of conflict resolution and the clash of vested interests, mainly in the electoral and legislative systems, that result from efforts to implement these proposals. The question of how issues evolve and present themselves for adoption has been given little attention.¹⁸ One cannot study everything at once, unfortunately, and few of the problems of diffusion have been laid to rest, but if our theories of the way policies spread through the federal system are built from studies of decision making, cue taking, and the flow of information between organizations, they are more likely than explanations built from an inspection of diffusion curves to merge with theories of how problems are identified and new proposals created in the first place. The processes of innovation and diffusion go together, and so should theories that are meant to explain them.

¹⁶ This literature is reviewed in Raymond E. Wolfinger, "Nondecisions and the Study of Local Politics," Frederick W. Frey, "Comment: On Issues and Nonissues in the Study of Power"; and Raymond E. Wolfinger, "Rejoinder to Frey's Comment," *American Political Science Review*, 65 (December, 1971), 1063-1104.

¹⁷ I refer to: Daniel J. Elazar, *American Federalism: A View From the States* (New York: Crowell, 1966); and Edward C. Banfield and James Q. Wilson, *City Politics* (Boston: Harvard University Press, 1963). Also see: Raymond E. Wolfinger and Fred I. Greenstein, "Comparing Political Regions: The Case of California," *American Political Science Review*, 63 (March, 1969), 74-85.

¹⁸ Two recent studies that include interesting efforts to explain the origins of policy are: Matthew A. Crenson, *The Unpolitics of Air Pollution* (Baltimore: Johns Hopkins University Press, 1971); and Mark Nadel, *The Politics of Consumer Protection* (Indianapolis: Bobbs-Merrill, 1972).

A Plea for More Research

Before setting out to investigate the diffusion of public policies among the American states, one must make several important decisions about the strategy and tactics of research. In this comment I have touched on some of the most important problems that arise at the earliest stages of research design, such as what fundamental assumptions about the nature of the federal system can legitimately be made, what kinds of findings can be taken as acceptable confirmation of central hypotheses, upon what bases issues should be selected or samples constructed, whether to focus mainly on the policies being diffused or to pay more attention to the adopting units—or how to merge these two concerns—and finally, what further theoretical problems or empirical questions can be attacked once the study being planned has been completed. All these questions would have to be dealt with, although in slightly different form, if one were to study the international diffusion of innovations¹⁹ or the spread of new policies among cities or counties.

I hope this brief commentary on Gray's article provides realistic illustrations of how these questions affect research on diffusion and how the two of us differ in our approach to the subject. Although both approaches offer promise of opening up a previously uncharted region for more intensive scholarly exploration, our techniques differ and so do our answers to some of the fundamental conceptual problems in the field. All scholars are inflicted with the imperialistic notion that their field of interest deserves much greater recognition and attention from their colleagues. It is an academic commonplace to call for more research on almost any problem. In the case of the innovation and diffusion of public policies, however, more research, recognition, and interest actually are needed, so I do not feel reluctant to make such a plea. Neither Gray nor I have exhausted the possibilities for significant investigations in this area. It is my hope that Gray's article and this dialogue between us over the problems and challenges of research on diffusion and innovation will attract many more political scientists into this important field.

¹⁹ For a study of the international diffusion of social security programs see: David Collier and Richard E. Messick, "Functional Prerequisites Versus Diffusion: Testing Alternative Explanations of Social Security Adoption," (Paper Delivered at the 1973 Annual Meeting of the Midwest Political Science Association, Chicago, Illinois).

Rejoinder to "Comment" by Jack L. Walker

VIRGINIA GRAY

University of Minnesota

I enter the final round of this dialogue with the same scholarly intent Professor Walker has so ably expressed—to wrestle with the problems and challenges of research on diffusion of innovations in the hope of attracting attention to this relatively unexplored realm of policy research.¹ Perhaps we should be worrying about a more practical problem of diffusion: in Rogers and Shoemaker's review of the literature, including a bibliography of 1500 studies of innovation, political science is not recognized as having a tradition of research in this field.² Yet, both of our studies refer to this vast body of research in the other social sciences; mine in particular relies heavily upon previous theoretical work, resulting in some differences in the approaches of Walker and myself and thus sometimes in our different conclusions. Professor Walker has done an excellent job of summarizing those differences so I will confine myself to discussing first a few technical points in which we remain in disagreement; second, a more fundamental difference we apparently have on the usefulness of models in social science; third, the direction of future research.

Technical Points of Disagreement

As Professor Walker makes clear, his "patterns of adoption" refer to geographical patterns while mine refer to the tendency of the distribution of adoptions over time to approach a normal curve. His footnote in the original article asserting that there is little difference in the diffusion patterns of issues of different types appears in Section I, "Definitions and Distinctions," following an introduction where a long footnote discusses the theory of diffusion used in other fields.³ It is not until Section V that he develops his argument about the emulation of pioneer states and cue taking from neighboring states. So I believe my interpretation to be a

reasonable firsthand one, but I stand corrected. Walker says this diffusion process "can be visualized as a succession of spreading ink-blots on a map."⁴ I recall trying a rough approximation of this technique—coloring a map for each of twelve laws—and found no discernible regional variation for time of adoption among my data.

One disagreement I have is with Walker's suggestion at several points that one should remove the South for purposes of testing the model. This line of thinking is offered all too frequently in the state politics literature; to throw out the deviant, and hence the interesting, cases really begs the question of explanation. In one exercise Walker suggests this procedure because he believes the removal of the southern states would make the random mixing assumption more realistic.⁵ However, the problem he finds does not stem from this "unrealistic" assumption of the model but from the fact that the civil rights laws have not yet completely diffused, and thus the data for adequate validation are not at hand.

In the second exercise where Walker suggests this procedure he fails to understand my section "Analysis of First Adopters."⁶ In this section I found that the first ten states to adopt any particular law were usually more politically competitive and wealthier than the nationwide average in the year of adoption. It is important to note that after ranking and scoring the states according to time of adoption, with many southern states falling at the bottom of the scale, I analyzed only the first ten states to adopt; these, usually, were not southern states. Hence, my finding is not a "statistical artifact" resulting from a failure to treat the South as a separate group; my analysis does not rest at all upon those cases.⁷ But as a matter of idle speculation, just how would one go about rank ordering states on relative time of adoption to obtain an ordinal score for the purpose of further statistical analysis and at the same time leave out a quarter of the states making up the ranking for this particular political system?

One final technical point of misunderstanding between Walker and myself concerns my

¹ Among those few studies are Edgar C. McVey, "Patterns of Diffusion in the United States," *American Sociological Review* (April, 1940), pp. 219-227; Thomas M. Scott, "The Diffusion of Urban Governmental Forms as a Case of Social Learning," *The Journal of Politics* (November, 1968), pp. 1091-1108; Robert L. Crain, Elihu Katz, Donald B. Rosenthal, *The Politics of Community Conflict* (Indianapolis: Bobbs-Merrill, 1969), particularly chapter 2.

² Everett M. Rogers and F. Floyd Shoemaker, *Communication of Innovations: A Cross-Cultural Approach* (New York: The Free Press, 1971).

³ Jack L. Walker, "The Diffusion of Innovations Among the American States," *American Political Science Review*, 63 (September, 1969), 882.

⁴ Jack L. Walker, "Comment: Problems in Research on the Diffusion of Policy Innovations," *American Political Science Review*, 67 (December, 1973), 1186-1191.

⁵ Walker, "Comment," p. 1188.

⁶ Walker, "Comment," p. 1188.

⁷ Walker, "Comment," p. 1188.

use of rank-order correlations to get at the underlying structure of "innovativeness." I agree with him that my finding of little interrelatedness among twelve issues does not imply that we abandon research on "innovativeness." But my findings do constitute, I think, a caveat that one ought not to assume, a priori, that "innovativeness" is a stable factor; rather, one must establish the existence of "innovativeness" before he seeks to explain it. Walker does not sufficiently validate his innovation scale before he uses it.

Fundamental Disagreements

Our disagreement here is rather like that over a half-glass of water, whether it is half full or half empty. Walker asserts that because of my fundamental assumption that the population is completely intermixed I have had to give away much explanatory power.⁸ On the contrary, I think I have bought much explanatory power by beginning with one simple model based upon one "unrealistic" assumption. As Milton Friedman says, "the relevant question to ask about the 'assumptions' of a theory is not whether they are descriptively 'realistic,' for they never are, but whether they are sufficiently good approximations for the purpose at hand."⁹

Many other diffusion theorists have considered this assumption, unrealistic though it may be, to be sufficient at first. Both Bartholomew's birth process model for the diffusion of news and rumor and Coleman's social structure model begin with this assumption and then go on to more complex and more realistic theoretic models by relaxing this assumption.¹⁰ However, these more complex models quickly become intractable when their empirical validation is attempted. For this reason I retained my simple model; certainly the assumption does not make New York interchangeable with Mississippi; rather it represents formally a single theory which attempts to explain both states' innovations.

Furthermore, Walker fails to understand that the theoretic assumption of a completely intermixed population (for the sake of simplicity of the model) is not wedded to whether the distri-

butions of adoptions over time are normal. As I mentioned, many other theories can be constructed to fit this curve and their formal representation in a mathematical model may be simple or quite complex, e.g., allowing for constant source diffusion, within-group but not between-group interaction, and disadoption.

I elected to leave the model in its simple state and to work in the direction pointed out by failures of the empirical model to yield accurate predictions—toward developing a political explanation for Mothers' Aid, for example. When one uses this method, beginning with individual issues and not their aggregations, which is far from a "case study approach" as Walker calls it, the likelihood is greater, I think, for the emergence of patterns among states, issue areas, and other phenomena of interest to political scientists than when one begins his study by aggregation.¹¹

Direction of Future Research

I agree entirely with Walker that research is needed both on the macro level (the nature of the diffusion process of innovations) and on the micro level (the behavior of adopters as political actors). Also it is critical to establish the bounds of the universe to which our findings are generalizeable. A related problem is that of clarifying the operational definition of an innovation. I have relied on the data source in each case which reports, usually in a single table, when states adopted some law. Over time, however, as the language begins to diverge (as in the old game of "Gossip") the last adopter might not be really innovating in exactly the same manner as the first adopters. One could, of course, compare the language of the two bills from the states' legal codes, not an insurmountable task unless the number of innovations being studied is large.

Finally, I agree with Walker that a theory such as mine based on user interaction obviously cannot account for the first adopter; similar theories, however, have been applied in varying degrees of rigor with success to a range of social phenomena. This is usually taken to be an indicator of the generality of a theory—it can explain what seems to be a wide variety of behaviors. Another virtue of a good theory is that it is consonant with, or can subsume, other theories within the same paradigm. Thus, I, in contrast to Walker, can see no intellectual reason why a theory of diffusion which is parsimonious, powerful (in its predictive ability here), and general cannot merge with appropriate theories of the origin of innovations in a social system.

⁸ Walker, p. 1187.

⁹ Milton Friedman, *Essays in Positive Economics* (Chicago: The University of Chicago Press, 1953), p. 15. More strongly, Friedman asserts that "truly important and significant hypotheses will be found to have 'assumptions' that are wildly inaccurate descriptive representations of reality, and, in general, the more significant the theory, the more unrealistic the assumptions (in this sense)," p. 14.

¹⁰ David J. Bartholomew, *Stochastic Models for Social Processes* (London: John Wiley & Sons, 1967), p. 206; James S. Coleman, *Introduction to Mathematical Sociology* (London: Free Press of Glencoe, 1964), chapter 17.

¹¹ Walker, "Comment," p. 1189.

Economic Growth and Intergenerational Change in Japan

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Introduction

During the 1960s, comparative politics became concerned with the problems of the developing areas, leading to a proliferation of books and articles on what are now familiar themes—political development, modernization, and nation building. In the 1970s, interest in these themes will no doubt continue, but at the same time problems relating to the advanced industrial societies will probably command increasing attention. A case in point is a recent study by Ronald Inglehart on intergenerational change in the industrial societies of Western Europe.¹

The question of what happens to politics when industrialization and economic growth produce affluence is both intriguing and important. For a time it was believed by some, for instance Daniel Bell, that affluence would lead to an "end of ideology." Recent events, such as the rash of student revolts and the rise of New Left protest movements in various countries, have cast doubt on the validity of that thesis. In this context, Inglehart's study is revealing and provocative, for it suggests, on the basis of survey research data, that affluence might lead in time to a restructuring of political loyalties in Europe.

Professor Inglehart develops his thesis by integrating theory and empirical data. His theory rests on the idea derived from the late Abraham Maslow's writings on human motivation that individuals have a hierarchy of values, "giving maximum attention to the things they sense to be the most unsatisfied needs at a given time."² According to this theory, human beings first must seek to fulfill subsistence needs—food, water, shelter—in short, economic security. When this basic need is met, individuals then seek higher values, namely sense of belonging, followed by self-esteem, and intellectual and aesthetic fulfillment. A further assumption is that individuals tend to maintain a given hierarchy of values "once a basic character has been formed during childhood and

youth."³ It would follow from this that the milieu in which a person has been socialized is important.

The most dramatic change in milieu that has taken place, according to Inglehart, is the rapid economic growth leading to affluence which nations in Western Europe have come to experience in the postwar era. Today's youth "have been socialized during an unprecedentedly long period of unprecedentedly high affluence. For them, economic security may be taken for granted, as the supply of water or the air we breathe once could."⁴ The result is a generation gap: the older generation, having been socialized in a period of scarcity, clings to acquisitive "bourgeois" values, while the younger generation gives priority to "postbourgeois" values "relating to the need for belonging and to aesthetic and intellectual needs."⁵ Inglehart foresees, moreover, that as the younger generation takes over during the next decades, either new political parties responding to value cleavages might emerge, or there might be a realignment of the social basis of existing parties. Specifically, in the past the middle class has tended to support conservative parties, whereas the working class has preferred the Left parties; but in the future the youth from middle-class families with postbourgeois values may support Left or New Left parties, while individuals from working-class families who have only recently acquired a taste for prosperity may become potential recruits for conservative parties because they will seek to defend or extend their recent gains.⁶

To test this theory of intergenerational change, Inglehart utilized empirical data drawn from Western European nations, namely, Great Britain, France, West Germany, Belgium, the Netherlands, and Italy. He found that since Britain had industrialized early, the relative size of the population with postbourgeois values was largest there, but also that other countries which had a higher economic growth rate than Britain in recent years, notably Italy and France, had experienced the largest amount of

¹ Ronald Inglehart, "The Silent Revolution in Europe: Intergenerational Change in Post-Industrial Societies," *American Political Science Review*, 65 (December, 1971), 991-1017.

² Inglehart, p. 991.

³ Inglehart, p. 991.

⁴ Inglehart, p. 991.

⁵ Inglehart, pp. 991-992.

⁶ Inglehart, p. 992.

intergenerational change in value priorities.⁷

The Japanese Case

One of the hazards of studying the political effects of industrialization is that historically it has been associated with Western nations. This raises the possibility that one's findings with respect to the consequences of industrialization and economic growth may be greatly influenced by Western history, society, and culture. Thus we need to introduce "controls" for historical, social, and cultural variables. One way to do this would be to look at an industrialized country that is not Western; Japan, which today is a leading industrialized power, is an obvious choice. One objection that could be raised is that Japan is not entirely free of Western influences. Undoubtedly Japan has become Westernized to a certain degree; nevertheless, it is also true that in value preferences and in interpersonal relations, the Japanese have managed to retain much of their own traditions. Most people would probably agree that an examination of the Japanese experience should afford us a better perspective in trying to understand what is going on in the nations of Western Europe.

Another advantage of looking at Japan is the availability of the kind of empirical data not easily found elsewhere. Professor Inglehart noted, for example, that he was studying changes in value priorities that have occurred over a long period of time, but that since relevant time-series data were not available he could not test his hypotheses directly.⁸ It so happens that the Institute of Mathematical Statistics, a government-supported but autonomous organization located in Tokyo, has conducted a series of national surveys bearing on national character at five-year intervals beginning in 1953, and repeated in 1958, 1963, and 1968.⁹ Thus we have access to a longitudinal

study that spans the period of economic growth, namely the late 1950s and the decade of the 1960s. So in a way we can test directly some of the effects of economic growth (the independent variable) on changes in value priorities in Japan.

Of course, the ideal situation would be to replicate the European study, but this is not feasible. So from the long list of questions that were asked in the national character surveys, a small number was chosen that appeared to get at some of the same value priorities that concerned Professor Inglehart. Particular attention has been paid to responses to questions that were asked in 1953 and in 1968 of respondents in two age groups, 20 to 24, and 50 to 54 years old. By so doing, two periods of time separated by fifteen years (during which economic growth took place), and two groups of individuals who were born 30 years or roughly one generation apart can be studied.

The basic assumption is that our samples should include sets of individuals with rather different life experiences. The 20- to 24-year-olds in the 1953 survey were born between 1929 and 1933. Therefore the assumption is that they must have gone through their early childhood during a period of economic depression, experienced early schooling in an atmo-

Table A. Sample Size of Age Groups in Four Surveys

Age Group	1953	1958	1963	1968
20-24	423	201 149	335	376
25-29	345	199 127	361	398
30-34	257	202 128	367	399
35-39	247	146 91	328	389
40-45	221	161 96	279	344
45-49	187	120 89	239	243
50-54	201	107 58	252	222
55-59	135	101 64	152	205
60-64	82	92 43	155	192
65-69	81	65 37	113	128
70-	63	55 38	117	137
Total	2,242	1,449 920	2,698	3,033

⁷ Inglehart, pp. 999-1000.

⁸ Inglehart, p. 993.

⁹ The national character survey data cited in this article were drawn from Tokei Suri Kenkyujo, *Nihonjin no Kokumin-sei* (Tokyo: 1961) and *Dai-2 Nihonjin no Kokumin-sei* (Tokyo, 1970). For the 1968 national survey a sample of 4,000 respondents was drawn. Metropolitan, urban, and rural areas were stratified in terms of population, region, and occupation. One election district was chosen from each of the areas, the probability of selection being proportionate to the size of the population. Respondents were chosen on a random basis from voter lists, with the size of the sample proportionate to the size of the population. From the 4,000 in the sample, 3,033 interviews were completed. The size of the sample for the four surveys according to age groups is given below. Two sets of figures are given for the 1958 survey. For Tables 1-5, use figures in the lower row; and for Tables 8-9 and 12-13 take the total of both rows.

sphere of increasing authoritarian and militaristic repression, suffered deprivation brought about by severe shortages of food and other necessities during the latter part of the war, and felt the dislocation—economic, social, and psychological—wrought by the trauma of defeat and subsequent military occupation by the victorious Allied Powers. This was a generation obviously buffeted by severe winds of change, and therefore one that might conceivably value economic security rather highly. Those in the 50 to 54-age group, by contrast, were born about 1900 and so went through the Depression and World War II as mature adults.

The 1968 sample, of course, focuses on individuals born between 1944 and 1948, and except for those born in 1944, it would contain the postwar generation. One presumes that in terms of early childhood experiences, they were probably not well off, because the early years of the postwar era were characterized by considerable economic dislocation; but it is also true that as they grew older, the economy also grew, so that by the time they reached their late teens there was a labor shortage and jobs were plentiful. The hypothesis is that this generation would be more likely to prefer postbourgeois values, while the older generation ought to be in favor of acquisitive values. Is the same kind of generation gap to be found in Japan as in Western Europe? I now turn to the Japanese data.

One of the questions in the national character surveys had to do with "goals in life." Respondents were given six choices and asked to pick one as their major life goal:

- (1) Work hard and get rich.
- (2) Study hard and make a name for yourself.
- (3) Don't think about money or fame; just lead a life that suits your own tastes.
- (4) Live each day as it comes, cheerfully and without worrying.
- (5) Resist all evils in the world, and live a pure and just life.
- (6) Never think of yourself; give everything in the service of society.

The first two questions in the list would appear to tap personal achievement values, while questions three and four are related to self-fulfillment and self-gratification. The final two, by contrast, point in the direction of "dogoodism." The responses from the four surveys are tabulated in Table 1:

What the table suggests is that during the fifteen years covered by the surveys, personal achievement values have remained stable, while the self-fulfillment and self-gratification priorities have risen. At the same time, lower value priorities are accorded to leading an exemplary life or working for the good of society.

We can get a better perspective on what is happening to these values by taking questions (1) and (3), which in my estimation roughly correspond to Inglehart's acquisitive and postbourgeois values, and breaking the data down by age groups. Table 2 shows changes in the value of getting rich over the 15-year period:

As Table 2 shows, the 20 to 24-age group has not become less acquisitive over the years, which is to say that affluence does not appear to have affected them. If this age group is compared with the older generation, that is, those in the 50 to 54-age group, it is clear that the older generation is somewhat more acquisitive and that the gap existed as early as 1953, well before the period of rapid economic growth.

Table 2. Age and Percentage Assigning Highest Value to Working Hard to Get Rich, 1953-68

Age	1953	1958	1963	1968
20-24	9%	12%	12%	13%
25-29	12	13	16	14
30-34	11	24	21	19
35-39	15	20	18	21
40-44	19	24	19	16
45-49	20	20	15	17
50-54	17	10	20	22
55-59	16	17	24	18
60-64	15	14	18	17
65-69	27	22	13	18
70-	27	10	13	11
Average	15	17	17	17

Table 1. Goals in Life, 1953-68

Year	Get rich	Fame	Suit one's tastes	Live cheerfully	Live pure	Work for society	Other; DK	Total	(N)
1953	15%	6%	21%	11%	29%	10%	8%	100%	(2,254)
1958	17	3	27	18	23	6	6	100	(920)
1963	17	4	30	19	18	6	6	100	(2,698)
1968	17	3	32	20	17	6	5	100	(3,033)

Table 3. Cohort Analysis of 20-24 & 50-54-Age Groups on Value of Getting Rich

Age Group	1953	1958	1963	1968
20-24 (50-54)	9% (17%)			
25-29 (55-59)		13% (17%)		
30-34 (60-64)			21% (18%)	
35-39 (65-69)				21% (18%)

Actually what may be operating here is life-cycle change. Youth still unburdened by family obligations feel that money is less important, but as they grow older they become more acquisitive out of necessity. Indeed cohort analysis of the data suggests this. In Table 3 are tabulated responses in successive surveys from the same age groups over the 15-year span.

The table shows that 9 per cent of the sample of 20- to 24-year olds in 1953 responded affirmatively to the question of working hard to make money, but that 15 years later the figure from that same age group had risen 12 percentage points. By contrast, the older generation stayed virtually the same.

A rather different profile emerges from question (3), which rejects seeking money or fame and favors instead leading a life that suits one's own tastes. The responses are tabulated in Table 4:

Table 4. Age and Percentage Assigning Highest Value to Leading a Life that Suits One's Tastes, 1953-68

Age	1953	1958	1963	1968
20-24	34%	38%	45%	51%
25-29	23	34	38	42
30-34	29	36	33	38
35-39	21	22	31	31
40-44	15	23	30	37
45-49	14	21	23	28
50-54	14	21	22	23
55-59	12	19	25	19
60-64	9	19	20	19
65-69	10	3	17	16
70-	5	10	12	11
Average	21	27	30	32

Again there is a generation gap, for as early as 1953, the 20- to 24-age group chose this value more frequently than the 50- to 54-year-old generation. But it is also evident that the proportion of youth responding affirmatively has risen with every survey, from 34 per cent in 1953 to 51 per cent in 1968. This suggests the possibility that long-term intergenerational change has been occurring. This interpretation appears to be confirmed by a cohort analysis of changes in the younger and older generations summarized in Table 5:

As will be noted, the percentage of those responding affirmatively has not changed very much among these age groups over the 15-year period. That is, the change that has occurred is mostly accounted for by each new age group's acquiring somewhat different value priorities. Thus, as the older generation dies off, there is a progressive shift in values in the direction, in this case, of self-fulfillment.

Another way of looking at acquisitive vs. postbourgeois values is provided by a question that states: "In bringing up children of primary school age, some people think that one should teach them that money is the most important thing. Do you agree or disagree?" The responses obtained in three surveys across all age groups is presented in Table 6:

In terms of the overall average, the majority of respondents agree that children ought to be taught that money is most important. There is some difference, however, between the 20 to 24 and the 50 to 54-age groups. The older generation is somewhat more acquisitive and has been as far back as 1953. But the percentage of respondents who agree with the question has

Table 5. Cohort Analysis of 20-24 & 50-54-Age Groups on Value of Leading a Life that Suits One's Tastes, 1953-68

Age Group	1953	1958	1963	1968
20-24 (50-54)	34% (14%)			
25-29 (55-59)		34% (19%)		
30-34 (60-64)			33% (20%)	
35-39 (65-69)				38% (16%)

Table 6. Age and Percentage Assigning Highest Value to Teaching Children that Money is Most Important, 1953-68

Age	Agree			Disagree		
	1953	1963	1968	1953	1963	1968
20-24	60%	43%	34%	31%	36%	50%
25-29	66	55	49	28	26	36
30-34	63	58	58	22	23	27
35-39	62	56	59	21	23	26
40-44	65	63	59	27	19	26
45-49	66	62	62	21	22	24
50-54	72	68	65	18	20	18
55-59	72	72	67	19	16	19
60-64	77	66	66	15	23	20
65-69	78	72	73	12	15	14
70-	78	66	60	11	20	14
Average	65	60	57	24	23	28

Table 7. Cohort Analysis of 20-24 & 50-54-Age Groups on Teaching Children that Money is Most Important, 1953-68

Age Group	1953	Respondents Agreeing 1963	1968
20-24 (50-54)	60% (72%)		
30-34 (60-64)		58% (66%)	
35-39 (65-69)			59% (73%)

been declining somewhat over the years (from 65 per cent in 1953 to 57 per cent in 1968); and the change is especially marked among the young group, which suggests that intergenerational change may also be at work here. Again cohort analysis of these two age groups confirms the impression, for as Table 7 shows, samples from the same age group over a period of time show almost no change.

What about the espousal of postbourgeois

values, which, according to Professor Inglehart, characterizes youth in Western Europe? The Inglehart survey tried to measure postbourgeois values by scoring responses to two items, namely, "Giving the people more say in important political decisions," and "Protecting freedom of speech." Unfortunately, the Japanese national character surveys do not contain questions that get at exactly the same value dimension. Two questions, however, at least point in

Table 8. Age and Willingness to "Leave Everything up to" Political Leaders, 1953-68

Age	Agree				Disagree			
	1953	1958	1963	1968	1953	1958	1963	1968
20-24	30%	26%	22%	22%	54%	60%	58%	63%
25-29	33	31	24	25	48	50	54	58
30-34	41	32	24	30	46	51	52	52
35-39	44	35	27	26	38	45	46	54
40-44	52	34	24	25	28	46	51	57
45-49	51	31	29	34	29	46	50	49
50-54	52	46	33	29	26	34	40	50
55-59	51	42	36	34	28	27	40	45
60-64	51	37	47	43	19	29	30	35
65-69	57	50	35	39	15	18	34	33
70-	70	43	45	46	6	19	30	27
Average	43	35	29	30	38	44	47	51

Table 9. Cohort Analysis of 20-24 & 50-54-Age Groups on "Leaving Everything up to" Political Leaders, 1953-68

Age Group	Respondents Agreeing			
	1953	1958	1963	1968
20-24 (50-54)	30% (52%)			
25-29 (55-59)		31% (42%)		
30-34 (60-64)			24% (47%)	
35-39 (65-69)				26% (39%)

the same direction. The first one has to do with who should make political decisions. The question reads: "Some people say that if we get good political leaders, the best way to improve the country is for the people to leave everything up to them, rather than for the people to discuss things among themselves. Do you agree or disagree? Table 8 summarizes the replies:

The data show that the younger generation is much more reluctant than their elders "to leave everything up" to their political leaders. Again the gap is evident as early as 1953, but interestingly enough, it has narrowed over the years. There was a 22-percentage point difference in 1953, but only 7 percentage points in 1968 between the 20- to 24-year olds and the 50- to 54-year olds. What appears to be happening here is neither intergenerational nor life-cycle change, but rather adult change. This is a shift in attitudes in a certain direction among the adult population over a period of time. This is borne out by Table 9, which presents a cohort analysis of the two generations:

As Table 9 shows, the percentage of those among both the younger and older generations who agree to leave everything up to political leaders has in general declined somewhat.

The second question which would appear to be related to postbourgeois values has to do with the socialization of the young. "In raising

children of elementary school age, do you think it is more important to teach them to value freedom or discipline?" Table 10 summarizes the results:

Since this question was asked only in 1953 and again in 1968, we cannot follow the change (if any) that occurred in the intervening years. It is evident, however, that the majority of respondents, young and old, favor discipline over freedom. Japanese youth, in this sample at least, do not appear to be very rebellious. Still, proportionately more youth favor freedom than do older people, for in 1968, 34 per cent of the 20 to 24-year old group favored freedom in contrast to 17 per cent for the 50 to 54-year olds. Although there is not enough data to do cohort analysis, an examination of Table 10 suggests that this may represent a case of life-cycle change. For instance, 25 per cent of those in the 20- to 24-age group in 1953 favored freedom, but a sample from that same age group 15 years later revealed that only 16 per cent favored it. The opposite is true with those who favor discipline. In 1953, 56 per cent favored discipline, but by 1968 respondents in that particular group favored discipline even more strongly, registering a 73 per cent affirmative response. It appears that as people get older they favor freedom less and discipline more.

Table 10. Age and Percentage Favoring Freedom vs. Discipline in Raising Children, 1953, 1968

1968			1953		
Age Group	Discipline	Freedom	Age Group	Discipline	Freedom
20-24	57%	34%	20-24	56%	25%
25-29	63	27	25-29	64	17
30-34	68	21	30-39	60	14
35-39	73	16			
40-44	74	12	40-49	69	14
45-49	72	14			
50-54	73	17	50-59	66	11
55-59	75	15			
60-	66	15			
Average	68	20	Average	63	16

To summarize, Japan's youth, when compared to their elders, appear to be less acquisitive, more democratic, and somewhat more inclined to value freedom; in short, they show signs of preferring the kind of values defined by Inglehart as postbourgeois. Closer examination of the rather limited data suggests, however, that three kinds of value changes may be occurring: (1) intergenerational change, (2) lifecycle, and (3) adult. In the case of acquisitive values, there is a mixed picture. Responses to one question hint at intergenerational change, but another question suggests life-cycle. The same thing may be said of postbourgeois values. The question on living the kind of life that suits one's own tastes elicited responses that suggested intergenerational change; but the question on leaving everything up to political leaders implied that adults had been changing their values in a more democratic direction. In short, the view an observer gets of value changes in Japan is not so neat and tidy as the one Inglehart presents on Europe. I can only speculate about why this should be the case. More cross-currents may be operating in Japan because of the juxtaposition of tradition and modernity in Japanese society and culture. The greater complexity may also be an artifact of the kind of data available to the political analyst. If Professor Inglehart had had available to him the kind of longitudinal data I have used, he might have come out with a somewhat more complicated and therefore less tidy picture of the value change that had been going on in Europe.

Changes in Political Partisanship

What is the relationship between value changes and political partisanship? Inglehart believes, it will be recalled, that shifts in the social basis of parties may well occur in Western Europe during the next 20 years as a result of intergenerational change in values that has occurred and presumably will continue. Specifically, he suggests that "the presence of post-bourgeois values is linked consistently with a relative tendency to remain loyal to the Left, among those who were brought up in that tradition, and with a tendency to shift to the Left among those who were raised in other political climates."¹⁰ Unfortunately the data from the Japanese surveys do not allow this proposition to be tested directly in the Japanese setting.

Indirectly, however, some inferences can be made. If one assumes, as does Inglehart, that education is an accurate indicator of "parental affluence during the respondent's formative

Table 11. Education and Support for Socialist Party, 1968
Percentage Supporting Socialist Party

Year	Elementary & Middle School	College
1953	34%	33%
1958	53	36
1963	37	26
1968	40	22

years,"¹¹ then college-educated children, presumably having come from the more affluent families, ought to be the bearers of postbourgeois values. There is some data on the relationship between education and support for the Socialist party over the 15-year period.

Table 11 shows quite clearly that except for 1958, when the Socialists reached the peak of their popularity, support among those who went only to elementary or middle schools increased slightly. By contrast, support for the Socialist party among college-educated respondents has dropped (except for 1958). This would suggest that the situation in Japan is somewhat different from what Inglehart found in Western Europe.

Another way to look at partisanship is to relate it to age groups. In the past, age has been a good predictor of party support; that is, the young tend to prefer the Socialists and other Left parties, while the elders incline toward conservatism. Table 12 shows the relationship between age and party support:

As can be seen from Table 12, in 1958 there was a 26 per cent spread in support for the Socialists between the group of 20- to 24-year olds and the 50- to 54-year olds. The difference has narrowed in recent years, but the younger age groups generally remain more radical when compared to the older people. Another observation that may be made is that life-cycle change also appears to be present. This is consistent with the conventional wisdom that says that young people tend to be radical but become more conservative as they grow older. In 1953, 41 per cent of the sample of 20- to 24-year olds supported the Socialist party, but the same age group by 1968, now 35 to 39 years old, had dropped to 22 per cent. A third tendency that may be noted from Table 12 is the steady decline in support for the Socialists among youth in their early twenties during the 15-year span. This finding again is consistent with the argument that has been made by numerous journalists and observers that Japanese

¹⁰ Inglehart, p. 1013.

¹¹ Inglehart, p. 1005.

youth in recent years have become less radical. But in becoming less radical they have not necessarily become pro-conservative. Rather the evidence suggests that in recent years a sizable group of young people have withdrawn from active concern with the electoral process. Responses to questions about intention to vote in national elections are tabulated in Table 13:

A general decline in the commitment to vote in almost all age groups may be noted from the data in Table 13, but the decline is particularly steep among the younger age groups. Supporting evidence may be found in other surveys. Since voting is often regarded as a duty rather than as a right of every citizen, election commissions are actively engaged in trying to get as many people as possible to turn out on election day. It is not surprising, given this orientation, that the government should be interested in collecting data by means of surveys of who votes and who does not. According to government statistics, in 1952 about 15 per cent of those in their twenties, it is estimated, did not bother to vote in elections for the House of Representatives, but by 1969, the figure had risen to about 29 per cent, that is had almost doubled.¹² Moreover, in the 1968 national character survey, 31 per cent of the respondents in the 20- to 24-age group reported they supported no party (the highest of all age groups), and since this same group had the lowest percentage of individuals who intended to vote, the inference is that the fall-off in the commitment to vote must be related at least in part to an ebbing of partisan feelings.

There has been some speculation about why Japanese youth have become more apathetic in recent years. Lack of information probably can

be dismissed as a primary cause. Generally in public opinion surveys, the percentage of respondents in their twenties who say "Don't know" when asked about their views on international affairs and current political issues is lower than those in the older age groups, who are more likely to vote in elections. Perhaps this is to be expected, since the younger generation is usually better educated than their elders. Undoubtedly, part of the apathy is a reflection of negative sentiments toward the present political system. The prospects of changing the one-party dominant system in which the conservative Liberal Democratic party has held the reins of power for years are not bright, at least in the near future. Thus any person who seeks to use his vote to bring a new group of leaders to power understandably feels frustrated.¹³

It is also conceivable that the decline in electoral turnout reflects a rejection of the parliamentary system. There are some ultra-radical student organizations that nihilistically reject virtually all forms of authority. But the number of young men and women who belong to such organizations is very small and certainly unrepresentative of the great majority of Japanese youth.

It is doubtful that the inability to dislodge the conservatives or rejection of the parliamentary system represent major causes of the fall-off in electoral participation. I am inclined to believe that an important factor is the tendency toward individuation and privatization. A leading Japanese political scientist, Masao Maruyama, has argued that modernization in Japan has increased the desire for "individuation." By this he means that an individual has become "emancipated" from "communal ties which have hitherto bound him and which have pre-

¹² Aiba, Jun-ichi, "Seinen no Seiji Ishiki," *Shakaigaku Hyoron*, 22 (December, 1971), p. 17.

¹³ Aiba, p. 28.

Table 12. Age and Party Support

Age Group	Liberal Democratic				Socialist			
	1953	1958	1963	1968	1953	1958	1963	1968
20-24	30%	31%	27%	29%	41%	40%	32%	25%
25-29	42	37	38	34	28	36	30	25
30-34	32	38	38	35	27	40	27	28
35-39	31	44	40	42	25	24	24	22
40-44	34	41	51	43	21	30	17	25
45-49	39		45	48	18		23	19
50-54	37	44	48	52	15	22	20	17
55-59	37		55	48	8		11	18
60-64	39	37	57	47	7	14	9	15
65-69			50	56			9	14
70-			52	52			4	9
Average	41	38	43	41	23	31	22	22

Table 13. Age and Intention to Vote, 1953-68

Age Group	Will Vote			Will Try to Vote			Disinterested		
	1953	1963	1968	1953	1963	1968	1953	1963	1968
20-24	54%	39%	34%	40%	55%	58%	4%	6%	7%
25-29	59	47	40	36	48	53	5	4	6
30-34	63	55	41	34	40	53	3	5	6
35-39	62	51	56	34	41	42	4	7	2
40-44	62	60	57	32	35	38	5	5	5
45-49	69	53	60	28	42	37	2	4	2
50-54	66	57	57	26	35	39	7	6	3
55-59	71	59	60	21	38	35	8	1	5
60-64	68	66	59	26	26	33	4	4	6
65-69	63	57	67	26	35	28	10	7	4
70-	55	61	61	23	30	29	16	6	8
Average	62	53	51	32	41	44	5	5	5

scribed to him certain traditional behavior."¹⁴ Robert J. Lifton has described in his study of Japanese youth the concern young people have with the idea of *shutaisei*, which involves having and living by personal convictions, or selfhood, and joining with other like-minded people to achieve historical goals, that is having social commitment. "Their greatest difficulty," he continues, "is in realizing to their own satisfaction its first element, that of selfhood; and the sense of 'smallness' or the 'inferiority complex' which they talk so much about seems to reflect the great difficulty the Japanese have in perceiving and believing in a relatively independent self."¹⁵

One of the questions in the national character surveys cited earlier having to do with leading a life that suits one's own tastes (Table 4) may well be tapping this dimension, that is the search for selfhood, or individuation. It will be recalled that responses to this particular question indicated intergenerational change.

One of the several ways in which this individuation process can proceed is "privatization," which Maruyama defines as an orientation toward the "achievement of self-gratification rather than public goals" and a "withdrawal" or "retreatism" as a "conscious reaction against the increasing bureaucratization of the system and against the complexities of the social and political process in which he finds himself involved."¹⁶ There is some empirical evidence that privatization may be occur-

ring among sections of youth in Japan.

One of the questions in the 1968 national character survey sought to determine which had higher priority, individual rights or the public interest.

There are the following opinions, which one do you approve? Of course, it may depend on the situation and degree, but in general which one should be followed? (a) In order to give recognition to one's rights, it may be necessary that the public interest be sacrificed sometimes; (b) an individual's rights may have to be sacrificed sometimes in order to promote the public interest.

The responses are indicated in Table 14:

One might have hypothesized that young people, being more idealistic and less acquisitive, would favor promoting the public interest, but as Table 14 shows, this is not the case. If the 20- to 24-year old group is compared with the 50- to 54-year olds, there is a 17-percentage point difference, with youth favoring individual

Table 14. Age and Individual Rights vs. Public Interest, 1968

Age Group	Individual Rights	Public Interest	Other
20-24	46%	50%	4%
25-29	38	52	10
30-34	42	48	10
35-39	32	59	9
40-44	28	66	6
45-49	33	58	9
50-54	29	60	11
55-59	26	60	14
60-	19	62	19
Average	33	57	10

¹⁴ Masao Maruyama, "Patterns of Individuation and the Case of Japan: A Conceptual Scheme," in *Changing Japanese Attitudes toward Modernization*, ed. Marius B. Jansen (Princeton: Princeton University Press, 1965), p. 494.

¹⁵ Lifton, Robert J., "Youth and History," *Asian Cultural Studies*, No. 3 (October, 1962), p. 125.

¹⁶ Maruyama, p. 498.

rights over the public interest. The older generation was socialized in the prewar period when both the educational system and the official propaganda emphasized the duty of every person to subordinate his welfare to the good of the collectivity, whether it was the family, society, or the state. The younger generation, raised in a more democratic political atmosphere and educated in a school system that no longer emphasizes service to the state, appears to put higher priority on personal welfare and self-gratification.

In the absence of more detailed data, one can only speculate about why some Japanese youth should favor privatization. It is certain that economic pressures on youth lessened considerably in the 1960s, thanks to the high economic growth rate. The business boom, fed by rising exports as well as domestic consumption, led to persistent labor shortages. Jobs became more plentiful, and although prices were pushed upward by the inflation, wage scales rose even faster. All of this enabled Japanese youth to indulge themselves and to engage in the pursuit of pleasure and self-gratification. By contrast, the older generation, still bound by the traditional ethic that stressed individual sacrifice for the collectivity, continued to take its social responsibilities more seriously.

Conclusion

Since the available data are rather sketchy and not sharply focused on the problem at hand, one must be circumspect about drawing sweeping conclusions from them. It can be said, however, that different kinds of change—intergenerational, life-cycle, and adult—appear to be at work and that the general picture is less clearcut than the one Inglehart paints for Western Europe. I am inclined to believe that in Japan there is less a shift from acquisitive to postbourgeois values than a long-term change from a collectivity orientation to individuation. Indeed, the most parsimonious explanation for much of the data would be the trend toward individuation. For example, a shift away from acquisitive values may represent a weakening of the family as a cohesive unit. In the past the pressure for individual achievement usually came from parents who were eager that their children achieve fame and fortune because, among other things, this would help elevate the social standing of the whole family. I do not wish to imply that this is no longer the case, but it is true that family cohesion is weakening in the face of modernization. Likewise, the de-

cline in willingness to take the trouble to vote in national elections may be taken as a sign that community pressures are abating. Much of the time, voting may represent more an act of group conformity than an expression of one's political convictions. Finally, privatization, as has been suggested, may represent a form of individuation.

In any case, the Japanese data would seem to cast a shadow of doubt on the theory of a hierarchy of values which Professor Inglehart invokes to explain his thesis. Following Maslow, he argues that man's first goal is to meet his physiological needs and when he has attained them, he turns to social needs, followed by needs for self-esteem. Some critics have questioned the usefulness of this formulation in explaining human behavior and have also asked whether we are to "assume that the needs operate in all cultures and situations, or only in some."¹⁷ It is my contention that in Western culture, which has long stressed individualism, youth may seek a sense of belonging, whereas in Japanese culture, which has emphasized the group, youth may yearn for individuation and privatization.

What implications all this has for the future of Japanese politics is unclear. There are still no clear signs that new parties may emerge or that a realignment of the social basis of old parties may take place to reflect new cleavages in value priorities. If anything, many young people appear to be turning away from politics. This may be a short-run phenomenon, and a period of more active involvement in politics may yet appear, just as the political apathy found in many American campuses in the 1950s was replaced by the turmoil of the 1960s. At the very least, the direction of change in party loyalties becomes difficult to predict. There is now an element of uncertainty, portending that the political stability and incremental change characteristic of postwar politics in Japan may in time come to an end.

Finally, the Japanese data here analyzed calls into question the validity of generalizations about the political consequences of industrialization based solely on data drawn from Western nations. The rise of Japan to a status of a leading industrial power means that no one interested in the problem of the effects of industrialization and economic growth on politics can afford to ignore the Japanese experience.

¹⁷ Silverman, David, *The Theory of Organizations: A Sociological Framework* (London: Heinemann, 1970). pp. 81-82.

Issue Area and Foreign-Policy Process: A Research Note in Search of a General Theory

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Is there a foreign-policy process? How does foreign policy making differ from policy making in the domestic domain? What is the impact of process on policy outcome? This note attempts to further our efforts to answer these questions by suggesting a general paradigm for the study of the foreign-policy process. The paradigm is explicitly synthetic; it combines insights generated by an early contribution of Arnold Wolfers to our understanding of the linkage between foreign-policy behavior and domestic political process with those prompted by two recent efforts by Theodore Lowi to affirm the nexus between issue and policy process. The paradigm, it is argued, makes it possible to specify the nature of the policy process (the identity of the major actors, the intensity of conflict) which will obtain in reasonably integrated modern states for a given case in point. It permits us to specify the social science literatures which are most germane to the generation of insights pertaining to the nature of the policy outcome. Finally, it gives us a framework within which it will be more possible to answer the old question about case studies: "Of what is this a case?"

The Pole of Power and the Pole of Indifference

Wolfers's essay¹ is largely devoted to ascertaining when in the name of analytic parsimony is it justifiable to disregard the impact of the internal workings of the state on foreign policy behavior. When, in other words, may one confidently expect that "black boxing" the state will serve the scientific goal of prediction? His answer is based on several propositions. It depends, he asserts, on whether the decision makers acting in the name of the state attach primary loyalty to the state and on whether, similarly, states are the sole corporate actors in the international environment.

¹ Arnold Wolfers, *Discord and Collaboration: Essays on International Politics* (Baltimore: The Johns Hopkins Press, 1962), pp. 3-35. Harold K. Jacobson reminds me that Wolfers was frequently less disposed than in "The Actors . . ." to accord predictive power to rational models, even at the pole of power—hence his emphasis on "National Security as an Ambiguous Symbol," *Discord and Collaboration: Essays on International Politics*, pp. 147-65.

The most relevant aspect of Wolfers's argument for the concerns of this article is his implication that a crucial determinant of the policy process is the nature of the issue at stake. He develops the relation of issue to policy process by asserting the existence of two poles—the pole of power and the pole of indifference—which may be conceived as end points of a continuum measuring the hierarchy of values of participants in the political process. With regard to the pole of power, Wolfers's analogy to individual behavior when someone yells "fire in the hall" should be familiar to all students of foreign policy. The preservation of the state is a goal highly esteemed by almost all decision makers. Consequently, a direct, profound threat to the existence of the state is in most instances likely to result in power maximizing behavior of the kind anticipated by those in the states-as-sole-actors tradition. What is frequently forgotten is Wolfers's suggestion that it is equally plausible to assume a predictable result in situations where "danger and compulsion are at a minimum. When not more than minor values are threatened by international discord, governments usually find it expedient to act according to established rule, since their interest in seeking others to do likewise exceeds their interest in winning an occasional and minor advantage"²—and hence one expects a high incidence of compliance with diplomatic protocol even by new states which have no part in establishing the rules. Thus, according to Wolfers, when events approximate either end of the continuum it is appropriate to expect that unitary, rational decision-maker models of state behavior will provide a close fit with reality. Under such circumstances, analysis of policy process or the perceptions of the decision makers would be reserved for the occasional deviant case.

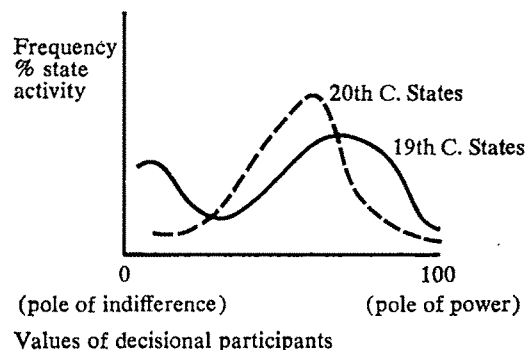
Wolfers's analysis concedes too much to those who disregard the internal workings of the state or the minds of those who act in its name. It is by no means clear that in the atomic age the response of all decision makers to the functional equivalent of fire in the hall will be similar and highly predictable. Wolfers observes that "in international politics, the house

² Wolfers, *Discord*, p. 16.

is not always, nor everywhere, on fire although the temperature may not be comfortable even under the best of circumstances."³ What he fails to note is that in international politics, even when the house is on fire, staying within the house may be preferable to going outside. The divergent exhortations "Better red than dead" and "Better dead than red" symbolize the extremities among a wide range of alternatives which *might* be adopted by U.S. decision makers when core values are threatened. The range of responses to the Soviet missiles in Cuba in 1962 actually advocated by members of Kennedy's entourage lends further support to stressing the severely limited predictive power of models which disregard microanalytic phenomena, even under crisis conditions, when applied to superpower behavior.⁴ The response of lesser powers to armed intervention by a superpower is likewise much affected by micro-analytic variables: The propensity to fight if attacked—as the Yugoslavs since 1968 have repeatedly told themselves and others—appears greatly dependent on national culture and level of development. Even in crisis situations one disregards the perceptions of decision makers—and the elements which shape perceptions—at one's peril. We have progressed too far in our awareness of the impact of organizational structure and small-group dynamics on policy outputs not to include these in assessments of policy outcomes.

These caveats notwithstanding, Wolfers's argument is highly suggestive. It intimates an important explanation for the durability of the "billiard ball" model of international politics. Autocracies and republics alike in the nineteenth century were much more restricted in the scope of their activities than are their twentieth-century counterparts. Given the norm of *laissez faire*, the bulk of state actions in international affairs was concentrated precisely on the tasks of protection and the facilitating of interaction between states (the exchange of information and diplomats) and, in the mid-nineteenth century, regularizing the exchange of mail and telegraphic dispatches between individuals of various states. In terms of Wolfers's continuum, nineteenth-century state activity in international affairs was bimodally distributed. By contrast, twentieth-century liberal democracies and modern dictatorships alike extended the scope of their activities. For twentieth-century states the preoccupations of decision makers have been more normally distributed along

a continuum reflecting the values of the relevant internal actors. A reasonable graphic aggregation of such states' behavior might look like a normal curve skewed somewhat toward the pole of power (Figure 1). For events located at the poles of power and indifference, the analysis of domestic politics *as such politics is commonly understood* may be largely disregarded. (The qualification is an important one; I am not suggesting that, for instance, politics is absent in crisis decision making—only that the participants in the process act as individuals rather than as spokesmen for groups, institutions, or major strata of society.) For these events it suggests a rationale—aside from the



Distribution of State Activity

Figure 1.

presumably asymmetrical impact of the external environment—for the study of foreign policy separate from the study of "domestic" political processes. A corollary, furthermore, is that there exists an enormous range of events between the two poles for which domestic politics as normally understood is immensely relevant. In the large majority of "foreign policy" cases the analysis of policy process is of major concern.

Arenas of Power

It is the existence of policy processes which alter as a function of issue area⁵ ("arena of

³ Wolfers, p. 15.

⁴ Graham T. Allison, *Essence of Decision: Explaining the Cuban Missile Crisis* (Boston: Little, Brown, 1971).

⁵ Other contributions to the "issue area" literature include several community power studies—Robert A. Dahl, *Who Governs: Democracy and Power in an American City* (New Haven: Yale University Press, 1961); Nelson W. Polsby, *Community Power and Political Theory* (New Haven: Yale University Press, 1963); Aaron Wildavsky, "The Analysis of Issue Contexts in the Study of Decision-Making," *Journal of Politics*, 24, (November, 1962), 717–32; and James N. Rosenau, "Pre-theories and Theories of Foreign Policy," in *Approaches to Comparative and International Politics*, ed. R. Barry Farrell (Evanston: Northwestern Univ.

power" in Lowi's terminology) which constitutes Lowi's fundamental insight.⁶ The argument of each article may be summarized briefly. In "American Business, Public Policy, Case-Studies, and Political Theory," Lowi identifies three major arenas of power which, he contends, constitute the major patterns of politics in the United States and, in an aside, excludes foreign policy from his analytical scheme.⁷ The three issue-areas he labels the arenas of distribution, regulation, and redistribution. What separates one issue area from another is the extent and manner in which the political goods in a case in point can be disaggregated. Those domestic issues which are highly subject to disaggregation Lowi labels the politics of distribution. "Pork barrel" items typify this issue area; the political goods are readily subject to being parceled out to the major units. Nearly everyone (read, everyone who counts politically) gets—in the contemporary argot—a piece of the action.

The arena of regulation, by contrast, is the domicile of the group theorists. Coalitions of like-minded groups represent the major patterns of alliance as the classic question of who gets what is decided. In such instances the po-

litical goods are not as subject to disaggregation as they are in distributive politics. Where it is often difficult in the distributive arena to identify specific losers, there are always losers in the regulatory arena. "In the short run the regulatory decision involves a direct choice as to who will be indulged and who deprived."⁸ Some will not obtain a liquor license in Ann Arbor, just as some will not obtain an overseas air route.

If the arena of regulation is one in which the models of pluralist theorists provide a close approximation to reality, redistributive politics are the domain of elitists such as C. Wright Mills. The actors neither are groups nor are they the firms or individuals typical of distributive politics. Rather, the impact of the event is felt by far larger aggregates—social classes, "peak associations," movements. Since, moreover, the (perceived) relative status and power position of such aggregates is thought to be at stake—the political goods involved being both tangible and nontangible—disaggregation is extremely limited.

In "Making Democracy Safe for the World," Lowi's focus and argument shifts somewhat. Whereas in "American Business, Public Policy . . .," Lowi dismisses foreign policy in a footnote, in "Making Democracy Safe for the World," his central concern is with the foreign-policy process. His major conclusion is that there is no substance to the view that "foreign policy-making is a fundamentally different policy-making system, or one that is in any considerable way insulated from domestic political

Press, 1966) pp. 27-92 and "Foreign Policy as an Issue-Area," in *Domestic Sources of Policy*, ed. James N. Rosenau (New York: The Free Press, 1967) pp. 11-51.

⁶ Theodore J. Lowi, "American Business, Public Policy, Case Studies and Political Theory," *World Politics*, 16 (July, 1964), 677-715; "Making Democracy Safe for the World: National Politics," in Rosenau, *Domestic Sources*, pp. 295-331.

⁷ Lowi, "American Business . . ." p. 689, n. 17; "Foreign policy, for which no appropriate '-ion' word has been found, is obviously a fourth category."

⁸ Lowi, pp. 690-91.

Lowi's Arenas of Power: A Summary

Chart I

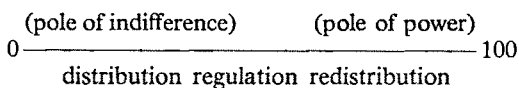
Arena	Primary Political Units	Patterns of Interaction	Power Structure	Foreign Policy/ Domestic Policy
I. distribution	individual, firm	log rolling; "unprincipled" alliances of uncommon interests	nonconflicting elite with support groups	both
II. regulation	group	"the coalition;" shared subject matter interest aggregation	pluralistic, multi-centered	both
III. a. protection b. interaction	individuals "without their institutions"	"team norms"	highly elitist	foreign policy
IV. redistribution	classes, movements generations	"peak associations"	conflictual elite: elite and counter elite	domestic

(Borrows heavily from Lowi, "American Business . . . , p. 713.)

forces."⁹ Instead, he again finds that "three predictable subsystems" may be identified. For foreign policy they are: (1) "an elitist subsystem (although involving a much smaller elite than the 'power elite') that prevails (a) in crisis and (b) in any noncrisis situations in which in the short run no internal resources are involved at all;"¹⁰ (2) "a logrolling pattern . . . similar to . . . [domestic] distributive politics;"¹¹ and (3) a pluralistic regulatory subsystem in which conflicting groups are the main actors. This time it is redistribution which is relegated to an aside.¹² From the two articles, thus, four separate policy processes are identified. The two Lowi articles are summarized in Chart 1. Each, Lowi concludes, has modal actors and patterns of interaction. Two, distribution and regulation, are useful for both the study of domestic and foreign policy. One—arena III—presumably is unique to foreign policy while the fourth, redistribution, is restricted to the domestic domain.

An Issue-based Foreign Policy Paradigm

One element which serves to link the Wolfers and Lowi pieces is immediately evident. Apparently entirely independent of Wolfers, Lowi has "discovered" the poles of power and indifference. Like Wolfers, Lowi concludes that for events at these two poles, the policy process does not involve the nongovernmental, societal actors, or patterns of interest articulation and aggregation typically found in "domestic" politics. Beyond that, there is a clear ordering in the stakes for which politics is played in the arenas of distribution, regulation, and redistribution—an ordering which relates to Wolfers's continuum. For if the poles of power and indifference are the extremities of a continuum measuring the values of decisional participants, it follows that the arenas of distribution, regulation, and redistribution may be located in ascending order on such a continuum:



and the policy process for issues which may be properly subsumed under the regulatory interval along Wolfers' continuum would predict-

⁹ Lowi, "Making Democracy Safe . . .," p. 314.

¹⁰ *Ibid.*, p. 324.

¹¹ *Ibid.*

¹² *Ibid.*, p. 325, n. 61: ". . . it is interesting to note the absence of a foreign-policy variant of redistribution. This might be due to my incomplete coverage rather than to a datum about the system."

ably correspond to the conflictual, pluralistic group politics which is the mode of the regulatory arena.

The issue, of course, then becomes by what criteria shall the intensity of the stakes or values at issue be measured. Again the linkage between Wolfers's and Lowi's analysis stimulates an answer: If, in terms of process, the politics of protection and interaction are similar and if the poles of power and indifference do represent the ends of a continuum, then it ought theoretically to be possible to treat that continuum as circular and to identify and label a matrix in which the poles of power and indifference may be appropriately located in the same cell.

Lowi's assertion is that the reason for the similarity between the policy process of crisis politics and that of such foreign-policy acts as recognition is that both have no short-run domestic political consequences.¹³ The assertion that crisis politics does not have domestic political consequences seems, however, sufficiently bizarre—in the era of the fifteen-minute war the consequences may be felt in the very short run—to warrant an alternative hypothesis.

The kind of "apolitical" consensual politics associated with protection-insuring and interaction-facilitating policies occurs when the dominant perception of all involved is that the impact of the decision will be *symmetrical* in its impact on the values of citizens. If most citizens are affected in the same way, regardless of the substance of the decision, instances of protection and interaction are directly analogous to distributive politics. In all three instances, decisions have a symmetrical impact domestically. Either the decision has no perceived impact, or there is a threat to the core values of the society, or all the relevant political actors are indulged. Under both regulation and redistribution, by contrast, there are those who are indulged and those denied. An event will be either regulative or redistributive if in the short run its effects are nonsymmetrical in impact.

At the same time, politics in the arena of interaction and protection shares something in common with politics in the arena of redistribution: all these issues involve values which are not confined entirely to tangible resource allocations matters alone. Rather, in the case of all three, nontangible issues are clearly at stake. Interaction issues involve no domestic resource allocation questions. Events properly classed within the arena of protection by their very nature pertain to the core values of the society. Redistributive issues, as we have seen, entail the reallocation not only of tangible goods be-

¹³ *Ibid.*, p. 324.

tween classes and other large elements in society but also such nontangible and highly important values as the redistribution of status and power in society. Each contrasts with the arenas of distribution and regulation—the special preserves of resource allocation issues.

The answers, then, to two questions serve as the basis for a paradigm of foreign-policy processes. These questions are: (a) Is a decision perceived to be symmetrical in its impact on politically relevant domestic political actors? (b) Are the political goods at issue exclusively tangible? (Figure 2). By ascertaining answers to these questions, one should be able to predict the nature of the policy process and to stipulate the social science literature most likely to facilitate the generation of insights relevant to the prediction of outcomes. An affirmative answer to both questions predicts that the issue will be distributive. As such, the primary political units, and the patterns of interaction and power structure are those predicted for the arena of distribution (Type I policy process), and the theoretical literature recommended is largely that of E. E. Schattschneider.¹⁴

A negative answer to question (a), i.e., the decision is perceived to be asymmetrical, coupled with an affirmative answer to (b), similarly classifies the issue as regulatory (Type II) and informs us that this is an occasion when the usual analysis of domestic politics by the group theorists will be entirely appropriate for a "foreign policy" issue.

By contrast, a positive response to (a) and a negative answer to (b) informs us that rational

¹⁴ *Politics, Pressures, and the Tariff* (New York: Prentice-Hall, 1935) and Lowi's summary of Schattschneider in "American Business . . .," pp. 680–682.

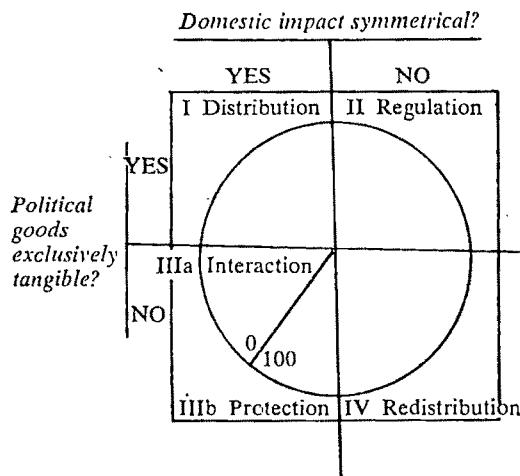


Figure 2. An issue-based foreign policy paradigm.

policy models augmented by a grounding in social psychology—specifically, literature treating the behavior of small groups, information processing by individuals and organizations (e.g., cognitive dissonance), deviant behavior—and a thorough awareness of the ubiquitousness of high politics will be relevant to the prediction of outputs. Similarly, it permits one to assert with some confidence that such traditional societal inputs into the policy process as interest articulation may be dismissed as effectively irrelevant to the outputs for such Type III issues.

Finally, should the answers to questions (a) and (b) both be negative, one would properly conclude that foreign-policy issues may indeed be redistributive and thus to assert that elite theorists and specialists on internal conflict¹⁵ constitute relevant authoritative sources for inspiration.

The Unlimited Politics of Limited War

It is to the fourth type of policy process that we now turn. The advantages of the scheme set out here will be modest if the fourth type of policy process has no empirical-world referents. If the fourth cell is virtually devoid of entries, then the only gain over Lowi's categorization would derive from the way in which the questions underlying the paradigm facilitate the task of distinguishing between distributive, regulative, and protective/interactive policy processes.

It can, however, be shown that certain important foreign-policy acts are redistributive and do manifest a policy process which one predicts for the redistributive arena. One major class of such redistributive events is limited wars. Strictly speaking, not all limited wars are redistributive. The war may be so brief or otherwise limited that no opportunities would be foregone domestically. Those involved in actual fighting would be limited to professional military personnel—whose promotional opportunities are accelerated by a war (if they survive)—and the resources utilized would be drawn exclusively from those procured before the outset of the war. A limited war may at the outset widely be perceived as the initial phase of a general war¹⁶ or as part of a general security strategy. While these perceptions hold, the policy process will be consensual-elitist rather than

¹⁵ Harry Eckstein, ed., *Limited War* (New York: The Free Press, 1964); Lewis A. Coser, *The Functions of Social Conflict* (Glencoe: The Free Press, 1956).

¹⁶ Kenneth N. Waltz, "Electoral Punishment and Foreign Policy Crises," in Rosenau, *Domestic Sources . . .*, p. 279, points out that in July, 1950 "just over half" the Americans polled thought the United States was "actually in World War III."

conflictual-elitist (Type III, rather than Type IV). Nevertheless, in practice limited wars have been searing events in American history. Knowing the history of the War of 1812, the War with Mexico, the Spanish-American War,¹⁷ or the Korean War,¹⁸ one would hardly be surprised to learn that the war in Vietnam has engendered high domestic conflict or to be informed that the politics of the war have at times resembled those of internal war. Viewed against a paradigm of policy processes—rather than, for instance, against a conception of foreign policy conducted by a rational, unitary decision maker—the nearly unlimited politics of limited war becomes readily understandable and in fact predictable. While the values at stake are less central to the core values of the society than those in genuine crisis situations, they extend far beyond those at issue in resource allocation questions. (Indeed, one ploy which the ruling group may adopt in order to mute conflict is to escalate the war to a point where general war may become a genuine possibility—in effect attempting to change the issue from a Type IV to a Type III policy process.) Limited wars are distinctively asymmetrical in their domestic political consequences. In discussing redistribution as a policy process relevant for certain internal events, Lowi observes, following Wallace Sayre, that the relevant cleavage is between those who provide the money and those who demand the services. What makes limited war even more acutely conflictual than most domestic redistributive issues is that the cleavage is not between money-providing and service-demanding segments of society but rather between those who demand the money and those who provide the services.

All four cells thus seem to have predictive implications for the analysis of the American foreign policy process. When an event is generally perceived as being distributive we can state with some confidence on the basis of previous case studies how the policy process is going to operate and which kinds of literature about politics are most likely to be productive of insights. In the case of crisis decision making, similarly, the highly elitist pattern involving the

legitimate decision maker and his coterie of advisers acting “without their institutions” characteristic of Type III protective interactive policy processes will be observed. An event like a limited war with a dramatically asymmetrical impact on society and involving intense (although not core) nontangible as well as tangible values soon will engender class warfare, generational confrontation, acute sectional strife, or other highly conflictual politics.

Directions for Future Research

Even without the research urgently needed to test the range of applicability of this paradigm, we can at least address ourselves tentatively to the range of states for which the paradigm has intuitive relevance. We have already sketched certain limits. One obvious qualification is that the state must have achieved minimal social and political consensus. In the absence of such consensus, a direct threat to the existence of the state will not be perceived as a threat to core values. A second qualification concerns the relationship of the regime to society. For our purposes, modern totalitarianism may be conceived as an effort by the regime artificially to re-create the qualities of the absolute monarch's state by insulating itself from the forces operative in “normal” complex industrial states (as well as by rendering itself impermeable to outside influence). If the regime succeeds, all policy disputes will resemble in their *process* those associated with the protective-interactive arena (Type III).

Within these clear limits, the paradigm may prove of some predictive value. Indeed, the task of comparison may be well served if we hold issue area constant and depict the manner by which, within a given issue area, differences in political structure and political culture affect the general contours of the policy process. For most developed, pluralist states, the paradigm certainly seems plausible.

Recent work by Roland Pennock, for instance, while not germane to foreign policy, suggests that what we have called the distributive arena is a major feature of British politics. Pennock attacks the conventional notion that the pork-barrel, log-rolling politics typical of the distributive arena are characteristically American. “It is true,” Pennock grants, “that what has come to be defined as the pork-barrel phenomenon does not exist in Great Britain.” Nevertheless, he asserts,

If we look at the use of public funds for the benefit of identifiable groups, farmers, old age pensioners, recipients of general public assistance, manufacturers, and the like, something bearing a distinct family resemblance quickly appears. . . . Legally

¹⁷ The literature on each is extensive. Recent capsule depictions of the internal politics of the three are to be found in Samuel E. Morison, Frederick Merk and Frank Freidel, *Disseint in Three American Wars* (Cambridge: Harvard University Press, 1970).

¹⁸ John E. Mueller, “Trends in Popular Support for the Wars in Korea and Vietnam,” *American Political Science Review*, 65, (June, 1971), 358–75 shows the striking similarity in mass attitudes to the wars in Korea and Vietnam. He concludes that popular support decreases logarithmically with the increase in American casualties.

established minimum wages for agricultural labor, tariffs, and other benefits to manufactures, constitute in a larger sense a "package deal" that is not sharply dissimilar from the American "pork barrel." It is implicit log rolling, or vote trading, and it apportions benefits to special groups, the cost of which is spread throughout the taxpaying or, as the case may be, consuming public.¹⁹

My own research has indicated that in Yugoslavia a procedure for disaggregating resources to politically relevant units at the expense of the general interest is well established. In the Yugoslav context, those which count politically are republic party organizations and, frequently, large republic-specific enterprises. Evidencing only minimal national integration, Yugoslavia is a country whose political elite is fearful that tangible resource allocation questions, via the catalyst of republic-level nationalism, will be transformed into a redistributive issue. Thus, Yugoslavia displays an unusual propensity to disaggregate resource allocation decisions. For students of the American political process, the result is predictable. Even though for Yugoslavia to compete internationally economic rationality would prescribe an intensive strategy, the political process ordains a strategy of putting a few eggs in several baskets. A railroad terminating in the Croatian city of Rijeka is followed by its counterpart through Serbia to Bar. Ship building and harbor modernization funds are parceled out accordingly, each republic with an Adriatic outlet being indulged. Even major foreign service appointments are parceled out; insufficient "ticket-balancing" may result in the vetoing of an ambassadorial appointment, for instance, by a *republic* assembly.

A recent study by Donald C. Hellmann²⁰ illustrates the relevance of the regulatory arena to the explanation of Japanese foreign-policy behavior in negotiating a peace agreement with the Soviet Union. Hellmann discovers that in the course of negotiating a peace agreement with the Soviet Union, "neither a clear public sentiment nor a broadly based articulate opinion emerged in [in Japan]."²¹ He further finds that business interests generally played a negligible role in the decisional process—notwithstanding the close interlinkage of business, party, and government in Japan, an interlinkage bearing a resemblance to C. Wright Mills's power-elite image (Type IV) of American de-

cision making. With a single exception, Hellmann concludes, the process of negotiating the peace agreement with the Soviet Union was highly elitist, in exactly the sense predicted for Type III issues.

The one exception proves interesting. "One economic issue, Japan's fishing rights in the northern seas, did figure significantly in Soviet relations. Accordingly, the Japanese fishing industry actively participated in the negotiations, seeking access primarily as an interest group working for its own benefit."²² The fishing rights issue was tangible and highly asymmetrical in its domestic impact in Japan—a fact of which Moscow was well aware. Moscow in effect utilized the fishing rights facet of the settlement in order to alter the Japanese political process and thus to alter the outcome of the negotiations. By linking the peace settlement with fishing rights in the northern seas, what was otherwise a Type III protective-interactive issue became instead a kind of international licensing issue, i.e., a Type II regulatory issue.

One imagines, further, that the hypotheses pertaining to the arena of redistribution are transferable to other pluralist systems. A close comparison, for instance, of the domestic politics of Great Britain's Boer War and France's war in Indo-China with the domestic politics of the United States' war in Indo-China would presumably prove these events sufficiently congruent as to justify extending the projections about the redistributive politics of limited wars to all pluralist systems.

Finally, with regard to the protective-interactive arena it seems reasonable to assume tentatively that it is the United States which constitutes the limiting case. As such, we may assume that in other pluralist systems (several of which have less participant political cultures)²³ the decisional process will be no less elitist than in the United States. Bluntly put, crisis decision making appears to be much the same the world

¹⁹ J. Roland Pennock, "The 'Pork Barrel' and Majority Rule: A Note," *Journal of Politics*, 33 (August, 1970), 709–16, at 714.

²⁰ Hellmann, *Japanese Foreign Policy and Domestic Politics: The Peace Agreement with the Soviet Union* (Berkeley: Univ. of California Press, 1969).

²¹ *Ibid.*, p. 149.

²² *Ibid.*, p. 130. Hellmann's conclusion warrants comparison with another important case study in which fish played an important role. I have in mind, of course, Bernard C. Cohen's *The Political Process and Foreign Policy: The Making of the Japanese Peace Settlement* (Princeton: Princeton University Press, 1957). Once again the fish issue was the only really controversial aspect of the settlement. The peace settlement itself provoked little debate. The fisheries issue was settled separately by the North Pacific Fisheries Convention—a process which had all the attributes of regulatory politics. There were, thus, for the United States two issue areas: one, the peace settlement (Type III); the other, the regulatory fisheries convention (Type II). (For a similar assessment, see Rosenau, ed., *Domestic Sources*, pp. 48–49.)

²³ Gabriel Almond and Sidney Verba, *The Civic Culture* (Boston: Little, Brown, 1965).

around; military grand strategy is likewise formulated in a manner according with a rational purposive model.²⁴

Perhaps the most intriguing test of the utility of the paradigm both for its predictive capacity and for its value in reorienting research, however, is not whether the paradigm has relevance for pluralist systems other than the United States. Rather, the crucial question stems from the implication of the paradigm that for comparable events in divergent political systems the policy process within an issue area is sufficiently similar as to explain or predict the foreign policy process. If such is the case, then, the paradigm has predictive value for states which are *neither* totalitarian *nor* pluralist but rather modern authoritarian systems characterized by a polyarchy of elites.

Such a country is the Soviet Union. Do the political structure and political culture of the Soviet Union so greatly shape the nature of policy formation as to preclude the value of issue-related hypotheses in the Soviet case? Nothing approaching a definitive answer can be given in this article—which is intended to prefigure my own further research and to redirect others' research orientation. We can state with considerable confidence that crisis decision making in the Soviet Union is highly elitist in the Type III protective-interactive sense.²⁵ More generally, we can assume that, in terms of process, events subsumed within the protective-interactive arena will correspond to that anticipated by the paradigm. The problem of course is that such a finding by itself is completely consonant with an explanation of the absence of domestic societal influences (save those transmitted through the personalities of the leadership) based on the nature of the political system.

We must look to the other arenas of power to speak to the relation of issues on policy process in the Soviet Union. Since for U.S. foreign policy a major manifestation of distributive arena politics is the intraservice parceling-out of the military budget, a replication of Samuel Huntington's *The Common Defense* in the Soviet context might be in order.²⁶ Such a replica-

tion would make it possible to ascertain whether there are fundamental distinctions in process, as in the United States, between Soviet force-procurement and Soviet strategic decisions, and whether the relevant service branches, ministries, and industrial enterprises are indulged through a disaggregation process brought about by "unprincipled" coalitions from within the Soviet counterpart to the military industrial complex.²⁷

In the Soviet Union, during the post-Stalinist period, influence relations and communications patterns are asymmetrical. Given the interaction between the Soviet society and the regime, and given the redistributive character of limited wars, I would expect that, were the Soviet Union to become extensively embroiled in such a war (say in the Middle East), the domestic political process would resemble the American experience with the Vietnam war; the only major qualification would be that the active role of large social aggregations (nationalities, generations of "fathers and sons" in conflict) would be more circumscribed because of Marxist-Leninist political culture and the Soviet political system. I would fully expect that elites and incipient counterelites would employ symbols designed to appeal to such large social aggregates and that the anticipated response of these political actors would be predominant in shaping the process of intra-elite political behavior. To take a case in point: had the Czechoslovak regime elected in 1968 to resist forcibly the Soviet invasion, and had it succeeded in transforming the Soviet action into a limited war, the fissiparous tendencies within the Soviet Union would have been immense. Indeed, if Alexander Dubcek had made credible the possibility Czechoslovakia would resist, the Soviet policy debate prior to the invasion would have been dramatically altered. The choice would no longer have been intervention with its foreign policy consequences or nonintervention with its foreign-policy costs and the dangers of domestic contamination (especially in the Ukraine)²⁸ from the Czechoslovak disease. Instead, the Politburo would have had to choose between nonintervention, with its possible domestic dangers, and intervention, with the at-

²⁴ Samuel P. Huntington, *The Common Defense* (New York: Columbia Univ. Press, 1961).

²⁵ For evidence that Soviet decision making in the 1962 Cuban missile crises paralleled that in the United States, see U.S. Senate Committee on Government Operations, Sub-Committee on National Security Staffing and Operations, *Staffing Procedures and Problems in the Soviet Union 88th Congress, 1st Session, 1963*.

²⁶ More recent general studies of Soviet civil-military relations include: Roman Kolkowicz, *The Soviet Military and The Communist Party* (Princeton: Princeton Univ. Press, 1967); Thomas W. Wolfe, *Soviet Strategy*

at the Cross Roads (Cambridge: Harvard Univ. Press, 1964); Arnold Horelick and Myron Rush, *Strategic Power and Soviet Foreign Policy* (Chicago: Univ. of Chicago Press, 1966).

²⁷ See, especially, "The Military-Industrial Complex: USSR/USA" *Journal of International Affairs*, 26 (Spring 1972), 1-97.

²⁸ Grey Hodnett and Peter J. Potichny; *The Ukraine and The Czechoslovak Crisis* (Canberra: Australian National Univ., Department of Political Science, Occasional Paper No. 6, 1970).

tendant domestic costs of waging a limited war against a small, friendly, socialist state.

Where political structure probably does have a major impact on the Soviet foreign-policy process is in the arena of regulation. In recent years, considerable work has been done on Soviet interest groups,²⁹ much of it rather desultory in character and almost none of it bearing on foreign policy. That there is an interest articulation process seems no longer seriously at issue; what remains to be clarified is whether, given the prohibition against associational interest groups, the process is suitably depicted by conceptions derived from Western group theorists; i.e., whether the claims for resource allocation priorities are best conceptualized as

the articulation of groups, groupings, or tendencies.³⁰

In any event, viewed against an issue area framework, the traditional dichotomization of authoritarian and pluralist foreign-policy processes needs re-examination. To the extent that the argument developed herein withstands extensive testing against the empirical record, the impact of political structure on process would seem to be considerably more circumscribed than generally assumed. The ultimate conclusion may even be that differences in policy process across issue areas within a given state, the United States or the Soviet Union as cases in point, may be as great as differences in foreign-policy process within a particular arena of power for each.³¹

²⁹ The best is Milton Lodge, *Soviet Elite Attitudes Since Stalin* (Columbus, Ohio: Charles E. Merrill, 1969). See, too, Joel J. Schwartz and William R. Keech, "Group Influence and the Policy Process in the Soviet Union," *American Political Science Review*, 62 (September, 1968), 840-851, and H. Gordon Skilling and Franklyn Griffiths, eds., *Interest Groups in Soviet Politics* (Princeton: Princeton Univ. Press, 1971).

³⁰ E.g., the articles in Skilling and Griffiths.

³¹ After this article was written, Thomas L. Brewer published an important contribution to the literature linking issue-area and foreign-policy behavior in the American case. See his "Issue and Context Variations in Foreign Policy: Effects on American Elite Behavior," *Journal of Conflict Resolution*, 17 (March, 1973), 89-114.

Party and Incumbency in Postwar Senate Elections: Trends, Patterns, and Models*

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In the last half-century or so, incumbents of all offices for which we have data have exhibited greater strength at the polls than have their electoral rivals. Of those incumbents who have sought to retain their position, city councilmen have succeeded about 80 per cent of the time;¹ state legislators have defeated between 70 and 90 per cent of their challengers;² United States senators have achieved re-election in more than 80 per cent of their attempts (as shown in Table 2); and congressmen have emerged victorious at a phenomenal 90 per cent rate.³ Pity the poor vulnerable governor:

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I am indebted to many people for their help in this study. Among those who improved this paper by reading and commenting on earlier drafts are Walter Dean Burnham of the Massachusetts Institute of Technology, Lawrence S. Mayer of the Virginia Polytechnic Institute and State University, Eric Uslaner of the University of Florida, and Ronald E. Weber of Indiana University. The two anonymous referees saved me from several errors and helped clarify the exposition. I am grateful to Washington University for making available the requisite computer facilities. Most of all, though, my thanks go to John D. Sprague of Washington University for sacrificing much time and energy to preach what he practices, superior scholarship. Finally, I gleefully adopt what may turn out to be the new fashion in academic writing: I claim full credit for whatever is worthwhile in the study, and blame all errors on my benefactors.

¹ At least this is the case in the San Francisco Bay area. See Kenneth Prewitt, "Political Ambitions, Volunteerism, and Electoral Accountability," *American Political Science Review*, 64 (March, 1970), 5-17, at p. 9.

² For example, in Connecticut during the period 1946-1958, 982 out of 1201 incumbents won re-election to the state legislature, for a success rate of 81.8 per cent (James David Barber, *The Lawmakers* [New Haven: Yale University Press, 1965], on p. 8); and David Leuthold reports an incumbent success rate of 73 per cent in Missouri state races during the period 1960-1964 (*Electioneering in a Democracy* [New York: Wiley, 1968], p. 127). See also Charles S. Hyneman, "Tenure and Turnover of Legislative Personnel," *Annals of the American Academy of Political and Social Science*, 195 (1938), 21-31, for data covering ten state legislatures over the decade 1925-1935. Finally, Wisconsin data collected by three of my students, Gary Londergan, Patrick Maguire, and Timothy Smock, show an even higher state-level success rate than those found elsewhere. Of the 50 state senators running for re-election in the period 1958-64, 46, or 92.0 per cent, won. Of the 315 state assemblymen running for re-election in the period 1956-62, 271, or 86.0 per cent, won.

³ Computed from figures supplied in Charles O. Jones,

the last three decades have seen him taste victory "only" 64.7 per cent of the time.⁴ It certainly appears that for the office-holder in an election, as for the property-holder in law, possession is worth about nine points. Yet it is not clear that incumbents win only, or even mainly, because they are incumbents. Other factors, such as the strength of the incumbent's party in the constituency involved, could be producing these results.

This paper is concerned primarily with incumbents for the office of United States senator. The two questions it seeks to answer are the following: (1) To what extent do incumbent senators win because they are incumbents, and to what extent do they win because their party is strong in their state? (2) How has the relative importance of party and incumbency in Senate elections changed during the postwar period? These questions merit investigation for at least three reasons. First, an understanding of the role of party in influencing the way Americans vote for subpresidential offices is a necessary complement to the knowledge thus far gained about how the American voter thinks and behaves in presidential contests. Second, the degree to which incumbency helps assure re-election speaks directly to some fundamental notions of the representativeness and (especially) the responsiveness of elected officials and institutions in the American political system. Third, the role of seniority in shaping the behavior of senators, and thereby the policies issuing from the upper chamber, is surely of sufficient consequence to warrant examination of the factors determining seniority itself, namely, re-election. In addition, the answer to the longitudinal question provides fuel for the ongoing debate about whether there is presently underway some sort of "realignment" of American electoral politics. We shall return to some of these questions at the conclusion of the paper.

The study consists of four main parts. First comes a brief review of some of the literature

Every Second Year (Washington, D.C.: The Brookings Institution, 1967), p. 68.

⁴ Computed from figures supplied in J. Stephen Turrett, "The Vulnerability of American Governors, 1900-1969," *Midwest Journal of Political Science*, 15 (February, 1971), 108-32, at p. 118.

on congressional elections dealing with our central question—the relative influence of party and incumbency on electoral outcomes. The second part then describes some of the contours of phenomena probably unfamiliar to most scholars, namely, the extent to which senators have run for re-election during the period 1948–1970, and the extent to which those running have been successful. Both rates (running and winning) are strikingly high. The third part begins to deal with the problem of separating out the effects of party and incumbency in Senate elections. The fourth and final section constitutes the core of the analysis. It develops a theory accounting for the outcomes of Senate elections in terms of the major potential sources of electoral support given to the candidates. The theory is then represented symbolically in formal models for which multiple regression techniques are used to generate quantitative estimates of the relative importance of party and incumbency in all postwar Senate contests. This section closes with a brief summary of the findings and a statement regarding their implications for the changing shape of contemporary American politics.

Literature Review: Incumbency and Congressional Elections

The major recent work on the electoral effects of House incumbency has been carried out by Milton C. Cummings, Charles O. Jones, and Robert S. Erikson. Cummings devoted an entire chapter to the topic in his excellent volume, *Congressmen and the Electorate*,⁵ analyzing its relationship to such phenomena as "Presidential tide," third parties, and special local factors. One of Cummings's research questions was quite similar to what we ask of senatorial races: How important is the strength of the *party* in a given constituency, and how important is the strength of the individual (e.g., incumbent) *candidate* in shaping the electoral outcome? The answer his data convey is that "party" is by far the more consequential of the two, although incumbency does in fact have a significant independent impact.⁶ For example, in the period covered by his analysis, incumbent representatives of the party that lost the presidency fared much better than nonincumbents of that losing party. More precisely, 84.7 per cent of the House candidates who won in the face of an unfavorable "Presidential tide"

were incumbents.⁷ Indeed, even in the party "rising with the tide," incumbents do better than nonincumbents, although the difference is of course much less pronounced.

Charles Jones's research in this area further underscores not only the "independent" electoral advantage of House incumbents, but also the importance of "party" in influencing election results. For example, even when an incumbent is not running (i.e., has died, retired, or been defeated in a primary), the *incumbent party* candidate is likely to prevail three-fourths of the time.⁸ Obviously, then, there is more at work than "incumbency" in assuring a congressman re-election; the nature of the district is often such that his party is strong, or the opposition party is weak, or both.

Another effort to gauge the independent influence of incumbency in House elections is that of Robert Erikson.⁹ He used multiple regression techniques with data from 1954 to 1960 to determine the percentage of the vote accounted for by incumbency. His analysis shows the advantage of House incumbency to be about two percentage points. In a subsequent study Erikson found that by the late 1960s, the advantage had grown. For example, in 1966 being a House incumbent was worth about five percentage points.¹⁰ These patterns are especially suggestive in the light of the finding to be reported in the present paper that the advantage of *senatorial* incumbency, despite variation, has generally increased in the postwar period.

The principal works dealing directly with the electoral impact of senatorial incumbency are Donald R. Matthews's classic study, *U. S. Senators and Their World*,¹¹ and Barbara Hinckley's recent article, "Incumbency and the Presidential Vote in Senate Elections: Defining Parameters of Sub-presidential Voting."¹² Unfortunately, not all of Matthews's findings are directly comparable to those reported

¹ Computed from figures supplied in Cummings, p. 60.

³ Charles O. Jones, "The Role of the Campaign in Congressional Politics," in *The Electoral Process*, ed. M. Kent Jennings and L. Harmon Zeigler (Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1966), pp. 21–41.

⁹ Robert S. Erikson, "The Advantage of Incumbency in Congressional Elections," *Polity* (Spring, 1971), 395–405.

¹⁰ Robert S. Erikson, "Malapportionment, Gerrymandering, and Party Fortunes in Congressional Elections," *American Political Science Review*, 66 (December, 1972), 1234–45, at pp. 1240 ff.

¹¹ Donald R. Matthews, *U. S. Senators and Their World* (New York: Vintage Books, n.d. First published 1950). See especially pp. 239–42.

¹² Barbara Hinckley, "Incumbency and the Presidential Vote in Senate Elections," *American Political Science Review*, 64 (September, 1970), 836–42.

⁵ Milton C. Cummings, Jr., *Congressmen and the Electorate* (New York: The Free Press, 1966), chapter 3.

⁶ A fair inference, it would seem, from the data presented in Cummings, pp. 78–79.

here since, for example, this study does not analyze senators in terms of his "career type" classification (i.e., Amateur, Professional, Patrician, Agitator). It is worth noting, however, that he was able to demonstrate clearly that a senator's chances for re-election are indeed related to the degree of party competition in his state.

Hinckley's article explores the question of "how much of the fluctuation in vote for Senator is explainable by the 'pull' of the presidential vote,"¹³ on the one hand, and how much can be explained by senatorial incumbency, on the other. Employing as the standard the state-by-state estimates of base party identification developed by Ronald Weber and Frank Munger,¹⁴ she tries to determine how much of the deviation from this standard can be statistically explained by her two variables. Through a simple correlation analysis, she concludes that in the period 1956-1966, 64 per cent of the fluctuation in voting is accounted for by this pair of nonidiosyncratic factors.

Although there is some overlap between Hinckley's concerns and those of the present paper, the two studies differ in several crucial respects. For one, while Hinckley's investigation utilizes data only from the decade 1956-1966, the present inquiry covers nearly the entire postwar period. More important, however, is the manner in which the two sets of data are analyzed. For example, Hinckley treats the entire decade as a single, highly aggregated unit of time, examining neither differences between the parties nor changes over time. The present work examines both. In addition, the statistical technique used in the Hinckley piece is correlation, while the principal method used here is multiple regression. This latter method seems clearly preferable for at least three important methodological reasons: (1) it forces us to be explicit about the *form* of the relationships we posit when we construct the models for which the parameter estimates are to be generated; (2) it enables us to concentrate primarily on

the *shape* of the relationships being examined rather than focusing exclusively on the less interesting and really less enlightening "goodness-of-fit" criterion of correlation analysis; and (3) it provides us with actual *prediction equations* which we can then use to identify deviant cases, a procedure which has as one of its desirable outcomes the likelihood of learning something new and unexpected about the political world.¹⁵

Incumbency and Senate Elections: Postwar Patterns and Trends

This section addresses two principal sets of questions. To what extent have senatorial incumbents run for re-election? To what extent have those running been successful? In dealing with the first phenomenon, I shall deal exclusively with the overall pattern, investigating differences between presidential and off-year elections, and attempting to determine whether there is a trend over time. With the main concern of the paper, the "success rate" of senators, the analysis proceeds a step further. In addition to looking at the *overall* pattern, presidential and off-year differences, and trend, the analysis also separates *by party* and goes through the same procedure once more.

"Few die and none resign,"¹⁶ says the only slightly overstated Capitol Hill aphorism describing the extent to which congressmen cling to their jobs. There is ample justification for applying it as well to the behavior of senators during the last quarter-century. Of the 392 general Senate elections during that period, 320 involved incumbents.¹⁷ That is, 81.6 per cent of

¹³ Hinckley, p. 838.

¹⁴ Ronald E. Weber and Frank Munger, "Party Identification and the Classification of State Party Systems," paper delivered at the 1968 Annual Meeting of the American Political Science Association, Washington, D.C. Their basic approach is to use what is known about the relationship between party identification and the demographic and socioeconomic characteristics of voters to construct a number of "voter-types." Determining the proportion of a state's electorate fitting each such "type" permits an estimate of each party's identifiers in that state. The approach is similar to that employed in the Simulatics project reported by Ithiel de Sola Pool, Robert P. Abelson, and Samuel Popkin in *Candidates, Issues, and Strategies*, rev. ed. (Cambridge: MIT Press, 1965).

¹⁵ Arguments and evidence for this general position can be found, among other places, in Ronald J. Wonnacott and Thomas H. Wonnacott, *Econometrics* (New York: John Wiley & Sons, Inc., 1970), especially pp. 124-25; Edward R. Tufte, "Improving Data Analysis in Political Science," in *The Quantitative Analysis of Social Problems*, ed. Edward R. Tufte (Reading, Mass.: Addison-Wesley, 1970); and, for the *coup de grâce* to the goodness-of-fit criterion in theory construction, William Feller, *An Introduction to Probability Theory and Its Applications*, Volume II (New York: John Wiley & Sons, Inc., 1966), p. 52. The relevant portions of Feller's point are summarized and quoted in Adam Przeworski and Glauco A. D. Soares, "Theories in Search of a Curve," *American Political Science Review*, 65 (March, 1971), 51-68, in fn. 34 on p. 67.

¹⁶ Clem Miller, *Member of the House* (New York: Charles Scribner's Sons, 1962), quoted at p. 93.

¹⁷ *Incumbent* is defined here, and throughout the study, as a senator who has achieved the office through *election*, even a special election. Rates of running and winning always refer only to general election contests. A person holding the office at the time of the general election by virtue of appointment was not considered an incumbent. The reason for this distinction is presented in the final section.

Although I do not deal with primary contests here, I

those contests saw a senator seeking to remain in office. While even this figure does not match the ninety-plus House percentage (for 1956-1966),¹⁸ it is impressively high nonetheless.

As Table 1 indicates, there appears to be very little difference between presidential and off-years in the rate at which incumbents choose to seek re-election. The rate for presidential years is 82.1 per cent, for off-years 81.1 per cent. For purposes of comparison, it is worth noting that a similarly minor difference between the two types of election years occurred for the House during the decade 1956-1966: in presidential years—91.3 per cent of the races involved incumbents; in off-years—90.0 per cent.¹⁹

Figure 1 depicts the per cent of Senate contests involving an incumbent in each election year of the period under scrutiny. To determine whether these data contain a trend, the percentage of races involving incumbents (the "y-values") are regressed on time (the "x-values"), where $T = 1$ in 1948 and $T = 12$ in 1970. This results in the equation

$$(1) \quad \hat{P} = 0.05 T + 81.3,$$

showing the trend (slope) to be nearly zero. This technical operation adds weight to an impressionistic conclusion that there has been no real increase in the postwar period in the rate at which senators have run for re-election.

The belief prevalent on Capitol Hill is not only that congressmen *want* to remain in

might note for the record that only about 7 per cent of the postwar senators who sought renomination were denied it, a higher figure than the House's 1.6 per cent (from data in footnote 3), to be sure, but still small enough to pose no real problem to the present analysis.

¹⁸ Computed from figures in Jones, *Every Second Year*, p. 68.

¹⁹ Jones, p. 68.

Table 1. Number and Percentage of Senatorial Contests Involving Elected Incumbents: Presidential and Off-Year Elections Compared: 1948-1970

Year	Presidential Election Years		
	Number of Contests	Number Involving Incumbents	Percentage Involving Incumbents
1948	32	23	71.9
1952	32	27	84.4
1956	32	29	90.6
1960	33	28	84.8
1964	33	30	90.9
1968	34	24	70.6
Totals	196	161	82.1

Year	Off-Year Elections		
	Number of Contests	Number Involving Incumbents	Percentage Involving Incumbents
1950	32	25	78.1
1954	32	26	81.2
1958	32*	26	81.2
1962	34	30	88.2
1966	33	26	78.8
1970	33	26	78.8
Totals	196	159	81.1

Source: Congressional Quarterly, *Politics in America*, third edition (Washington, D. C.: Congressional Quarterly Service, 1969), pp. 133-144; *Biographical Directory of the American Congress: 1774-1961* (Washington, D. C.: Government Printing Office, 1961); *Congressional Directory*, 88th through 92nd Congresses (Washington, D. C.: Government Printing Office, various dates). Where discrepancies occurred between the government and the private source, the former was preferred.

* Two Alaska and two Hawaii contests omitted since they could not have involved incumbents.

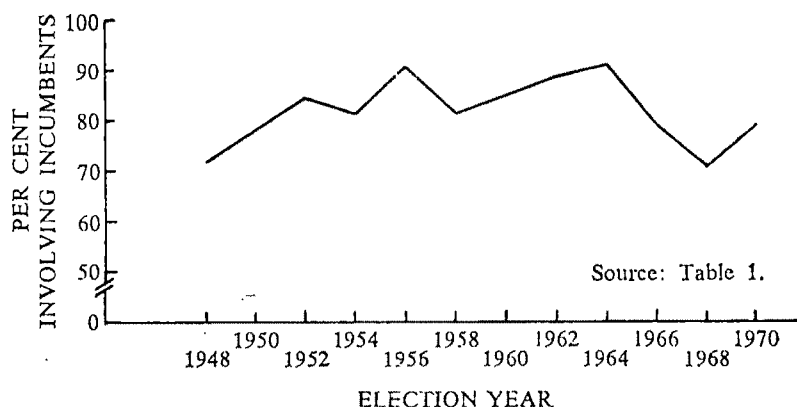


Figure 1. Percentage of Senatorial Contests Involving Elected Incumbents: 1948-1970

**Table 2. Number of Elected Senatorial Incumbents Running for Re-election;
Number and Percentage of Winners: 1948-1970**

Year	Number of Incumbents	Number Who Won	Percentage Who Won	Year	Number of Incumbents	Number Who Won	Percentage Who Won
1948	23	15	65.2	1960	28	27	96.4
1950	25	20	80.0	1962	30	27	90.0
1952	27	20	74.1	1964	30	28	93.3
1954	26	22	84.6	1966	26	25	96.2
1956	29	25	86.2	1968	24	20	83.3
1958	26	17	65.4	1970	26	23	88.5
Totals					320	269	84.1

Source: See Table 1.

office, but also that they ought to be able to succeed in doing so. Charles Clapp, in *The Congressman*, summarizes the views of representatives thus: "There is a tendency to believe that, aside from isolated instances where an overriding issue is present, there is little excuse for defeat."²⁰ To what extent is this belief borne out in fact for the House counterparts, the Senators?

The number and per cent of senatorial incumbents running and winning are given in Table 2. Despite some fluctuations over time, the unmistakable generalization emerges that senators are usually returned to office. Out of 320 incumbents seeking re-election, 269, or 84.1 per cent, won. Moreover, there is virtually no difference between presidential and off-years. In presidential years, 135 out of 161 senators defeated their challengers, for a success rate of

83.8 per cent; in off-years, 134 out of 159 incumbents prevailed, for a rate of 84.3 per cent. Once again the pattern is similar to that for House members (for 1956-1966)²¹: presidential years—92.7 per cent were successful; off-years—92.3 per cent were successful.

A more detailed and direct comparison of the overall Senate rate with that of the House is provided by Table 3, showing the number and per cent of congressional and senatorial incumbents successful in their bids for re-election during the seven election years from 1954 to 1966. Both rates are high and unexpectedly close, at least given the conventional wisdom that senators are significantly more vulnerable: House—92.8 per cent; Senate—88.2 per cent.

Interestingly enough, the postwar trend has been toward even greater safety for senatorial incumbents, as Figure 2 amply illustrates. Re-

²⁰ Charles Clapp, *The Congressman* (Garden City, N.Y.: Anchor Books, 1963), p. 374.

²¹ Jones, *Every Second Year*, p. 68.

**Table 3. Number of Congressional and Senatorial Incumbents Running for Re-election;
Number and Percentage of Winners: 1954-1966**

Year	Number of House Incumbents	Number Who Won	Percentage Who Won	Number of Senatorial Incumbents	Number Who Won	Percentage Who Won
1954	401	379	94.5	26	22	84.6
1956	405	389	96.0	29	25	86.2
1958	391	354	90.5	26	17	65.4
1960	398	372	93.5	28	27	96.4
1962	382	368	96.3	30	27	90.0
1964	389	345	88.7	30	28	93.3
1966	402	362	90.0	26	25	96.2
Totals	2,768	2,569	92.8	169	149	88.2

Source: House figures computed from data supplied in Charles O. Jones, *Every Second Year* (Washington, D.C.: The Brookings Institution, 1967), p. 68, and, for 1954, in Charles O. Jones, "The Role of the Campaign in Congressional Politics," in *The Electoral Process*, ed. M. Kent Jennings and L. Harmon Zeigler (Englewood Cliffs, N.J.: Prentice-Hall, 1966), at p. 24. Senate figures, see Table 1.

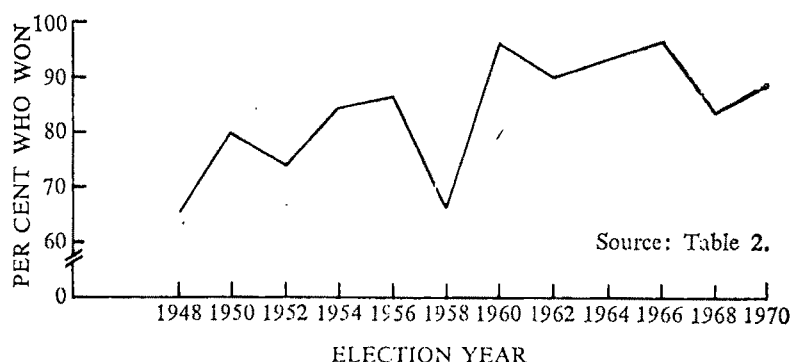


Figure 2. Percentage of Elected Senatorial Incumbents Running for Re-election Who Won: 1948-1970

gressing the percentage of successful senators on the time variable produces the equation

$$(2) \quad \hat{Y} = 1.68 T + 73.0,$$

where $T = 1$ in 1948. In other words, each election year sitting senators have become, on the average, almost "two per cent safer." Over a period of twelve elections, such an average increase becomes substantial, indeed.

This increase in safety for incumbents is, of course, not confined to the upper house. Some indication of the trend for the lower chamber is contained in the observation of Charles Jones that, for the Congress, "... between 1932 and 1940, 69.9 per cent of the districts were won by the same party in all five elections; between 1942 and 1950, 74.0 per cent were won by the same party; and between 1952 and 1960, 78.2

per cent were won by the same party."²²

Useful as these overall findings are, they may very well obscure some important differences between the parties. As the comparable data arrayed in Table 4 effectively demonstrate, that is, in fact, the case. While about 90 per cent of Democratic senators won re-election, "only" about three-fourths of the Republicans were so fortunate. Whether this difference is due to the Democrats' superior campaigning skill, greater money and charm, and more attractive families, or whether this difference in *incumbency* success rate is due more systematically to dif-

²² Jones, p. 68. Note, however, that this trend refers to the safety of districts for the *party*. Although we assume the general pattern is similar for the safety of *incumbents*, an empirical demonstration of that assumption's validity is clearly beyond the scope of the present paper.

Table 4. Number of Elected Senatorial Incumbents Running for Re-election; Number and Percentage of Winners, By Party: 1948-1970

Year	Democrats			Republicans		
	Number of Incumbents	Number Who Won	Percentage Who Won	Number of Incumbents	Number Who Won	Percentage Who Won
1948	10	10	100.0	13	5	38.5
1950	15	11	73.3	10	9	90.0
1952	9	6	66.7	18	14	77.8
1954	17	16	94.1	9	6	66.7
1956	13	12	92.3	16	13	81.2
1958	12	12	100.0	14	5	35.7
1960	18	17	94.4	10	10	100.0
1962	18	17	94.4	12	10	83.3
1964	23	23	100.0	7	5	71.4
1966	14	13	92.9	12	12	100.0
1968	17	13	76.5	7	7	100.0
1970	20	18	90.0	6	5	83.3
Totals	186	168	90.3	134	101	75.4

Source: See Table 1.

ferences in *party* strength in the electorate, remains to be seen. Indeed, it is precisely this question that is explored initially in the next part, and analyzed further in the final section of this paper.

Comparing the pattern for each party in presidential versus off-year elections reveals that there is no appreciable difference *within the parties* between the two types of election years. For the Democrats, the presidential year success rate is exactly 90.0 per cent, with the off-year rate an almost identical 90.6 per cent. For the Republicans, the difference is only slightly greater. In presidential election years, 76.1 per cent win, while in the off-years the figure is 74.6 per cent.

To determine whether the postwar trends for the two parties differ, consult Figure 3, which presents in graphical form the percentage of each party's senatorial incumbents who were successful each election year. As with the earlier estimates of trend lines the percentage successful is regressed on time to produce a trend for the Democrats described by the equation

$$(3) \quad \hat{Y} = 0.46 T + 86.5,$$

where $T=1$ in 1948. Although there is some movement toward greater safety, it is fairly small. But according to the equation, the Democrats start out so safe (an average of about 87 per cent win in 1948, solving at $T=1$) that even the slight increase of five or six percentage points over the past quarter century could have important political consequences.

The trend for the Republicans is far stronger. Their equation reads

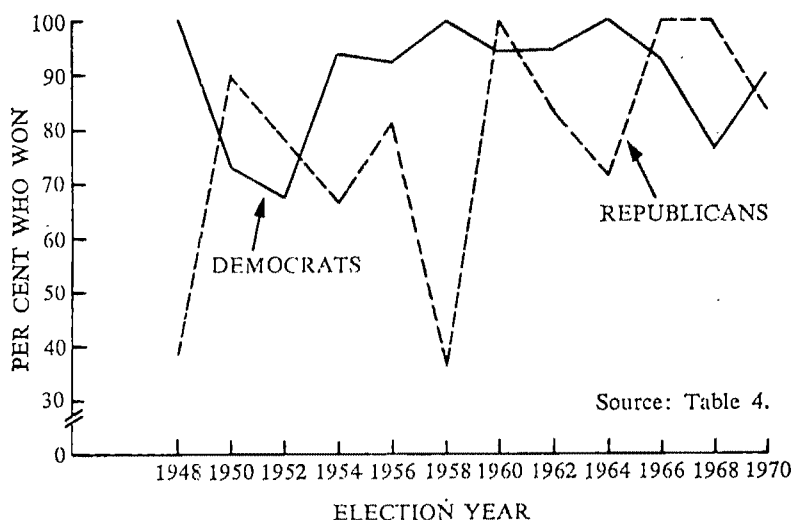
$$(4) \quad \hat{Y} = 2.91 T + 58.4,$$

where $T=1$ in 1948. Here we find that Republican senators won only about 60 per cent of their re-election bids immediately following the War, but now win, on the average, more than 90 per cent of them. This marked change suggests once more that we must ask whether the Republican gain is due to their party having grown stronger (and/or the Democrats weaker), or to a greater independent electoral importance assumed by incumbency.²³ Analysis of this question is, of course, the task of the next two sections of the paper, with the final part paying special attention to the longitudinal aspects of the problem.

The discussion up to now has been confined to nationally aggregated patterns. Within-state patterns are suggestive as well.²⁴ For example, it is interesting to note that California had the

²³ Naturally, trend lines such as these are highly sensitive to extreme values at the end points of the time series. For example, adding 1946 to the present pair of series brings the two trend lines a good deal closer together due to the high success (100 per cent) of Republican incumbents and the low success (66.7 per cent) of Democrats in that year. The Democratic trend is then described by the equation, $\hat{Y} = 1.12 T + 81.1$, while the Republican estimate becomes $\hat{Y} = 1.54 T + 69.8$, where $T=0$ in 1946 in both instances. And yet despite such extreme readings, the difference between the parties does still remain, and still points to the same question: why the different rate of change for each party over time?

²⁴ These within-state patterns are based on data from 1946–1970.



Source: Table 4.

Figure 3. Percentage of Elected Senatorial Incumbents Running for Re-election Who Won: by Party: 1948–1970

smallest proportion of its postwar Senate contests involving an incumbent—four out of nine; the remaining states all had a sitting Senator vying for the seat in at least half, and sometimes all, of their Senate races. And a second observation, this one suggestive of the electoral power of incumbency, is that three of the four states that did worst in returning senators to office still re-elected incumbents fifty per cent of the time.²⁵ A success rate of one-half is what would be expected under the assumption of purely random outcomes of a win-lose situation, so the fact that the remaining forty-six states had success rates in excess of fifty per cent is consistent with the notion that incumbents have factors working systematically in their behalf.

Preliminary Analysis of the Relationship Between Party and Incumbency

Party strength is one of those systematic factors we suspect affects the incumbent success rate. Some idea of the extensive overlap, in a quite literal sense, between the two can be gained from the following simple "exercise": List all those states where both Senate seats were won by the same party in every general election of the last two-and-a-half decades. There are fourteen such states. Ninety-nine incumbent senators ran for re-election in those states and ninety-seven won. Now relax the

criterion for selection of the states to include those where the same party won both seats every time but once. There is now a total of twenty-two states in the list. Next, make up a separate list of all those states in which a senator seeking to remain in office always succeeded in doing so. There are twenty-six such states. This exercise reveals that seventeen states appear on both lists, a considerable overlap.

If this overlap operates as hypothesized, then the more politically competitive states ought to have lower senatorial incumbency success rates, and vice versa. One way to examine this hypothesis is to group the states according to some measure of political competitiveness and to compare the success rates. The rationale underlying Table 5 is that the "regional" groupings are much less competitive than the "large industrial" states, and that states in neither category fall between these two extremes of competitiveness. The senatorial incumbency success rates follow the expected pattern.

A somewhat more sophisticated classification of the states according to party competitiveness has been developed by Austin Ranney.²⁶ His scheme averages together four measures of Democratic electoral strength in state offices over the period 1946–1963 to produce an "index of competitiveness" that ranges from 1.0000 (complete Democratic control) to 0.0000

²⁵ Maryland retained in office only two of the six senators seeking reelection. The three states returning an even half of their incumbents are Kentucky, Pennsylvania, and Wyoming.

²⁶ Austin Ranney, "Parties in State Politics," in *Politics and the American States*, ed. Herbert Jacob and Kenneth N. Vines (Boston: Little, Brown and Company, 1965), pp. 61–99; see especially pp. 63–70.

Table 5. Number of Senatorial Incumbents Running for Re-election in Certain Categories of States; Number and Percentage of Winners: 1946–1970

Category of States ^a	Number of States	Number of Incumbents	Number Who Won	Percentage Who Won
Total South	11	77	76 ^b	98.7
Deep South	(5)	(38)	(38)	(100.0)
Fringe South	(6)	(39)	(38)	(97.4)
"Solid North"	7	49	48 ^b	98.0
Border	6	41	29	70.7
Large Industrial	7	44	31	70.5
Remainder	19	126	98	77.8

Source: See Table 1.

^a The states are categorized as follows: Deep South: Alabama, Georgia, Louisiana, Mississippi, South Carolina; Fringe South: Arkansas, Florida, North Carolina, Tennessee, Texas, Virginia; Border: Kentucky, Maryland, Missouri, New Mexico, Oklahoma, West Virginia (Maryland's Senators won only twice out of six tries, thus accounting for the lower than expected success rate for this category of state. If Maryland were omitted from the calculations, the per cent for this group would be 77.1); Large Industrial: California, Illinois, Michigan, New Jersey, New York, Ohio, Pennsylvania; "Solid North": Kansas, Maine, Nebraska, New Hampshire, North Dakota, South Dakota, Vermont.

^b For the curious, the one Southern Democrat failing re-election was Albert Gore of Tennessee in 1970, and the one "Solid North" Republican was Payne of Maine, who lost to Edmund Muskie in 1958.

Table 6. Number of Senatorial Incumbents Running and Winning in the States:
(States Classified According to Degree of Inter-Party Competition^a)
Percentage of Winners, By Category: 1946-1970

One-Party Democratic			Modified One-Party Democratic			Modified One-Party Republican		
State	No. of Incumbents	No. Who Won	State	No. of Incumbents	No. Who Won	State	No. of Incumbents	No. Who Won
So. Carolina	7	7	Virginia	8	8	Wisconsin	8	7
Georgia	7	7	No. Carolina	6	6	N. Hampshire	8	8
Louisiana	8	8	Tennessee	6	5	Iowa	6	4
Mississippi	9	9	Oklahoma	6	5	Kansas	5	5
Texas	6	6	Kentucky	6	3	Maine	7	6
Alabama	7	7	Arizona	7	6	So. Dakota	5	5
Arkansas	8	8	W. Virginia	9	7	No. Dakota	9	9
Florida	5	5	Maryland	6	2	Vermont	7	7
			New Mexico	7	7			
Totals	57	57		61	49		55	51
Per cent		100.0			80.3			92.7

Two-Party								
State	No. of Incumbents	No. Who Won	State	No. of Incumbents	No. Who Won	State	No. of Incumbents	No. Who Won
Alaska	3	3	Colorado	6	5	Illinois	8	5
Missouri	7	5	Montana	7	6	Idaho	7	5
Rhode Island	7	7	Minnesota	6	4	Michigan	7	5
Washington	8	7	Utah	9	6	New Jersey	6	6
Delaware	8	5	Connecticut	6	4	Indiana	7	6
Nevada	8	7	Pennsylvania	8	4	Oregon	6	4
Massachusetts	8	6	California	4	3	Ohio	6	4
Hawaii	3	3	Nebraska	8	8	Wyoming	6	3
						New York	5	4
						Totals	164	125
						Per cent		76.2

Source: See Table 1.

^a From Austin Ranney, "Parties in State Politics," in *Politics in the American States*, ed. Herbert Jacob and Kenneth N. Vines (Boston: Little, Brown and Company, 1965), pp. 61-100. The states are listed here within each category according to their decreasing "Democratic-ness." For a brief explanation of the "index of competitiveness" upon which these categories are based, see the text.

(complete Republican control). An index score of 0.5000 signifies that the parties are perfectly competitive. The resulting clusters of states suggested the categories reproduced here in Table 6.

Once again a clear relationship between party and incumbency emerges. The most competitive states have the lowest senatorial incumbency success rates, and the least competitive the highest. One interesting sidelight is the difference between the success rates in modified one-party Democratic states (80.3 per cent) and modified one-party Republican states (92.7 per cent). A possible explanation for the difference may lie in the extent to which the state party systems are allied to or coincide with the national party systems in Democratic-domi-

nated versus Republican-dominated states, for while Ranney's measure is based exclusively on state offices, the position of senator is usually considered a national office.

Because both the Ranney index and the incumbency success rate can be considered interval measures, and because a value for both variables can be assigned to each state, performing an experiment may identify somewhat more precisely just how these two phenomena are related. Since the concern in this instance is more generally with "competitiveness" than specifically in Democratic or Republican strength, we first reconstruct part of the Ranney index. Using the measure as it is given for the interval 1.0000 to 0.5000, the interval

0.4999 to 0.0000 is recalculated by subtracting each value therein from 1.0000. This more general index of competitiveness then varies from 1.0000, non-competitive, to 0.5000, perfectly competitive.

The hypothesis being investigated is that the more politically competitive the parties are, the less likely to achieve re-election a senator will be. Hence, the dependent variable, the senatorial incumbency success rate (measured by the proportion of times sitting senators seeking re-election in a given state during the postwar period were successful), is regressed on the independent variable, the revised index of competitiveness. The relationship between these phenomena is then expressed by the equation

$$(5) \quad \hat{SR} = 0.58 C + 0.42,$$

where \hat{SR} is the senatorial incumbency Success Rate and C is the degree of Competitiveness.

This result provides an extremely interesting substantive interpretation of the independent impact of senatorial incumbency on electoral outcomes. What it says is that in a completely noncompetitive state, whether controlled by Democrats or Republicans, senators will win re-election every time (solving for $C = 1.0$). At the other extreme, where the state parties are absolutely competitive ($C = 0.5$), senators will still defeat their challengers about seven times out of ten. If this result is to be believed, the "independent" (nonparty-related) advantage a senatorial incumbent brings to the polls is very real and quite substantial. The odds are clearly in his favor. Two caveats must of course be entered, however. One is that the result obtained employed what must be regarded as two fairly crude measures, despite the pseudo-accuracy of carrying out the estimates to four decimal places. The second is that the Ranney index, being based solely on state offices, would be expected to produce some "slack" in its relationship to the incumbency phenomena for a national office. In any event, however, the least that can be said of this finding is that it is highly intriguing and suggestive of even better methods of generating precise estimates of the types of relationships in which we are most interested.

Models of Party, Incumbency, and "National Tides"

Whatever the specific nature of the role of party and incumbency in Senate elections, there is obviously persuasive evidence that they both exert great influence on those electoral outcomes. To understand fully the electoral process therefore requires gauging as accurately as

possible their separate and independent impact on such elections. The mode of analysis employed here involves representing the theory in a formal model for which are then generated quantitative estimates of the appropriate parameters through least squares multiple regression. In constructing the model the strategy is to draw upon available knowledge about political behavior in order to approximate "reality" as closely as possible. In short, the procedure involves embodying certain beliefs about the political world in a formal representation, and then testing those beliefs against what data are most relevant to the questions at hand.

Theory and Model. The phenomenon to be explained, or predicted, is the percentage of votes received by postwar senatorial candidates, given certain other information about the election. To accomplish this task involves identifying the major potential sources of a senatorial candidate's electoral support. The argument presented here is that there are three systematic, measurable sources common to all Senate races and one group of unsystematic, unmeasurable sources peculiar to each contest. "Party loyalty," incumbency, and "national tides" fall under the first rubric, while such idiosyncratic factors as personality, issues, and local conditions compose the second group.

Party identification exerts an extremely powerful influence on voting in the United States. Angus Campbell and his associates estimated that of those voters in their 1952 samples who had formed no evaluations of "political objects" (i.e., no intervening variables entering into their vote choice), 75 per cent voted their party identification.²⁷ Moreover, a substantial degree of straight-ticket voting is associated with party loyalty.²⁸ In states where straight-ticket voting is facilitated by the ballot form used, 77 per cent of the strong identifiers supported their party right down the line, with a full 70 per cent of the weak identifiers following suit. Even in states where such voting is hindered by the form of the ballot, 55 per cent of the weak identifiers chose to stick with one party, while just as many strong identifiers as in the "easy" states—77 per cent—selected the straight ticket. And equally significant is the nature of the split ticket, when that route is chosen. The finding is that when voters do cross

²⁷ Angus Campbell, Philip Converse, Warren Miller, and Donald Stokes, *The American Voter* (New York: John Wiley and Sons, 1960), p. 142.

²⁸ The following figures are taken from Angus Campbell and Warren E. Miller, "The Motivational Basis of Straight and Split Ticket Voting," *American Political Science Review*, 51 (June, 1957), 293-312.

party lines, they are far more likely to do so for state and local offices than for senator or congressman.²⁹

But it must be emphasized that there are in fact degrees of party identification, such as the "strong" and the "weak" mentioned above, and that there are demonstrable differences in the manner in which those degrees of attitudinal commitment are translated into "party votes," as the ballot-form example amply illustrates. The most loyal and deeply committed party identifiers will vote "party" under even the most severe cross-pressures and counter-trends, while the remainder will be far more likely to defect due to election-specific personalities and the issues of the day. It will be important to bear this in mind in selecting the indicator of "party loyalty," for one is needed which taps the true core of the party supporters.

The second potential source of a candidate's support is derived from the fact of being the incumbent. Above and beyond party influences, an incumbent is likely to receive an additional increment of votes because he holds the office at the time of the election. This extra margin is evident in the literature review of the first part of this paper, the descriptive materials of the second part, and the experiment with the revised Ranney index of competitiveness and the various cross-tabulation analyses of the immediately preceding section.

We should be explicit about the reason the current office-holder has an electoral advantage, for it should not be thought that "incumbency" is some sort of magical, mysterious phenomenon. There are good solid political explanations for the incumbent's garnering additional support. Although they have been examined often enough by scholars to permit our doing without a detailed exposition here, it may still be worthwhile to provide at least a skeletal framework of the larger arguments.³⁰

(1) The incumbent is likely to have substantial political skill and experience, and often a superior political organization, owing to his having been through the mill, successfully, at least once previously.

(2) He should enjoy greater voter recognition than his opponent through having been in the public eye during the last campaign and during the preceding years of his term, through his use of the franking privilege, the carrying

out of casework, appearances at public events, and so forth. This simple fact of public awareness can prove incredibly powerful. Stokes and Miller³¹ have shown that in the 1958 congressional races, of those voters who were aware only of the candidate of their own party, 98 per cent voted for him; conversely, of those aware only of the other party's candidate, 40 per cent voted for him. Who, we may ask, is the average voter likely to be aware of more, an incumbent senator, or his challenger? While there are certainly some cases where the challenger is even more well-known than the senator (suppose Bart Starr ran against Senator Gaylord Nelson in Wisconsin, or Joe Namath ran for the Senate in New York . . .), the reverse is surely more common. We can probably say, then, that for senators as for congressmen, incumbents are more salient to the voters than are nonincumbents, and that "in the main, recognition carries a positive valence; to be perceived at all is to be perceived favorably."³²

(3) The office-holder is likely to have a fair amount of political clout within his party because of patronage, fund-raising activities, prestige, favors of all kinds. This should make it easier for him to acquire a wide variety of campaign resources, from workers, to information, to credit.³³ Moreover, since fund-providers probably prefer to back a winner,³⁴ and since the success rate of incumbents has presumably not been lost on potential contributors, the incumbent should find money considerably easier to come by than his opponent.

In addition to party and incumbency, a third systematic influence on the electoral support received by a senatorial candidate is what I term "national tides." Although it is *not* generally true that "As Maine goes, so goes the nation," it is generally true that "As the nation goes, so

²⁹ Donald E. Stokes and Warren E. Miller, "Party Government and the Saliency of Congress," chapter 11 in *Elections and the Political Order*, by Angus Campbell, Philip Converse, Warren Miller, and Donald Stokes (New York: John Wiley and Sons, 1966), pp. 194-211, at p. 205.

³⁰ Stokes and Miller, in Campbell et al., *Elections and the Political Order*, p. 205.

³¹ See Leuthold, *Electioneering in a Democracy*, for an admirable discussion of congressional campaigns largely in terms of resource acquisition. His observations and findings undoubtedly apply, *mutatis mutandis*, to senatorial contests as well.

³² Such an inference can easily be drawn from Leuthold, Table 1, p. 67, at least insofar as labor unions are concerned. In addition, chapter 6 deals with the whole range of fund-raising questions. A comment regarding incumbents is telling: "Nonincumbents may have needed more money but they found it harder to raise, and ended up with less money than incumbents" (p. 77).

²⁹ Campbell and Miller; see their Table VI, at p. 306.

³⁰ A brief review of the electoral advantages of congressional incumbency can be found in Stephen K. Bailey, *Congress in the Seventies* (New York: St. Martin's Press, 1970), pp. 9-11. See also Robert Erikson, "The Advantage of Incumbency in Congressional Elections,"

goes your state.”³⁵ The point is, of course, that state elections held at the same time as a national election tend to be influenced by the national issues being raised and discussed, as well as by the national candidates in a presidential election year. A number of scholars have worked with the notion of “presidential tides,” but they tend to neglect the very real fact that there are often equally strong trends, or “tides,” in the off-year as well, and that they usually register similarly in each state. Clearly, 1958, although an off-year election, exhibited such a “tide,” and few are in doubt as to its direction.

Just as the third part of the paper established that party and incumbency are related both to each other and to Senate electoral outcomes, it can be demonstrated here that “national tides” and incumbency are similarly related. This demonstration will add some indirect evidence that the phenomenon of “national tides” is worth including in the analysis. Let us take as a rough indication of the overall “national tide” for a particular year the difference between the percentage of the total votes cast for a party’s House candidates this election year and the percentage cast for their candidates the previous election year. For example, the 1958 Republican “tide” was -5.3 per cent, the 1958 Democratic “tide” was +5.3 per cent (the 1958 House percentage minus the 1956 House percentage). Next we take an analogous difference for each party’s senatorial incumbency success rates in those two election years. Comparing the two differences for each election year of the postwar period reveals that in ten out of twelve elections the differences have moved in the same direction. The only two years they do not (and the discrepancy occurs in the same years for both parties) are 1952 and 1962, and in both instances the margin by which they fail to do so is almost negligible. Furthermore, the R^2 produced by a regression of the (differences between) senatorial incumbency success rates on the “national tides” variable exceeds 57 per cent for both parties.

So the conclusion is that “tides” do indeed systematically influence (Senate) elections. Yet the “tides” will have different effects in different states and, unlike the other phenomena, can have a punitive influence on the outcomes for one party while helping the other party. The most extreme example of the differential effects by state occurred in 1964. While virtually the entire country was sweeping toward Lyndon

Johnson and the Democrats, the five states comprising the Deep South were moving quite dramatically in the opposite direction, against the Democratic “tide.” Such an occurrence is of course perfectly consistent with the argument that these states were in fact responding to the same national issues and personalities as the remaining states; that they chose a different stand on the issues (especially race) and personalities is of no consequence to the theory. It does remind us, however, that since our analysis is state-centered, the “tides” variable must vary across states as well as over time. And it underscores the fact that failure to control for such phenomena in the analysis could have a confounding effect on our understanding of the relationship of central interest here, that between party and incumbency.

The final source of votes for a candidate involves such idiosyncratic elements as personality, issues, and local factors. In some elections, of course, these could conceivably outweigh all others, though it is the contention here that such an event is rare. The effect of these idiosyncratic factors is not measured directly, but is estimated indirectly, as will be shown below.

The reasoning thus far forms a basis for representing symbolically what has been stated in words. Bear in mind, however, that our “theory” comprises our words and sentences and hypotheses about politics; the model is but a shorthand formalization of its main features. It cannot be overemphasized that what we constantly seek are sentences about the political world; the symbols and numbers we employ are merely means to that end.

With that said, then, the theory is represented thus:

$$(6) \quad S = f(P, I, T, Id)$$

where:

S = the per cent of the votes received by a particular senatorial candidate,

f = means “a function of,”

P = “core party loyalty,” or “base party vote,”

I = incumbency,

T = “national tides,”

Id = idiosyncratic factors.

In words, this says that the percentage of votes received by a particular senatorial candidate is a function of the proportion of the voters who are “core party loyalists”; his status as an incumbent or nonincumbent; the magnitude and direction of the “national tides”; and, finally, his personality, the issues raised, and other local factors (e.g., the strength of the party organization and his relationship to it, the power of the labor unions and their traditional

³⁵ The close relationship between national and state electoral results is documented in Louis Bean, *How to Predict Elections* (New York: Alfred A. Knopf, 1948) and in V. O. Key, Jr., *American State Politics* (New York: Alfred A. Knopf, 1956), especially in chapter 2.

role in elections in that state, factionalism, etc.).

The theory is represented symbolically in this most *general* form to emphasize that there are any number of different *specific* forms the relationships could take. These various sources could be seen as being strictly additive in their effects, as the formal model in (9), below, indicates. But it is also plausible to assert, for example, that the particular portion of the voting population that will be most likely to vote for a candidate because he is the incumbent are those who are not "core party loyalists" for either party, for those loyalists are so locked into their party that they will vote for or against a candidate purely on the basis of his party label. Then, if P_d represents the proportion of the electorate which supports the Democrats under any and all conditions, and P_r represents those solidly loyal to the Republicans, then $1 - (P_d + P_r)$ stands for the proportion in neither group. Given our working hypothesis, and still assuming for convenience that the effects of the phenomena in (6) are additive, this revised theory could be represented as

$$(7) \quad S_d = a(P_d) + b(I)[1 - (P_d + P_r)] + c(T) + Id$$

where the statement has been restricted to Democratic candidates for ease of comprehension. Naturally, a , b , and c are parameters measuring the increment of support given the candidates from the indicated source.

An alternative formulation involves the contextual argument that although a certain proportion of the population is likely to vote for one party because of some individual loyalty or commitment, there is also a group effect. Hence, as Warren Miller, for example, found, at the higher concentrations of party strength, the interaction among the party faithful increases, as does the probability of their voting for their party's candidates.³⁶ This might be represented by the model

$$(8) \quad S = a(P * P) + b(P) + c(I) + d(T) + Id$$

where $(P * P)$ is, under random mixing assumptions, the probability that a given interaction involves one party loyalist interacting with another party loyalist, and a is the probability that the interaction will result in an additional vote for the senatorial candidate of that party. The rest of the model stands as before.³⁷

³⁶ Warren E. Miller, "One Party Politics and the Voter," *American Political Science Review*, 50 (September, 1956), 707-25.

³⁷ For a stimulating analysis of left voting which em-

phases the importance of the *form* of the relationships among phenomena, as well as a more detailed development of the "contextual" argument, see Przeworski and Soares, "Theories in Search of a Curve." On the problem of forms of relationships as it relates to an American electoral situation, see Walter Dean Burnham and John D. Sprague, "Additive and Multiplicative Models of the Voting Universe: The Case of Pennsylvania, 1960-68," *American Political Science Review*, 64 (June, 1970), 471-90.

$$(9) \quad S = a(P) + b(I) + c(T) + Id$$

is the model for which are estimated the parameters a , b , and c , and the intercept, Id .

Variables. The task now is to select the variables to be used as indicators of the phenomena the theory stipulates as most important. The variable for S is simply the percentage of the total popular votes cast for each major-party senatorial candidate in each state at each general election from 1948 to 1970.³⁹

Several contenders could serve as the variable measuring P . Two are especially attractive. The first is the set of estimates of base party identification developed initially by Ronald Weber and Frank Munger and used by Barbara Hinckley in her article on Senate elections cited earlier. The measure is described briefly in footnote 14 of the present paper. Unfortunately, two serious limitations prevent their adoption here. One is that the estimates are currently available only for the period 1954-1966, yet the present study examines the entire postwar period. The other difficulty attending the Weber estimates is that they are

phases the importance of the *form* of the relationships among phenomena, as well as a more detailed development of the "contextual" argument, see Przeworski and Soares, "Theories in Search of a Curve." On the problem of forms of relationships as it relates to an American electoral situation, see Walter Dean Burnham and John D. Sprague, "Additive and Multiplicative Models of the Voting Universe: The Case of Pennsylvania, 1960-68," *American Political Science Review*, 64 (June, 1970), 471-90.

³⁸ The two alternative forms mentioned here, as well as several others, are developed further and the parameters estimated in my paper, "Form and Substance in Theory Construction: Electoral Behavior in Postwar Senate Elections," presented at the Annual Meeting of the American Political Science Association, Washington, D.C., September 5-9, 1972. In addition, the contextual argument is elaborated in that paper several steps beyond the sketch given here.

³⁹ The source relied upon for all election data was the *Statistical Abstract of the United States* (Washington, D.C.: Government Printing Office, various dates). Errors noted were corrected (e.g., on two occasions special Senate elections were listed in the general election category) but otherwise the figures were used as given. Incumbency information was culled from the sources listed in Table 1 of the present paper.

attitudinal measures, while a behavioral measure is preferred. There are important differences between base party *identification* and base party *vote*.

Consequently, the "base party vote" measure used for the variable *P* in the model is constructed in the following manner. (1) Compute the per cent of popular votes cast for the House candidates of each party in each state for each election year from 1946 to 1970. For example, for a given state in a given year, the Democratic figure is calculated thus:

$$\frac{\text{Total number of votes cast for all Democratic House candidates}}{\text{Total number of votes cast for all House candidates}}$$

(2) For a given election year, say, 1960, choose as the "base party vote" for each state (i.e., the value for *P*) the *lowest* per cent of votes for House candidates cast in that state during the five election years of which the "target year" (in this instance, 1960) constitutes the midpoint year. In other words, for a particular state (e.g., Wisconsin) look at the percentage of votes garnered by the congressional candidates of a particular party (e.g., the Democrats) in each of the following years: 1956, 1958, 1960, 1962, 1964. The lowest Democratic percentage of those five elections is the Democratic "base party vote" for the midpoint year of the series, 1960. The same operation is carried out separately for the Republicans.⁴⁰

Observe that this procedure involves going a maximum of only four calendar years away from the target year to get the value of *P* for that year. Knowledge of the glacier-like pace at which *core* ("strong") party loyalty changes certainly suggests that such a short span of time is very unlikely to misrepresent the size of the group of loyalists. And yet there is being employed a behavioral measure of the proportion of the electorate in a given state which, by its *actions*, has demonstrated that it will support a given party under the severest conditions "of recent memory."

Two principal difficulties, neither of them crippling, make this choice of variable less than ideal. For one, although the vote for House candidates probably approaches better than most the notion of a "normal" party vote,⁴¹

it is not perfect. Ironically enough, there is bound to be some distortion introduced by the fact that many House candidates are incumbents! The extent to which this is true will vary somewhat among states and between parties, to be sure, but the problem is nonetheless a real one.⁴² Some correction is afforded by choosing the worst performance out of five elections, for even though most incumbents will probably *win* in trying times, their *share* of the vote is probably diminished, and it is the percentage received rather than the won-loss record that is the relevant datum.

The second problem with the measure is that of turnout. More people vote in presidential than in off-year elections; furthermore, the types of people voting differ between election years. These differences are likely to introduce distortion into any kind of measure of "party vote" based on actual votes. Yet the problem is less serious in fact than its potential suggests. For a study by Harvey Kabaker designed to test the extent to which turnout does actually influence the partisan division of the vote in *congressional elections* concludes that the effect is really quite minimal.⁴³ Hence there is reason to have some confidence that this indicator of "base party vote" is in fact measuring what our conceptual apparatus says it should.

Choosing a variable for incumbency is a simple operation. If a candidate is the current office-holder, and has achieved the position by virtue of an *election*, he is considered the incumbent. An election, rather than an appointment, is required because a principal source of strength for an incumbent, as argued above, is the political skill, organization, personal following, and recognition that results from having waged a successful campaign. In addition, appointees are sometimes selected to fill a vacancy precisely because they pose no real political threat to the governor appointing them; yet the governor can use the opportunity to reward favors bestowed. Evidence of the role of these factors is the lower re-election rate experienced by appointees as compared with those who

Elections and the Political Order, Campbell et al., pp. 9-39.

⁴² An extreme case is that of Nevada, where the "base party vote" for the Democrats exhibited an exceptionally strong and consistent increase during most of the postwar period. Whether this is due to a "true" growth of Democratic loyalists or to the fact that there is only one Representative who, having been the incumbent since 1956, has been able to steadily tighten his hold on the electorate over the last decade and a half, is a moot question. The potential for distortion in such a situation is clear.

⁴³ Harvey M. Kabaker, "Estimating the Normal Vote in Congressional Elections," *Midwest Journal of Political Science*, 13 (February, 1969), 58-83.

⁴⁰ At the end points of the election series, the following modifications prevailed: the "base party vote" for 1948 was selected from the period 1946-1952; that for 1968 from 1964-1970; and that for 1970 from 1966-1970.

⁴¹ The concept is developed and explicated by Philip E. Converse in "The Concept of a Normal Vote," in

have been elected, even in a special election. Once ascertained that the candidate meets the criterion set forth here, the variable I is set equal to 1; if the candidate is not the incumbent, $I=0$.

The "national tides" variable must be chosen to reflect the way a given state has responded to the general trend in the nation. Therefore, the "tide" for a particular party in a particular year in a given state is measured by the percentage of the votes received by that party's House candidates in the present election year minus the percentage received the immediately preceding election year.

The idiosyncratic factors are not measured directly. They are estimated with the constant term (the intercept) of the equation which results from directly estimating the rest of the parameters of the model through regression.

In using these variables to estimate the parameters of the model through multiple regression, we separate by party and by year.⁴⁴ That is, there are separate estimates of the relative importance of the phenomena contained in the theory for each party and for each election year from 1948 to 1970—twenty-four sets of estimates in all (two parties times twelve election years).

An overall model for each Senate "class" was also estimated, separating by party as before. In other words, all the Democratic party election data for each year from 1948 to 1952 were collected into one set in order to get one (slightly more stable, there being more observations) estimate, and the same was done for the Republicans. The procedure was repeated for each subsequent six-year sequence. The obvious rationale here is that in each six-year period all Senate seats will have been contested once and only once in a general election. Each period is designated as follows: Group 1, 1948–1952; Group 2, 1954–1958; Group 3, 1960–1964; and Group 4, 1966–1970. Let us now turn to the analysis of these estimates.

Results and Analysis: Grouped Data. The main focus in this analysis is on (1) the relationship between party and incumbency in Senate elections both within and between the two major political parties, and (2) changes in those rela-

tionships during the postwar period. The primary purpose of the "tides" variable is to remove certain confounding elements from the relationship between the two phenomena of central interest. Thus, although we will report its values and discuss its interpretation in the hypothetical data example below, it should be considered, for the most part, a "control variable," whose function is to control for otherwise distorting effects.

The results of the multiple regression on the grouped data are reported in Table 7. Both the actual parameter estimates (including the constant term) and the standardized regression coefficients, or beta-weights, are presented. I will employ the former to illustrate the use of these results as actual prediction equations. However, because the variables have somewhat different distributions between parties in a given election year (or Group), and even more varied distributions over time, the similarities and contrasts between the parties and over time will be analyzed in terms of the standardized coefficients.

Let us first describe the use of these equations in a hypothetical prediction situation. Recall that the model is

$$[(9) \text{ repeated}] \quad S = a(P) + b(I) + c(T) + Id,$$

where a , b , and c have now been estimated from data by multivariate statistical techniques. When the estimates for Group 3 (1960–64) are used, for example, the model for the Democrats reads

$$(10) \quad \hat{S} = 0.54(P) + 0.09(I) + 0.50(T) + 0.23,$$

and for the Republicans it reads

$$(11) \quad \hat{S} = 0.40(P) + 0.10(I) + 0.58(T) + 0.26,$$

where S , P , T , and Id (the constant term) are expressed in proportions, not percentages. If in 1964 the "base party vote" for the Democrats in a given state was 0.485 (i.e., 48.5 per cent) and the Democratic "national tide" in that state that year was +0.05, we would "predict" that a Democratic senatorial candidate who was the incumbent ($I = 1$) would receive

$$(12) \quad 0.607 = 0.54(0.485) + 0.09(1) + 0.50(+0.05) + 0.23,$$

or 60.7 per cent of the vote. Note that being an incumbent was "worth" nine per cent.

If in the same state that year the Republican "base party vote" was 0.475 (suggesting that the two parties were highly competitive), the

⁴⁴ Uncontested Senate elections were omitted from the analysis. Cases where the Representatives from a state were unopposed eliminated that state from the analysis for that year (i.e., the "tides" variable was considered to have a missing value for that year, which required deletion of the entire state). Such an occurrence was quite infrequent. All statistical computations were carried out on Washington University's IBM 360/50 via the *Statistical Package for the Social Sciences* (SPSS) routines.

Table 7. Multiple Regression Estimates of the Effects of Party, Incumbency, "National Tides," and Idiosyncratic Factors on Senate Elections, Grouped Data: 1948-1970

Years	Democrats							
	Parameters				Beta-Weights			$R^2(\%)$
	Party	Incumbency	Tides	Constant	Party	Incumbency	Tides	
1948-52 (1) ^a	.72 (.07) ^b	.044 (.02)	.62 (.11)	.16	.71	.19	.36	70.2
1954-58 (2)	.66 (.09)	.07 (.02)	.36 (.12)	.19	.61	.36	.22	67.2
1960-64 (3)	.54 (.11)	.09 (.02)	.50 (.13)	.23	.39	.45	.31	57.6
1966-70 (4)	.10 (.12)	.12 (.02)	.29 (.14)	.40	.08	.58	.19	50.2

Years	Republicans							
	Parameters				Beta-Weights			$R^2(\%)$
	Party	Incumbency	Tides	Constant	Party	Incumbency	Tides	
1948-52 (1)	.71 (.08)	.01 (.02)	.46 (.12)	.17	.72	.07	.28	62.7
1954-58 (2)	.58 (.10)	.06 (.02)	.17 (.14)	.19	.53	.31	.10	49.4
1960-64 (3)	.40 (.09)	.10 (.02)	.58 (.12)	.26	.34	.51	.36	61.6
1966-70 (4)	.15 (.11)	.11 (.02)	.40 (.13)	.39	.12	.50	.27	43.3

^a Data are grouped into "classes" of three consecutive election years. Number in parentheses denotes the Group or "class."

^b Standard error of the parameter.

senatorial candidate was not, of course, the incumbent ($I = 0$), and the Republican "national tide" was equal in size but opposite in sign to the Democratic (i.e., -0.05). the model "predicts" for that candidate

$$(13) \quad 0.421 = 0.40(0.475) + 0.10(0) + 0.58(-0.05) + 0.26,$$

or 42.1 per cent. Note that if the Republican were the incumbent instead of the Democrat, we would predict victory for the GOP candidate. Also, observe that party and incumbency, when both are present (i.e., $I=1$), are more substantial sources of support than are the idiosyncratic factors represented by the constant term. Finally, notice that if neither candidate was the incumbent, the Democrat would still win, but with slightly more than a majority of the total vote.

One of the reasons the Group 3 results were selected to exemplify the use of the models as prediction equations—and to illustrate thereby the substantive interpretation of the theory underlying the model—is that the parameters are all very stable. By this is meant that the coefficients are all more than twice the size of their standard errors. In terms of "statistical significance," this means that we can reject in each case the null hypothesis (i.e., that there is no linear relationship between the independent variable and the proportion of votes received by the senatorial candidates) with less than a five per cent chance of error. But a "statistical significance" interpretation is not strictly applicable here for it is a population that is involved rather than a sample, namely, the universe of all Senate elections from 1948 to 1970. Nevertheless, the relationship between the size of the

coefficients and their standard errors does give some indication both of the basic stability of the estimates and of the extent to which there is in fact a non-zero relationship between each independent variable and the dependent variable. Consequently, the results will be discussed in this light.

In only two instances is a parameter patently indistinguishable from zero, that is, in only two instances is the standard error *larger* than the coefficient itself. These occur with the Group 4 Democratic estimate of the effect of party, and the Group 1 Republican estimate of the effect of incumbency. As shall be seen, neither case causes any real problem of substantive interpretation, since the actual estimate of these effects should be nearly zero anyway.

Two other estimates, both in Republican "Classes," are somewhat unstable, viz., the Group 4 party coefficient and the Group 2 "tides" coefficient. These are only "somewhat unstable" because although the coefficient is not twice the size of its standard error, it is still the larger of the two. Fortunately, the party coefficient again causes no real problem substantively, as will become evident below. Nevertheless, the small size and relative instability of the Republican "tide" is curious given what we know about 1958. Yet since its function is that of a control variable, we need not concern ourselves here with its behavior.

Turning to the first of the two substantive questions at hand, examination of the beta-weights reveals no particularly outstanding incongruity *between parties* in the relative importance attaching to the various factors in each Group. The only relatively consistent difference between the party models is the greater amount of variance explained (R^2) for the Democrats than for the Republicans.⁴⁵

Far more interesting, though, is the response provided by the models to the second question of central concern: What has been the direction and magnitude of the *change* in the relative importance of party and incumbency, for both political parties, in Senate elections of the postwar period? The answer, shown schematically

in Figures 4 and 5, is as dramatic as it is unequivocal: The importance of party has undergone a sharp, secular decline, while the importance of incumbency has experienced an almost commensurate increase. There are only two notable differences between the parties. For one, the magnitude of the predominance of incumbency over party for the Republican "Class" of 1960-1964 is almost three times that for the Democrats, although the direction of the difference is the same in both cases. Second, while the *decline* of party is unquestionably monotonic and nearly linear for both parties, the *increase* in the importance of incumbency continues unabated over the whole period only for the Democrats; the increase for the Republicans, more rapid during the first dozen years, levels off for the last three elections.

Inspection of the "constant" term and the R^2 column of Table 7 shows them to be consistent with the theoretical and substantive implications of these patterns. As party has decreased in influence, and incumbency has taken up part, but not all, of the "slack," the role of idiosyncratic factors in Senate elections has steadily grown. Examination of the R^2 column of the same Table bears this out since, for the Democrats especially but for the Republicans as well, the ability to predict by relying on the two principal systematic factors (plus "national tides") has been appreciably reduced. The difference for both parties between the amount of variance explained by the models in the early part of the postwar period (1948-1952) and the amount explained in the last part (1966-1970) is approximately 20 per cent. The variance explained is still quite high, to be sure, but the drop is nonetheless considerable.

Analysis of Results: Ungrouped Data. The same general conclusions can be drawn from an analysis of the ungrouped, election year-by-election year data. The parameter and beta-weight estimates for each party are recorded in Table 8, with the values of the standardized regression coefficients for party and incumbency depicted in Figures 6 and 7. Once again there is a marked overall decline in the relative importance of party, and a concomitant increase in the relative importance of incumbency. Setting 1956 aside as a "deviant case" for the Republicans would accentuate the steadiness of these trends, while doing the same with 1958 for the Democrats would have a similar effect. In addition, the constant term and R^2 values follow the expected pattern. The influence of idiosyncratic factors has increased by an average of roughly one per cent each biennial election, and the amount of variance explained has

⁴⁵ Yet this should not lead us to conclude that the parties are in any important sense "the same," for the similarity may very well be due mainly to the nature of the variables employed. The Republican S-values are almost always complements of the Democratic S-values. Although the P-values for the two parties are certainly not linear transformations of each other, there is nonetheless enough of an inverse relationship between them (and between the incumbency variables for the two parties) to make the similarity in behavior of the separate party equations as much of a technical expectation as of a substantive finding. I am grateful to an anonymous referee for this observation.

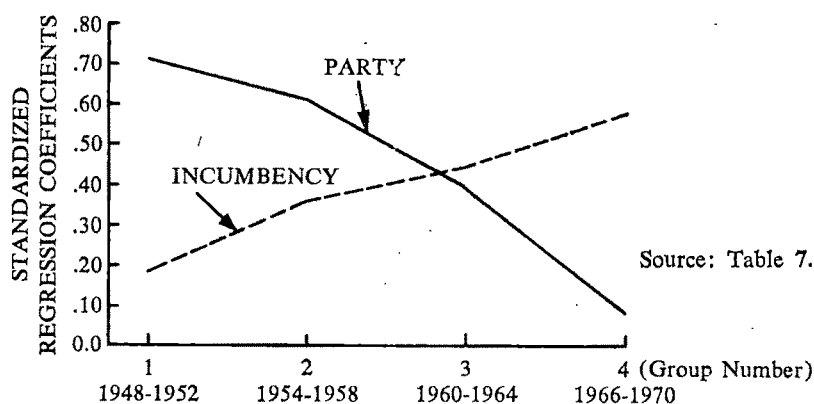


Figure 4. Multiple Regression Estimates of the Effects of Party and Incumbency on Senate Elections, from a Model Employing "National Tides" as a Control Variable: Data Grouped into "Classes" of Three Consecutive Election Years Each: Standardized Regression Coefficients: Democrats, 1948-1970

fallen by a total of almost 40 per cent during the quarter-century covered by the analysis, a drop twice the size encountered in the grouped data.

Still, it must be admitted that the ungrouped data reveal somewhat more complex phenomena at work than do the grouped data. Either set of data permits us to assert the clear predominance of party in Senate elections during the period 1948-1952; the ungrouped data extend that conclusion to 1954 for the Republicans, to 1956 for the Democrats. Thereafter, however, the interpretations based on the grouped data require at least three important qualifications: (1) While the overall decline in the importance of party did continue over the entire period for both parties, the Republican decline from the late 'fifties onward certainly lacks the urgency suggested by the grouped data results (cf. Figure 5). (2) For both parties,

incumbency has *not* shown a strong *consistent* increase in importance from the late 'fifties onward. This finding was already apparent for the Republicans from an analysis of the grouped data, but constitutes a modification of the conclusion of the earlier analysis regarding the Democrats. Although both sets of results attach greater contemporary importance to incumbency than to party for Democratic senatorial candidates, the ungrouped data suggest that this is due less to the continued overall increase in the incumbency trend reported in Figure 5 and more to the continued overall decline in the party trend. (3) The general pattern of the late 'fifties and the subsequent decade is one of fluctuation in relative importance from election to election, rather than the persistent dominance of either incumbency or party. In fact, in many elections it is impossible to determine whether one factor really exerted more influ-

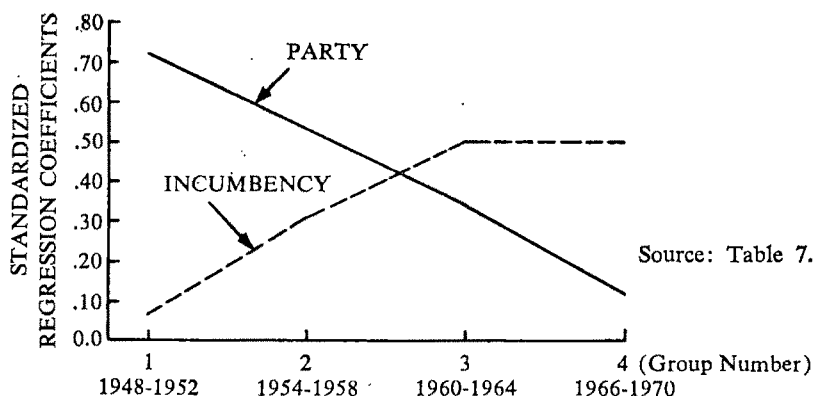


Figure 5. Multiple Regression Estimates of the Effects of Party and Incumbency on Senate Elections, from a Model Employing "National Tides" as a Control Variable: Data Grouped into "Classes" of Three Consecutive Election Years Each: Standardized Regression Coefficients: Republicans, 1948-1970

Table 8. Multiple Regression Estimates of the Effects of Party, Incumbency, "National Tides," and Idiosyncratic Factors on Senate Elections: 1948-1970

Year	Republicans							
	Parameters				Beta-Weights			R ² (%)
	Party	Incumbency	Tides	Constant	Party	Incumbency	Tides	
1948	.68 (.10) ^a	.01 (.03)	.83 (.36)	.20	.88	.06	.29	71.8
1950	.78 (.13)	.03 (.029)	.28 (.31)	.28	.75	.13	.10	73.2
1952	.92 (.15)	.01 (.02)	.28 (.26)	.07	.80	.07	.12	69.1
1954	.64 (.09)	.05 (.02)	-.39 (.24)	.17	.72	.21	.16	88.0
1956	.20 (.22)	.08 (.03)	.30 (.24)	.37	.17	.49	.20	44.0
1958	.57 (.15)	.07 (.03)	-.09 (.31)	.14	.54	.34	-.04	51.9
1960	.39 (.14)	.10 (.02)	.25 (.23)	.28	.40	.57	.14	66.4
1962	.42 (.12)	.10 (.03)	.29 (.20)	.26	.46	.51	.16	65.8
1964	.47 (.15)	.11 (.03)	1.12 (.29)	.25	.43	.44	.52	55.0
1966	.32 (.17)	.13 (.03)	-.03 (.26)	.33	.30	.59	-.02	51.5
1968	.47 (.14)	.08 (.038)	.72 (.32)	.26	.46	.28	.30	55.1
1970	.34 (.15)	.12 (.05)	-.08 (.35)	.25	.35	.38	-.03	31.3
Year	Democrats							
	Parameters				Beta-Weights			R ² (%)
	Party	Incumbency	Tides	Constant	Party	Incumbency	Tides	
1948	.63 (.08)	.06 (.03)	1.04 (.28)	.18	.84	.20	.37	79.4
1950	.69 (.10)	.04 (.03)	.40 (.25)	.16	.75	.17	.16	78.4
1952	1.06 (.13)	-.01 (.03)	.19 (.24)	.01	.88	-.04	.09	80.6
1954	.56 (.10)	.06 (.02)	-.58 (.23)	.27	.62	.23	-.25	88.9
1956	.57 (.08)	.06 (.02)	.51 (.17)	.23	.72	.28	.27	83.8
1958	.37 (.17)	.15 (.03)	.29 (.23)	.32	.28	.65	.13	72.8
1960	.77 (.24)	.08 (.036)	.02 (.28)	.12	.52	.36	.01	62.6
1962	.80 (.13)	.07 (.02)	-.12 (.19)	.11	.66	.34	-.07	69.0
1964	.41 (.17)	.10 (.03)	.59 (.25)	.29	.35	.51	.33	52.1
1966	.21 (.16)	.14 (.03)	.04 (.21)	.34	.19	.68	.03	56.3
1968	.36 (.21)	.08 (.037)	.54 (.44)	.31	.28	.36	.20	40.9
1970	.39 (.19)	.12 (.04)	.02 (.34)	.29	.31	.46	.01	34.5

^a Number in parentheses is the standard error of the coefficient.

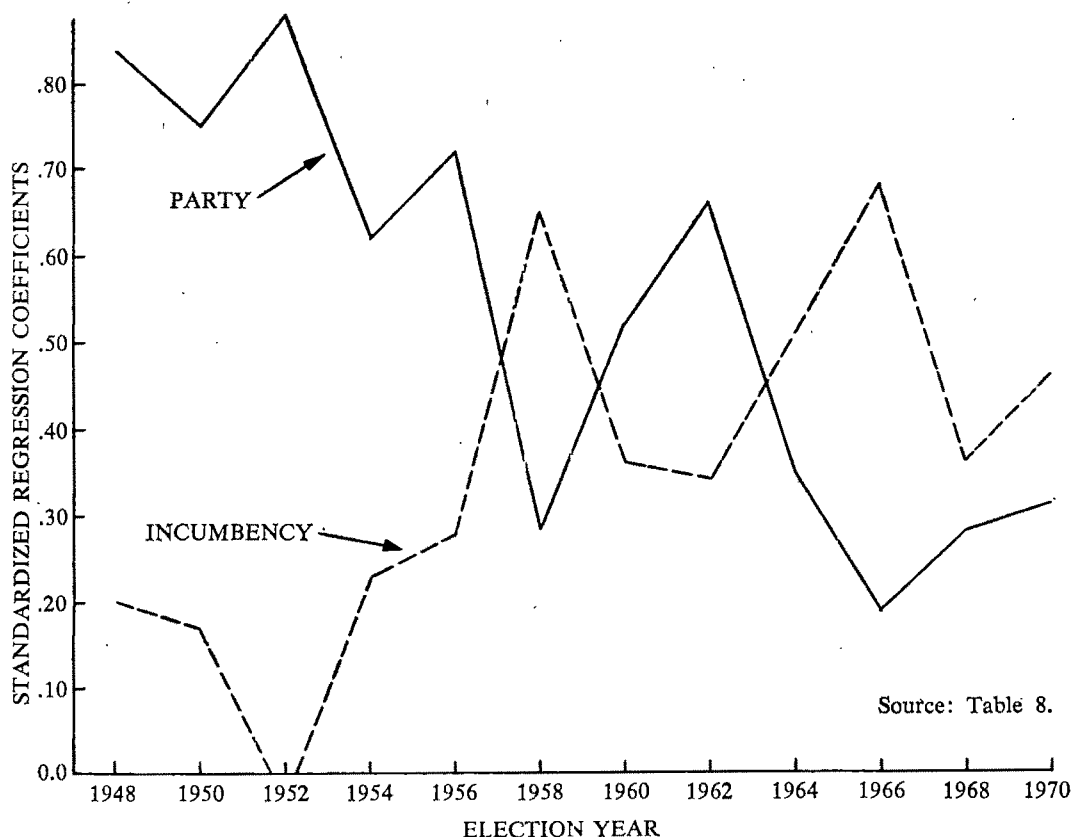


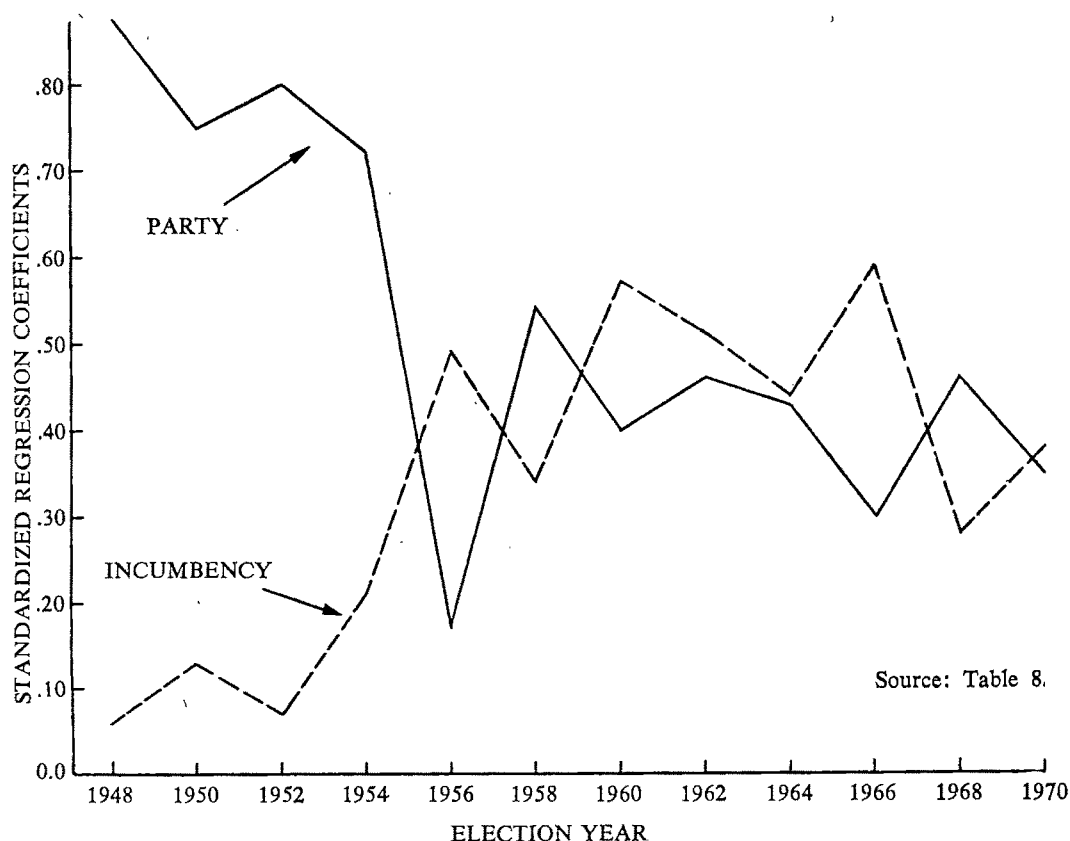
Figure 6. Multiple Regression Estimates of the Effects of Party and Incumbency on Senate Elections in Each Election Year from 1948 through 1970: Standardized Regression Coefficients from a Model Employing "National Tides" as a Control Variable: Democrats

ence than the other because the difference between them is so small. This difficulty is attenuated in 1962 and 1966 for the Democrats, and in 1966 for the GOP, but remains for the other years.

The stability of the estimates based on the ungrouped data is not as adversely affected by the reduction in the number of observations as might have been expected. The exception is the "tides" variable, for which the standard error is larger than the coefficient 8 out of 24 times. But, as noted before, this need not concern us, given the function the variable serves.

Such great instability occurs only three times for the incumbency variable, and only once for party. The instability of the incumbency variable causes no theoretical problems since it happens early in the period; even the very stable estimates of the influence of incumbency during those election years are very small. The standard error, however, is vexingly large for the party coefficient in 1956, near the middle of

the election series. But even here the substantive interpretation is a plausible one. We know that the outpouring of support for Eisenhower was far from a strict "party vote"; that is, many non-Republicans voted for the President's reelection. Although Eisenhower's "coattails" were not considered particularly long that year, the size of the "tides" component in our model (fourth-largest in the entire Republican series) indicates that they did in fact have some effect on the Senate elections. Many of those who voted for Eisenhower voted for the Republican candidate for the Senate as well. Nevertheless, the principal influence on the electoral support given Republican senatorial candidates that year was the congeries of idiosyncratic elements discussed previously. They probably were able to come into play, of course, partly because of the "loosening" effect of the strong "presidential tide." The only other times the standard error of the party coefficient approaches the size of the coefficient itself are



Source: Table 8.

Figure 7. Multiple Regression Estimates of the Effects of Party and Incumbency on Senate Elections in Each Election Year from 1948 through 1970: Standardized Regression Coefficients from a Model Employing "National Tides" as a Control Variable: Republicans

three instances in the late 1960s, again posing no real problem of interpretation, given the overall pattern.

Summary and Implications. The major finding is that the relative importance of party and incumbency in Senate elections has changed dramatically over the last quarter-century. Party has undergone an overall decline in influence, while incumbency has experienced a roughly proportionate increase. At the same time, the importance of idiosyncratic factors has grown. These patterns are virtually identical for both major political parties. Naturally, these trends have been accompanied by a substantial decrease in the (statistical) explanatory ability of the theory and models employed in the analysis.

These findings fit well with previous research documenting the present malaise in the capacity of "party" to structure both mass electoral behavior and congressional voting behavior.⁴⁶

⁴⁶ See, e.g., Gerald M. Pomper, Badi G. Foster, Charles E. Jacob, Wilson C. McWilliams, Alan Rosen-

Clearly a crucial linkage between electors and elected has been weakened. This study indicates, however, that incumbency now serves, at least in Senate contests, as an important alternate voting cue to party, thereby assuming some of the linkage function formerly performed by party. But the increase in the structuring role played by incumbency has not been sufficient to offset the substantial decline in party's ability to play that role. The consequence, of course, is a more tenuous, less regularized overall relationship between the voters and candidates for the U.S. Senate. This situation of relative disarray is reflected in the present analysis in the increased importance of idiosyncratic factors and in the decreased explanatory power of the models employed.

The implications of these findings for more general theories of American politics are wide-reaching and profound. For example, they ad-

thal, and Jay A. Sigler, *The Performance of American Government* (New York: The Free Press, 1972), pp. 81-86.

dress quite directly the ongoing debate over the existence of recurring periods of "critical realignment" in American electoral history.⁴⁷ Some scholars have argued that the United States is presently in the midst of such a realignment sequence. While the results of the present study do not necessarily contradict such an interpretation, they do disagree with certain areas of the argument. One such area involves the timing of the decline in the relative power of party to influence electoral behavior. Some studies date a precipitate drop in the power of party as recently as the mid-1960s, citing, among other things, the sharp increase at that time in the proportion of those who identify

themselves as "independents."⁴⁸ While such evidence is highly suggestive, the present data strongly suggest that, at least in Senate elections, the decline has been far more steady and long-term. Indeed, the effect of party has been muted during the entire postwar period. Thus, at the very least, this finding would seem to demand a fresh look at the subpresidential manifestations of such realignment phenomena.

These findings have implications as well for a number of fundamental problems of democratic theory. For example, the switch in the relative strengths of party and incumbency in influencing Senate elections bears on basic questions of the representativeness and responsiveness of American democratic institutions. For if "party" is no longer the controlling force it once was in determining electoral outcomes, and if representatives are more beholden to incumbency for their success at re-election, then new definitions of the relationship between voters and elected officials may be required. The nature and quality of the electoral and policy linkage provided by each of these two phenomena, party and incumbency, obviously demand careful, sustained attention.

⁴⁷ The most notable exponent of such an interpretation is Walter Dean Burnham. See his "The Changing Shape of the American Political Universe," *American Political Science Review*, 59 (March, 1965), 7-28, and his more recent and comprehensive *Critical Elections and the Mainsprings of American Politics* (New York: W. W. Norton, 1970). The concept of a "critical election" was first given academic sanction in V. O. Key, Jr., "A Theory of Critical Elections," *Journal of Politics*, 17 (February, 1955), 3-18. Also pertinent are E. E. Schattschneider, *The Semi-Sovereign People* (New York: Holt, Rinehart, and Winston, 1960); Gerald M. Pomper, *Elections in America* (New York: Dodd, Mead and Company, 1968); *The American Party Systems*, ed. William Nisbet Chambers and Walter Dean Burnham (New York: Oxford University Press, 1967); among others.

⁴⁸ See, e.g., Burnham, *Critical Elections*. Erikson also sees the mid-1960s as significant. See his "Malapportionment, Gerrymandering, and Party Fortunes in Congressional Elections," at pp. 1240 ff.

The Paradox of Vote Trading*

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The conventional judgment on vote trading in legislatures is one of severe disapproval. In the United States, at least, the usual idiom is "logrolling" and this word has always had pejorative connotations.¹ The commonsense of the language has been reinforced by the judgment of scholars. Schattschneider, whose study of the writing of the Smoot-Hawley Tariff of 1930 is the most detailed examination of a single event of vote trading, concluded his book with the remark: "To manage pressures [i.e., for vote trading] is to govern; to let pressures run wild is to abdicate."² Furthermore, it seems likely that the widespread scholarly support for concentrated national leadership—as embodied, for example, in such writings as the report of the Committee on Political Parties of the American Political Science Association, a report to which Schattschneider contributed substan-

tially—was engendered in part at least by a desire to minimize vote trading.³

Despite the long-standing unanimity of scholarly and popular judgment, however, it has recently been argued by a number of scholars that vote trading is socially desirable because it allows the expression of degrees of intensity of preference. Buchanan and Tullock argue that the voter or legislator can improve his welfare "if he accepts a decision contrary to his desire in an area where his preferences are weak in exchange for a decision in his favor in an area where his feelings are stronger." Hence they say: "Bargains among voters can, therefore, be mutually beneficial."⁴ The authors recognize that, when bargains are concluded "in which the single voter does not participate, . . . he will have to bear part of the costs of action taken."⁵ But they nevertheless conclude that these external costs are on the average less than the benefits obtained so that for the society as a whole vote trading or logrolling is a desirable kind of event.

Following this argument there has developed a small literature on the subject that interprets the market for votes as if it were a market for goods. Tullock has elaborated his earlier position.⁶ Coleman has argued that logrolling is a device both to avoid Arrow paradoxes and to arrive at optimal allocations.⁷ A number of writers have pointed out, however, that vote trading does not eliminate these paradoxes.⁸ But these same writers

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¹ *A Dictionary of Americanisms* (1951), which is probably the most authoritative collection, cites many political examples from 1812 (Ninian Edwards) to 1949 (*Time* magazine), in every one of which the word is clearly used in a pejorative sense. In the quotation from Edwards, logrolling is specifically identified with intrigue; and the climate of intrigue is conveyed by almost all the later quotations. The Oxford Dictionary (i.e., *A New English Dictionary on Historical Principles*) cites political usage from both sides of the Atlantic from 1823 (*Niles Weekly*) on to the end of the century, and its quotations also are uniformly pejorative. Bartlett's *Dictionary of Americanisms* (1859) contains a lengthy and righteously indignant explication of the political sense of "logrolling." None of these sources indicate any pejorative connotation at all for the word when it refers to clearing land or building houses. It must have acquired the sense of intrigue, therefore, only when it was transferred from the bartering of labor to the bartering of votes. And this demonstrates that the bartering of votes has regularly been socially disapproved.

² E. E. Schattschneider, *Politics, Pressures and the Tariff* (New Jersey: Prentice-Hall, 1935), p. 293. Schattschneider interpreted the process of tariff writing as an "attempt to set up a beneficently discriminatory set of privileges"—beneficent in the sense that a few particular industries deemed worthy would benefit. This attempt failed, he argued, because the legislation as actually written protected so "indiscriminately . . . as to destroy the logic and sense of the policy" (p. 283). One of the purposes of this essay is to explain the conditions for such failure.

³ Committee on Political Parties of the American Political Science Association, *Toward a More Responsible Two-Party System* (New York: Rinehart, 1950).

⁴ James Buchanan and Gordon Tullock, *The Calculus of Consent* (Ann Arbor: University of Michigan Press, 1962), p. 145.

⁵ *Ibid.*

⁶ Gordon Tullock, "A Simple Algebraic Logrolling Model," *American Economic Review*, vol. 60 (June, 1970), pp. 419-26.

⁷ James Coleman, "The Possibility of a Social Welfare Function," *American Economic Review*, vol. 56 (December, 1966), 1105-22.

⁸ Dennis C. Mueller, "The Possibility of a Social Welfare Function: Comment," *American Economic Review*, vol. 57 (December, 1957), 1304-11; and, in an especially lucid and compelling argument, Robert Wilson, "An Axiomatic Model of Logrolling," *American Economic Review*, vol. 59 (June, 1969), 331-41, shows that, except in one unlikely circumstance, logroll-

and others have taken pains to argue that it does improve allocations in the direction of Pareto optimality.⁹

We have thus an intellectual confrontation between an older (and popular) tradition and new developments in social science. In this essay, we intend to reconcile the conflict. We will show (1) that logrolling itself (as if in a legislative market for votes) does in fact improve the position of the traders; but (2) we will show also that this very improvement generates external costs for the non-traders—costs that may be—and probably sometimes are—of such magnitude that they generate a paradox of vote trading. This paradox has the property that, while each trade is individually advantageous to the traders, the sum of the trades is disadvantageous to everybody, including the traders themselves. Furthermore, since trading is individually rational, this paradox is inescapable, given an appropriate distribution of taste. Even if the original trades are not considered binding so that traders can try to extricate themselves from the paradox, they may make new trades which result in an indefinite cycle of return to the original position, return to the paradox, etc. Nor can this lack of a stable equilibrium be compensated for by the formation of coalitions. So truly the paradox, once generated, is inescapable. In general, therefore, the market for legislative votes is quite different from the market for private goods and it is not wise to draw analogies from one market to the other.

A Model of Vote Trading

In order to analyze some of the essential features of vote trading, it is useful to establish an abstract legislature consisting of the set N of members, $N = \{1, 2, \dots, n\}$. This legislature deals with motions, x, y, \dots , which are proposals to impose commands. The set of motions, A , is indefinitely large, $A = \{x, y, \dots\}$. If a motion, x , is adopted, then an associated outcome, X , exists, and X is the state of affairs in which the command in x is imposed. If, however, a motion, x , is rejected, then the associated outcome \bar{X} exists, and \bar{X} is similar to the status quo ante except that it contains the memory of the decision not to impose the command in x . Hence, associated with the set of motions, A , is a set of possible outcomes, O , which is twice the size of A and is also indefinitely large, $O = \{X, \bar{X}, Y, \bar{Y}, \dots\}$.

ing necessarily involves a violation of Arrow's condition of independence from irrelevant alternatives.

⁹ See Wilson, *op. cit.*; Edwin T. Haeffle, "Coalitions, Minority Representation, and Vote-Trading Probabilities," *Public Choice*, vol. 8 (Spring, 1970), 75-90; and Dennis C. Mueller, Geoffrey C. Philpotts, and Jaroslav Vanek, "The Social Gains from Exchanging Votes: A Simulation Approach," *Public Choice*, vol. 13 (Fall, 1972), 55-79.

In this abstract legislature, motions are made and voted upon serially, so that future motions are not necessarily anticipated when current motions are considered. Note that this assumption about the system is markedly different from the assumption made in the essays cited in notes 8 and 9, where it is typically laid down that all motions are simultaneously before the legislature. In such a system support can be traded back and forth in all possible ways, but in the present system support can be traded only with respect to the subset of A currently before the legislature or anticipated to come before it. The rationale for this more restrictive assumption is simply that it accords with a crucial feature of the real world. In the third section, however, we remove this restriction only to find that it makes no theoretical difference, although it may make a practical difference.

The legislators produce outcomes from motions by voting, where votes are defined thus:

$v_i(x) = 1$ is a vote cast yea by member i on motion x
 $v_i(x) = -1$ is a vote cast nay
 $v_i(x) = 0$ is a vote not cast (i.e., an abstention)

The rule connecting motions to outcomes is the usual one of simple majorities:

if $\sum_{i=1}^n v_i(x) > 0$, then X exists

if $\sum_{i=1}^n v_i(x) \leq 0$, then \bar{X} exists.

The members of N order the elements of the set O by a relation, R , which is connected, transitive, and reflexive.¹⁰ The relation, R , may be thought of as a combination of the relation of preference, P , and of indifference, I . The member, i , who holds the relation is, if necessary, identified by a subscript, R_i . The entire set O may be ordered by R and from that ordering cardinal utility numbers, $u_i(X)$, can be derived. Finally, it is assumed that the people in N are rational in the sense that they act to maximize utility or, if appropriate, expected utility.

It is important to note that utility is defined over outcomes, which are the goals themselves, not over motions, which are merely the means to the goals. While it is probably the case that most people care about the alternative goals rather than the means, the use of goals in the definition is not

¹⁰ To say R is connected means that either XRY or YRX is true; to say R is transitive means it is true that if XRY and YRZ , then XRZ ; to say R is reflexive means that it is true that XXR .

the consequence of a passion for realism. Instead utility is defined over goals in order to allow for voting against motions that lead to the achievement of one's immediate interest—which is the essence of logrolling. Suppose utility were in fact defined over motions, i.e., $u_i(x)$, rather than $u_i(X)$. Then if $u_i(x) > u_i(\tilde{x})$, it would necessarily and invariably be that, among rational actors, $v_i(x) = 1$. That is, if the goal itself were the motion, then one would always have to vote in accordance with one's true tastes. But if the motions are merely means, then it is conceivable that, even though $u_i(X) > u_i(\tilde{X})$, still $v_i(x) = -1$ because member i is achieving some other goal by devious means. In short, defining utility over outcomes rather than motions allows for the possibility of devious behavior such as is often involved in vote trading.

Without, we hope, a loss of generality, it is from now on assumed that all members of N are concerned about motions and that, as between X and \tilde{X} , either $XP_i\tilde{X}$ or $\tilde{X}P_iX$. There is not much loss of realism in this assumption if we think of real legislatures as varying in size from decision to decision according to the number of legislators who take part in them.

There is a set $S(x)$ and its complement $-S(x)$ which are pairwise disjoint subsets of N so that $S(x) \cup -S(x) = N$ and $S(x) \cap -S(x) = \emptyset$; where \emptyset is the empty set. $S(x)$ has s members and $-S(x)$ has $(n-s)$ members; and $n < 2s$ so that $S(x)$ is always larger than $-S(x)$. The defining property of $S(x)$ is that all the members of S have the same preference between X and \tilde{X} and all the members of $-S(x)$ have the opposite preference. That is, if $i, i \in S(x)$, has $XP_i\tilde{X}$, then $j, j \neq i$ and $j \in S(x)$, also has $XP_j\tilde{X}$ and $k, k \in -S(x)$, has $\tilde{X}P_kX$; while, if i has $\tilde{X}P_iX$, then j has $\tilde{X}P_jX$ and k has $XP_k\tilde{X}$. Thus $S(x)$ and $-S(x)$ may be thought of as the majority and minority that would exist on motion x if everybody who preferred X to \tilde{X} voted yea on x and everybody who preferred \tilde{X} to X voted nay on x .

There is in addition a set $T(x)$ and its complement $-T(x)$ which are also pairwise disjoint subsets of N so that $T(x) \cup -T(x) = N$ and $T(x) \cap -T(x) = \emptyset$. $T(x)$ has t members and $-T(x)$ has $n-t$ members; and, again, $n < 2t$ so that $T(x)$ is always larger than $-T(x)$. The defining property of $T(x)$ is that all members of T vote the same way on x and all members of $-T$ vote the opposite way. That is, if $v_i(x) = 1$, then for all $j, j \neq i, j \in T(x)$, $v_j(x) = 1$ and for all $k, k \in -T(x)$, $v_k(x) = -1$, while if $v_i(x) = -1$, then $v_j(x) = -1$ and $v_k(x) = 1$. Thus, $T(x)$ and $-T(x)$ are the actual majority and minority on a vote on motion x .

If a voter i always voted for x when $XP_i\tilde{X}$ and against x when $\tilde{X}P_iX$, then $S(x)$ and $T(x)$ would be identical. This is what is known as "voting in accord with one's true tastes" or "sincere" voting.

It might just as reasonably be called naive.¹¹ Regardless of name, sincere or naive voting is to be contrasted with sophisticated voting, which is defined as voting against one's immediate interest when there is an incentive to do so. That is, if $u_i(Y) > u_i(X)$ and $u_i(X) > u_i(\tilde{X})$ and if i can achieve Y by voting against x , then $v_i(x) = -1$ is sophisticated voting. There are several reasons why one might choose to do so (e.g., to exploit the fact of a voting paradox), some of which are analyzed by Black and Farquharson. Surely the main reason for sophisticated voting is logrolling and this is what will be analyzed here.

To vote sophisticatedly is to switch sides between $S(x)$ and $T(x)$. If one is in $S(x)$ and orders $XP_i\tilde{X}$, to switch is to resign from the set $S(x)$ and join $-S(x)$ where one acts as if one orders $\tilde{X}P_iX$. Hence for some i_0 , who is a particular member of $S(x)$, switching is:

$$\begin{aligned} &S(x) - \{i_0\} \\ &-S(x) + \{i_0\}. \end{aligned}$$

Of course, i_0 may be any number of persons, but for convenience the symbol is treated as if it represented a single person.

Under what conditions might switching be expected to occur? Surely one necessary condition is that the switcher belong to $S(x)$. The motive for switching is to bring about a result different from that which would occur if voting were sincere. Assume that some i_0 switches from $-S(x)$ to $S(x)$, then the result is unchanged. The preferred outcome of the members of $S(x)$ was the winner before the switch and remains so after it. If, however, i_0 (hereafter identified simply as " i ") without the particularizing subscript) switches from $S(x)$ to $-S(x)$, it may be that the preferred outcome of the members of $S(x)$ loses. Hence the only chance that a member has to change the outcome is when he belongs to the initial majority or to the majority under sincere voting. The first condition for switching is, for the switcher, i , that:

$$i \in S(x). \quad (1)$$

Elaborating the same notion, the switcher actually changes the outcome only if he is a pivot, only if, that is, his switching renders the initial winner a loser and the initial loser a winner. This condition can be expressed in a stronger way: In order to justify switching, the switcher must be

¹¹ Duncan Black, *The Theory of Committees and Elections* (Cambridge, Cambridge University Press, 1958), p. 44, uses the former phrase and Robin Farquharson, *Theory of Voting* (New Haven, Yale, 1970), pp. 17-18, uses the latter. Voting in accord with one's true tastes (defined over motions) seems "sincere." But if utility is defined over outcomes, it seems merely naive to confuse means and ends.

compensated by a specifiable amount. (If he switches without any compensation for his act of giving up his position on the side he himself favors, his behavior must be regarded as truly irrational and self-destructive.) Since, for the switcher, $u_i(X) > u_i(\bar{X})$ while he votes $v_i(x) = -1$ or $u_i(\bar{X}) > u_i(X)$ while he votes $v_i(x) = 1$, his vote costs him the absolute value of $|u_i(X) - u_i(\bar{X})|$. To induce him to bear that cost, there must be available in the system an amount b that is larger than the cost:

$$b > |u_i(X) - u_i(\bar{X})|.$$

The only way b can come into existence is that some event happen by the switcher's action to generate b . If nothing happens by the switch, that is, if the preference of the members of $S(x)$ is the preference of the members of $T(x)$, then no change occurs and b is not generated. Since no one gains, no one has a motive to compensate the switcher for switching. So, if the switcher is to be paid, the outcome must be changed, i.e., the switcher must be the pivot. Hence we have the second condition:

$$S(x) - \{i\} = -T(x) \quad (2)$$

Having defined switching, we now define vote-trading, which is a barter system of switching. A trade between $i \in N$ and $j \in N$ occurs when i switches on x and j switches on y in such a way that, though i loses from his switch, he gains from j 's, and, though j loses from his switch, he gains from i 's. The conditions for a trade are now specified.

Let $i, i \in S(x)$, order $XP_i\bar{X}$. Let i switch (i.e., $-S(x) + \{i\} = T(x)$), so that the members of $T(x)$ bring about X as the chosen state of affairs. Of course, i loses something thereby; and, in order to compensate i , someone else must gain. Let the gainer be $j, j \in -S(x)$, so that $\bar{X}P_jX$. By reason of i 's switch, j has obtained his preferred state of affairs despite his bleak initial expectations. Thus, i has a loss, j has a gain, and j in a sense owes i a debt.

If this were a system of exchange in which participants paid for services with some numeraire, like money or jobs, then j might simply pay i at least the amount b . But since in fact it is a system of barter, j must pay i with a switched vote. For j to switch on motion y , it must be by (1) that j belongs to $S(y)$. Let j order $YP_j\bar{Y}$ so that by his switch he brings about \bar{Y} , which is what is desired by $i, i \in -S(y)$, who orders $\bar{Y}P_iY$. Thereby, i is compensated for his previous loss and j has discharged his debt on motion x by taking a loss on motion y . Summarizing, the first condition of vote trading then is:

$$\begin{aligned} i &\in S(x), & j &\in -S(x) \\ i &\in -S(y), & j &\in S(y) \end{aligned} \quad (3)$$

Simple as this condition is, it tells us quite a bit about the actual occurrence of logrolling in real legislatures. It is apparent that, for a trade to be possible, the traders must be on opposite sides on two motions. One trader must be in the initial majority on the first motion and the initial minority on the second. The other trader must be in the initial minority on the first motion and the initial majority on the second. If this condition cannot be met, then trading is not possible.

One obvious place it cannot be met is in legislatures with just two disciplined parties. To say a party is disciplined is to say that all its members are on the same side on any motion. Hence if the members of a party are a majority on one motion, they are a majority on all motions. For the condition to be met, some of those people must sometimes be in the minority, but they cannot by reason of the fact of discipline.

Consequently, in a legislature with just two disciplined parties, logrolling is extremely rare. This is probably one reason so many American writers from Woodrow Wilson onward have admired the British Parliament, especially when it has approached the two-party situation in which statement (3) cannot be satisfied. They should have also admired those state legislatures like New York and Pennsylvania which party bosses have usually run with an iron hand because logrolling is equally impossible in them. But it has never been fashionable to admire boss rule.

Of course, when vote trading is banished from the legislature, political compromise goes on someplace else politically antecedent to the legislature. Thus in state legislatures and city councils with disciplined parties, it is in the majority caucus or in the mind of the boss that the compromise takes place. In England, the Cabinet serves as one place of compromise and very probably something like vote trading goes on there. Since the Cabinet situation is unstructured in comparison with the Parliamentary situation, however, it is probably hard to identify the trades and compromises that do occur.

To approach the second condition for vote trading observe that, in the running example where $i, i \in S(x), i \in -S(y)$, orders $XP_i\bar{X}$ and $\bar{Y}P_iY$ while $j, j \in -S(x), j \in S(y)$, orders $\bar{X}P_jX$ and $YP_j\bar{Y}$, it is the case that i gives up winning on x in order to win on y . Assuming that the trader is rational in the sense that he seeks to maximize utility, then he gives up on x only for a utilitarian reason. It must be, therefore, that, if he trades, the trader finds the joint utility of losing on x and winning on y greater than the joint utility of winning on x and losing on y .

To express this a little more formally, one can write of the utility of a trader's preferred position on motion x , $u_i(P_x)$, and of his not preferred position, $u_i(\bar{P}_x)$. By statement (3) a pair of traders initially, without trading, obtain:

$$\begin{aligned} &\text{for } i, u_i(P_x) + u_i(\bar{P}_y) \\ &\text{for } j, u_j(\bar{P}_x) + u_j(P_y), \end{aligned}$$

where i initially belongs to $S(x)$ and $-S(y)$ and j initially belongs to $-S(x)$ and $S(y)$. By trading, however, they can obtain:

$$\begin{aligned} &\text{for } i, u_i(\bar{P}_x) + u_i(P_y) \\ &\text{for } j, u_j(P_x) + u_j(\bar{P}_y). \end{aligned}$$

For the trade to occur, it must be that the latter are, respectively, greater than the former:

$$\begin{aligned} u_i(\bar{P}_x) + u_i(P_y) &> u_i(P_x) + u_i(\bar{P}_y) \\ u_j(P_x) + u_j(\bar{P}_y) &> u_j(\bar{P}_x) + u_j(P_y) \end{aligned}$$

or, rearranging:

$$\begin{aligned} u_i(P_y) - u_i(\bar{P}_y) &> u_i(P_x) - u_i(\bar{P}_x) \\ u_j(P_x) - u_j(\bar{P}_x) &> u_j(P_y) - u_j(\bar{P}_y). \end{aligned} \quad (4)$$

If we define the salience, s , to a member, i , of a motion, x , as the absolute difference between his utility for his preferred position and the utility for his not preferred position, that is,

$$s_i(x) = |u_i(P_x) - u_i(\bar{P}_x)|$$

then we can express statement (4) easily in words: for a trader to trade, his salience must be higher on the motion on which his partner switches than it is on the motion on which he switches.

In Tables 1 and 2 is presented an instance of a three-member committee in which vote trading is possible between members 2 and 3. In that example, the arrangements in the first column are made to satisfy statement (3) that member 2 belongs to

$S(x)$ and $-S(y)$ and member 3 belongs to $-S(x)$ and $S(y)$. Ignoring for the moment member 1's utilities (except as his preference is necessary to provide an initial majority), we construct the tables with cardinal utility numbers satisfying both on the one hand a set of preferences ($XP_2\bar{X}$, $\bar{Y}P_2Y$, $\bar{X}P_3X$, $YP_3\bar{Y}$) and on the other hand the utility requirements of statement (4). Translating statement (4) into the example of Tables 1 and 2, one gets

$$\begin{aligned} u_2(\bar{Y}) - u_2(Y) &> u_2(\bar{X}) - u_2(X) \\ 3 - (-3) &\quad 1 - (-1) \end{aligned}$$

$$\begin{aligned} u_3(\bar{X}) - u_3(X) &> u_3(Y) - u_3(\bar{Y}). \\ 2 - (-4) &\quad 1 - (0) \end{aligned}$$

For comparison, in Tables 3 and 4 is presented an example that satisfies statement (3) but not statement (4). Again translating, one gets

$$\begin{aligned} u_2(\bar{Z}) - u_2(Z) &= u_2(W) - u_2(\bar{W}) \\ 1 - (-1) &\quad 1 - (-1) \end{aligned}$$

$$\begin{aligned} u_3(\bar{W}) - u_3(W) &= u_3(Z) - u_3(\bar{Z}). \\ 1 - (-1) &\quad 1 - (-1) \end{aligned}$$

And because these are equalities rather than inequalities, statement (4) is not satisfied. Presumably, 2 and 3 could not trade on motions w and z .

From these examples one can readily see the advantages of vote trading and easily understand why the notion of logrolling has been so attractive to scholars. There may be a clear gain from trade for both parties. In the example in Tables 1 and 2, member 2 gains four units of utility and member 3 gains five units.

Nevertheless there are some severe limitations. The possibilities of trading are quite restrictedly

Table 1				Table 2			
Motion x				Motion y			
	Preferred Outcome	$u(X)$	$u(\bar{X})$		Preferred Outcome	$u(Y)$	$u(\bar{Y})$
Member 1	X	—	—	Member 1	Y	—	—
Member 2	X	1	-1	Member 2	\bar{Y}	-3	3
Member 3	\bar{X}	-4	2	Member 3	Y	1	0

Table 3				Table 4			
Motion w				Motion z			
	Preferred Outcome	$u(W)$	$u(\bar{W})$		Preferred Outcome	$u(Z)$	$u(\bar{Z})$
Member 1	W	—	—	Member 1	Z	—	—
Member 2	W	1	-1	Member 2	\bar{Z}	-1	1
Member 3	\bar{W}	-1	1	Member 3	Z	1	-1

finite. By reason of statement (2), a switch is pointless unless the switcher is a pivot with respect to the initial distribution of preference. Consequently, we have a maximum limit on logrolling: Since it takes two pivotal members to trade, there can at most be as many trades as half the number of issues. In a temporal world, moreover, only a finite number of issues can be juggled at once. In a session of a typical national legislature, there are probably no more than, say, two thousand motions, which gives a theoretical maximum of one thousand trades. But of course, many issues are trivial so that no trade is worthwhile; many others come up for decision so quickly that the elaborate arrangements of logrolling are not possible; for many others it may be difficult for a member to discover another one with whom statement (4) can be satisfied; and, finally, since motions come up serially so that many future motions cannot be anticipated and so that past motions are irrevocably settled, only a small subset of motions is available for trading at any one moment. One suspects that the practical maximum of trades is at most one-tenth of the theoretical maximum. Consequently, in the typical legislature (with, say, one hundred members) in the typical session it may well be that the average member can expect to be involved in at most one trade.

Furthermore, while some members may get enormous gains from trade, such enormous gains may be rare. In this sense vote trading, subject as it is to severe temporal restrictions, is quite different from commodity trading which can go on forever. So while it is true that gains can be made from trade, it is doubtful that the size and frequency of such gains is as great as the exponents of logrolling have imagined. Nevertheless, so long as one considers only the utilities of the traders themselves, trading is, for certain, both individually and socially desirable.

The Paradox of Vote Trading

Unfortunately, even this moderated enthusiasm for logrolling may be misplaced because the admitted gains from trade may be wiped out by external costs. Indeed, it is quite possible to imagine a system in which everyone gains from individual trades—and so has a positive motive to logroll—and yet everyone also loses if all such trades are carried out. This is the paradox of logrolling, to the illustration of which we now turn.

External costs are those which one bears as a result of the actions of others. They are to be contrasted with internal costs which result from one's own action. The noise that comes from the pneumatic drill is an internal cost with respect to the driller because it is part of what he must suffer to earn his pay. But to the neighbor at the construction site, the noise is an external cost. He must

bear it not in order to get something else, but merely because he is there.

Debate is never ending about the existence of some external costs. (For example, does the noise of the drill really bother the neighbor, or does he see a chance, by complaining of noise, even though it does not bother him, to blackmail the contractor?) But it seems impossible to doubt the existence of external costs in vote trading. Consider the position of some member who is not a party to a trade: Since the trade brings about a different winner on a motion, the nontrader bears an external cost if he was originally in the majority or he receives an external gain if he was in the original minority. Thus, if members i and j trade on motions x and y , then if some other member k belongs to $S(x)$ and $S(y)$, k is forced by their action to belong to $-T(x)$ and $-T(y)$. Thus k suffers a loss of $|u(P_x) - u(\bar{P}_x)| + |u(P_y) - u(\bar{P}_y)|$ —all as a consequence of no action of his own. Indeed, member k is completely passive throughout the entire transaction, but his outcomes are reversed.

External costs have generally been ignored in the writings of those who extol the benefits of vote trading.¹² Yet they must by the nature of the case, i.e., by statement (2) on pivoting, be present. Since trading changes the outcomes, there must always be innocent bystanders who lose. Although the size of their losses depends on the salience of particular outcomes in particular cases, it is possible to show that in some particular cases suffering is general and universal, which is the paradox of logrolling.

Table 5 depicts a system of trades for a three member legislature: between members 2 and 3 on motions x and y , between members 1 and 2 on motions w and z , and between members 1 and 3 on motions t and v . (This is not the only possible set of trades—one might have members 2 and 3 trade on x and w , 1 and 2 on t and y , and 1 and 3 on v and z , and other less exhaustive combinations—but all other complete sets of trades share the paradoxical feature displayed for the trades in Table 5. We assume, however, that once a set of trades on particular issues is made, new trades cannot be made on these issues. This means that a trader may not cancel an old trade with a new one simply because he subsequently discovers that what was thought to be advantageous is no longer

¹² An exception is Coleman, p. 1121, where it is noted in passing that the inclusion of such costs requires a "much more extensive calculation" than offered in that essay. More recently Coleman has provided an explicit calculation that leads to the conclusion that vote trading is profitable only under conditions of "absolutely free and frictionless political exchange" that preclude arbitrary decision rules. James S. Coleman, "Beyond Pareto Optimality," in *Philosophy, Science, and Method*, ed. Sidney Morgenbesser, Patrick Suppes, and Morton White (New York, St. Martin's, 1969), pp. 415-39.

Table 5. Utilities of Members for Outcome of Voting

	Preferred Outcome Col 1	Motion x		Preferred Outcome Col 4	Motion y	
		$u_i(X)$	$u_i(\bar{X})$		$u_i(Y)$	$u_i(\bar{Y})$
		Col 2	Col 3		Col 5	Col 6
Member 1*	X	1	-2	Y	1	-2
Member 2	X	1	-1	\bar{Y}	-2	2
Member 3	\bar{X}	-2	2	Y	1	-1
		Motion w			Motion z	
		$u_i(W)$	$u_i(\bar{W})$		$u_i(Z)$	$U_i(\bar{Z})$
Member 1	W	1	-1	Z	-2	2
Member 2	\bar{W}	-2	2	Z	1	-1
Member 3*	W	1	-2	Z	1	-2
		Motion t			Motion v	
		$u_i(T)$	$u_i(\bar{T})$		$u_i(V)$	$u_i(\bar{V})$
Member 1	\bar{T}	-2	2	V	1	-1
Member 2*	T	1	-2	\bar{V}	1	-2
Member 3	T	1	-1	\bar{V}	-2	2

* Nontrader on pair of motions.

so. Such an assumption is realistic because most legislatures provide ways to render past votes irrevocable, which means completed or partially completed bargains cannot be undone. In the next section, however, we drop even this assumption and show that the paradox persists even with unrestricted trading.) We have worked out examples similar to those of Table 5 for a five-member legislature, but they are too lengthy for inclusion here. Doubtless similar examples could be worked out for legislatures of any size. The three-member example can, of course, be embedded in any legislature; and, of course, the three members can be three factions.

It is quickly apparent that, if no trading occurs, the members neither gain nor lose from the passage of the six motions imagined for Table 5. The calculation is set forth in Table 6, where the several columns are a simple reproduction of columns 3 and 5 of Table 5—since in that Table all motions pass under sincere or naive voting. In Table 6 in the summary column on the extreme right, each member earns a net utility of zero as a result of the six votes.

Suppose, now, that the members, starting from

this base point of zero, calculate what they may expect to make from the trades in which they can engage. Each member sees that he can make two trades and increase his utility thereby. For the third pair of motions he can do nothing and might reasonably expect, though his expectation is in fact unrealizable, that he will receive his nontrading outcome on these motions. His expectations are, therefore, set forth in Table 7. If the member so calculates, it is clearly advantageous to trade whenever possible. When each does so, each generates a clear gain of four units of utility. From the point of view of the member, by taking these actions he increases his expected utility—which is the essence of rational behavior.

Unfortunately, however, if each member behaves rationally by making the trades possible for him, all the members suffer. They are, in fact, worse off than if they had voted sincerely or naively. The calculation is set forth in Table 8, where it is shown that each member receives a negative two units when all trades are made and external costs are taken into the reckoning.

This is the paradox of logrolling: that rational trades by all members make everyone worse off.

Table 6. Utilities of Outcomes Without Trades (Sincere Voting)

Members	Outcomes						Σu_i
	$u_i(X)$	$u_i(Y)$	$u_i(W)$	$u_i(Z)$	$u_i(T)$	$u_i(V)$	
1	1	1	1	-2	-2	1	0
2	1	-2	-2	1	1	1	0
3	-2	1	1	1	1	-2	0

Table 7. Utilities of Outcomes with Trades When Possible and without Anticipating External Costs

Members	Outcomes						Σu_i
	$u_i(X)$	$u_i(Y)$	$u_i(W)$	$u_i(Z)$	$u_i(T)$	$u_i(V)$	
1	1	1	-1	2	2	-1	4
2	-1	2	2	-1	1	1	4
3	2	-1	1	1	-1	2	4

Table 8. Utilities of Outcomes with Trades and with External Costs

Members	Outcomes						Σu_i
	$u_i(X)$	$u_i(Y)$	$u_i(W)$	$u_i(Z)$	$u_i(T)$	$u_i(V)$	
1	-2	-2	-1	2	2	-1	-2
2	-1	2	2	-1	-2	-2	-2
3	2	-1	-2	-2	-1	2	-2

It is not easy to extricate oneself from this paradox. For example, it might be said that if members were foresighted they would refuse to make trades at all, lest the trades lead to the realization of the paradox. Self-restraint is not so easy to achieve, however. Suppose each member makes no trade himself but anticipates a trade among the pair that does not include him. That is, he himself behaves with self-restraint; but, with a conservative appreciation of what others can do to harm him, he anticipates the worst from them. This expectation, which is analogous to the notion of a security level in the theory of games, is calculated for each of the members in Table 9. It is immediately apparent that this is the worst that can happen to any member, for each loses six units of utility.

If each member perceives that his security level (Table 9) without trading is -6, then the -2 from systematic trading (Table 8) looks pretty attractive. It looks even more attractive if one suspects for a moment that the members of the pair from which one is excluded will in fact trade. Then one's only protection is to trade oneself as much as possible. This then guarantees a minimum loss of -2.

Conceivably the members might escape the paradox by agreeing to vote sincerely. But, if any

member suspects that others may not abide by their promises, then the best strategy is to trade oneself. In this way it is rational to trade not only to try to achieve the maximally good results (of Table 7) but also to avoid the minimal results (of Table 9) by arriving at modest losses (in Table 8). Thus, each member must trade in order to behave rationally; but then, perversely, each member comes to a worse end than if he abstained from trades and always voted sincerely or naively. The paradox is inescapable—in fact it is a kind of n -person Prisoners' Dilemma as depicted in Table 10.

Some Conditions of the Paradox

This paradoxical result is not an accidental consequence of the particular cardinal utilities nor of the particular choices of trades to be made. Rather the paradox persists even if one uses ordinal utility and allows all possible trades. To show this we now translate our discussion into ordinal utility and develop an appropriate example.

In the example of Table 11, the six motions now no longer have cardinal utilities associated with them. Instead, reading across each row are the six initial majority (i.e., " $S(x)$ ") and minority (i.e., " $-S(x)$ ") sets to which each member belongs

Table 9. Security Levels when No Trades Occur but Each Member Anticipates External Costs

Members	Outcomes						Σu_i
	$u_i(X)$	$u_i(Y)$	$u_i(W)$	$u_i(Z)$	$u_i(T)$	$u_i(V)$	
1	-2	-2	1	-2	-2	1	-6
2	1	-2	-2	1	-2	-2	-6
3	-2	1	-2	-2	1	-2	-6

Table 10.

The Paradox of Vote Trading as a Three-Person Prisoners' Dilemma			
Member 3		Member 2	
Alternative 1 Not Trade		Alternative 1 Not Trade	Alternative 2 Trade
Member 1	Alternative 1 Not Trade	0, 0, 0	0, 0, 0
	Alternative 2 Trade	0, 0, 0	2, 2, -6
Member 3		Member 2	
Alternative 2 Trade		Alternative 1 Not Trade	Alternative 2 Trade
Member 1	Alternative 1 Not Trade	0, 0, 0	-6, 2, 2
	Alternative 2 Trade	2, -6, 2	-2, -2, -2

Note: In cell (1, 1, 1) the entry is "0, 0, 0" because no one trades. In cells (1, 2, 1), (2, 1, 1), and (1, 1, 2) the entries are also "0, 0, 0" because, if any pair consistently vote sincerely, then all three must do so. In cells (2, 2, 1), (1, 2, 2), and (2, 1, 2) the entries are appropriate combinations of 2 units for the traders and -6 units for the non-trader. In these cases, the -6 units are as calculated in Table 9, while the 2 units result from receiving, first, the payoff to sincere voting (a) when one is not a trader (1+1) and (b) when one's potential trade is with a sincere voter (-2+1); and second, the payoff to sophisticated voting when one's partner is a sophisticated voter (-1+2). In cell (2, 2, 2), the entry (-2, -2, -2) derives from Table 8. This is strictly a Prisoners' Dilemma because, while the outcome (in cell (1, 1, 1)) from the strategy of not trading is preferred by all three players to the outcome (in cell (2, 2, 2)) from the strategy of trading, still the strategy of trading dominates for all three players and the not preferred outcome must inevitably occur among rational players of a non-cooperative game.

when he expresses his true tastes. The sets are arranged from left to right according to the salience (i.e., $s_i(x) = |u_i(P_x) - u_k(P_x)|$) of the motion to the member, with the most salient motion on the left and the least salient motion on the right. For member 1, for example, victory for his position on x is ordinally most valuable, victory on y second most valuable, . . . , and victory on v least valuable. That is, $s_1(x) > s_1(y) > \dots > s_1(v)$. Recalling the second condition of vote trading (statement (4) that "for a trader to trade his salience must be higher on the motion on which his partner switches than it is on the motion on which he switches"), one can easily find trades for each pair of members. Thus member 1 can trade with member 3 on motions t and v . That is, member 1 votes against his interest on his least salient motion, v , and gains thereby from member 3's switch on 1's third most salient motion, t . Similarly, member 3 votes against his interest on his next to least salient motion, t , and gains thereby on his motion of next higher salience, v . These trades are indicated in Table 11 by lines connecting the positions of a member for each trade in which he may be engaged.

After the trades indicated in Table 11 have been made, it can be rewritten as Table 12 in terms of the resultant majorities (i.e., " $T(x)$ ") and minorities (i.e., " $-T(x)$ ") where " $T_d(x)$ " means that the member is on the winning side only because he has voted sophisticatedly and in spite of the fact that he disagrees (" d ") with it.¹³

It is easy to see that this is a paradoxical result. Each member loses a majority position on his two most salient motions, and incidentally obtains a majority position contrary to his true tastes on his least salient motions. For thus losing on his two most and his two least salient motions, he gains

¹³ We assume that the utility of a member for a majority position on a motion he disagrees with, ($T_d(x)$), is the same for him as the utility of a minority position on that same motion, ($-T(x)$), because in both cases his sincere position differs from the prevailing one.

Table 11.

Members	High Salience		Low Salience				Trades	Switches	
	Majority and Minority Positions of Members before Trades		Positions of Members before Trades						
1	$S(x)$	$S(y)$	$-S(t)$	$-S(z)$	$S(w)$	$S(v)$	with 3 with 2	to $T_d(v)$ to $T_d(w)$	—
2	$S(t)$	$S(v)$	$-S(w)$	$-S(y)$	$S(x)$	$S(z)$	with 1 with 3	to $T_d(z)$ to $T_d(x)$ oooo
3	$S(w)$	$S(z)$	$-S(x)$	$-S(v)$	$S(t)$	$S(y)$	with 2 with 1	to $T_d(y)$ to $T_d(t)$	oooo —

Table 12.

Member	High Salience				Low Salience	
	Majority and Minority Positions after Trades					
1	$-T(x)$	$-T(y)$	$T(t)$	$T(z)$	$T_d(w)$	$T_d(v)$
2	$-T(t)$	$-T(v)$	$T(w)$	$T(y)$	$T_d(x)$	$T_d(z)$
3	$-T(w)$	$-T(z)$	$T(x)$	$T(v)$	$T_d(t)$	$T_d(y)$

only on his mediumly salient motions. Thus, clearly, since the degree of utility where he worsens his position is greater than the degree of utility where he better his position, he is in sum hurt by the set of trades.

It might be thought that this result could be avoided by a different set of initial trades. But, referring back to Table 11, it is apparent that such trades are not possible. Member 1, for example, wants to gain on motions t and z by getting someone to switch on these motions. He cannot trade with member 2 on motion t , however, because this would force 2 to switch on his most salient motion in clear violation of statement (4). Therefore, member 1 *must* trade with member 3 on motion t as in Table 11. Similarly member 1 cannot trade with member 3 on motion z because this would force 3 to vote against his true tastes on a more salient motion (z) in order to gain his taste on a less salient one (x or v). Again, therefore, member 1 *must* trade with member 2 on motion z , exactly as in Table 11. By a similar argument, each of the members in this Table is constrained to exactly those trades included in it.

The example in Table 11 shows that the paradox is inherent in the ordinal relationships and is not dependent on particular cardinal utility numbers. It is not necessarily true, however, that the situation depicted in Table 12 is in equilibrium. If one translates the post trading agreements of Table 12 into a new round of trading one gets Table 13, which, if the indicated trades are made, takes the three members exactly where they were in the beginning in Table 11. Thus by voting sophisti-

actedly in the first round they get to a paradox from which they can extricate themselves by voting sincerely in the second round.

Voting that involves first a switch to a position contrary to one's true tastes and then a reverse switch back may seem more devious than sincere. But this is only the beginning. For after a reverse shift, when everybody assumes once again his original sincere positions, there is no reason to believe that pairs of members will not plunge themselves once again into a new round of trades which are individually rational. Indeed, the inexorable logic of individual rationality dictates that if a member is willing to doublecross an old trading partner in order to improve his position through another trade, he is willing to triplecross his new trading partner, etc. The consequence of all members' acting in this self-serving fashion is that trading continues indefinitely. The lack of a stable equilibrium that could break this cycle means that there is no guarantee that even the improved benefits to all, as before the initial or after the subsequent trades in our example, will halt the process. In practice, only when motions are actually voted upon will the process of vote trading be terminated.

Is there any way in which this cyclical paradox can be avoided? One member might refuse to trade; but, if he does so, he exposes himself (as we showed in the previous section) to external costs from the other pair's trade without any compensating gains from his own trade. Hence, as we previously noted, it is a kind of n -person Prisoners' Dilemma in which each member is forced by individual rationality to choose the socially and individually destructive alternative.

Another possible way to avoid the paradox is for a pair to form a coalition agreeing to trade only with each other. Thereby they might obtain the benefits of trade and avoid the external costs of others' trades. This solution was suggested by Dr. Edwin T. Haefle in a critique of the original version of this essay when it was presented at the

Table 13.

Member	High Salience		Low Salience				Trades	Switches	
	Majority and Minority Positions before Round 2 of Trades								
1	$-S(x)$	$-S(y)$	$S(t)$	$S(z)$	$S_d(w)$	$S_d(v)$	with 2 with 3	to $T(v)$ to $T(w)$	— ----
2	$-S(t)$	$-S(v)$	$S(w)$	$S(y)$	$S_d(x)$	$S_d(z)$	with 3 with 1	to $T(z)$ to $T(x)$	oooo —
3	$-S(w)$	$-S(z)$	$S(x)$	$S(v)$	$S_d(t)$	$S_d(y)$	with 1 with 2	to $T(y)$ to $T(t)$	---- oooo

1972 Annual Meeting of the American Political Science Association.¹⁴ Of course, this solution simply denies statement (3), the first condition for vote trading; but it is one way to cut the Gordian knot. It is comparable to cooperative solutions to the Prisoners' Dilemma where, with preplay communication allowed and criminals free to make binding agreements, they agree never to confess and thereby thwart the prosecutor. In the absence of enforceable agreements, however, there may be instabilities in such solutions simply because they are not individually rational. So it is here.

Referring again to Table 11, suppose members 2 and 3 coalesce on motions x and y . Member 1 is excluded, but he may propose that he and member 2 coalesce instead on motions z and w . Once this alternative is proposed, member 2 prefers the coalition with member 1 on z and w to the coalition with member 3 on x and y simply because he (2) gains more from an improvement of his position on his more salient motion, w , than he does from an improvement on his less salient motion, y , and he loses less from voting sophisticatedly on his least salient motion, z , than from so voting on his next-to-least salient motion, x . Thus member 2 prefers coalition (1, 2) to coalition (2, 3) and, of course, member 1—who is in the former but not the latter—also prefers coalition (1, 2) to coalition (2, 3). One can write then

$$(1, 2) D (2, 3)$$

where " D " means that the former coalition dominates the latter in that both members of the former prefer it to the latter.

By a similar argument it can easily be shown that coalition (2, 3) dominates coalition (1, 3). Suppose coalition (1, 3) forms over t and v . Then coalition (2, 3) involving x and y is preferred by member 3 because he (3) gains in coalition (2, 3) on x which is more salient for him than v and loses in coalition (2, 3) on y , which is less salient for him than t . Hence:

$$(2, 3) D (1, 3)$$

Finally, coalition (1, 3) which involves t and v is preferred to coalition (1, 2) which involves z and w , simply because t is of higher salience to member 1 than is z and v is of lower salience to him than is w . Hence:

$$(1, 3) D (1, 2)$$

Putting these statements together we obtain an intransitive result:

$$(1, 2) D (2, 3) D (1, 3) D (1, 2).$$

¹⁴ Edwin T. Haefle, "Paradox Lost" (Washington D.C., mimeographed, 1972).

As is well known, when domination is intransitive the system is unstable. There is always one member left out of the coalition and this member has a strong incentive to break up any existing coalition. Furthermore, he can always do so because it is always possible for him to offer an alternative coalition that dominates an existing one.

This fact suggests that the paradox of vote trading is closely related to the paradox of voting. In Table 14 we have stated the preferences of each member for the possible trades that might be made. Thus for member 1, the highest preference is for the trade on motions t and v with member 3 simply because t is the most salient motion on which 1 can improve his position and v is the least salient motion on which he can vote sophisticatedly. His medium preference is for the trade on motions z and w (with member 2) because z is the next most salient motion on which 1 can improve his position and w is the next-least-salient motion on which he can vote sophisticatedly. His lowest preference is for the trade on motions x

Table 14.

Member	Preference for Trades		
	Highest Preference	Medium Preference	Lowest Preference
1	(t, v)	(w, z)	(x, y)
2	(w, z)	(x, y)	(t, v)
3	(x, y)	(t, v)	(w, z)

and y (between members 2 and 3) on which he is left out. If, now, the members vote on which trades they wish to occur, the outcome will of course be an intransitive social arrangement:

(t, v) is socially preferred to (w, z)

by members 1 and 3,

(w, z) is socially preferred to (x, y)

by members 1 and 2,

and

(x, y) is socially preferred to (t, v)

by members 2 and 3

so that the arrangement is a cycle:

$$(t, v) \rightarrow (w, z) \rightarrow (x, y) \rightarrow (t, v),$$

where the arrow indicates a majority preference.

Thus, no trade is preferred to all other trades, which is equivalent to our earlier demonstration that no coalition can defeat all others. Since there

is no preferred set of trades on which to base a stable coalition, no stable coalition is possible. The paradox of vote trading cannot, therefore, be simply solved by waving it away with a coalition. Rather it is inherent in the nature of the legislative process and, given an appropriate distribution of tastes and external costs, cannot be avoided.

This absence of stability in coalitions means that vote trading cannot assure a dominant collective preference.¹⁵ Just as a cycle among all members (as in Table 13) precludes a dominant collective preference, so the instability of coalitions precludes a dominant collective preference among proper subsets of members. There seems, therefore, to be an inherent disequilibrium in vote trading which is occasioned by the assumption of individual rationality.

The Significance of the Paradox

The popular distrust of logrolling derives, we suspect, from an intuitive, incomplete, but, nonetheless, sure comprehension of this paradox or at least of the external costs on which it is based. To make a judgment on the validity of the popular distrust requires, therefore, that one estimate the likelihood that the paradox arise in real legislatures. If it arises frequently, then surely the popular distrust has a reasonable basis. If it arises only rarely, however, then one probably ought to conclude that logrolling is a socially useful technique to approach Pareto-optimal states of the world. (The positions of members are Pareto-optimal if there are no trades such that the position of at least one member can be improved without detriment to any other members.)

There is no systematic way, so far as we can discover, to investigate real world frequencies of the paradox. One can, however, specify types of circumstances in which it is likely to occur and then estimate the likelihood of these circumstances.

The crucial element of the paradox is external costs, which are of course unique to each trade. It is always the case, however, that more people

gain than lose on each switch (i.e., the members of $-S(x)$ and the pivotal member(s) of $S(x)$ gain, while the non-pivotal member(s) of $S(x)$ lose.) Consequently, in a system of trades—and usually for each participant as well—more instances of gain occur than do instances of loss. But gains and losses are not necessarily equal in size so the paradox occurs when, in a system of trades, members generally lose more when they lose than they gain when they gain. The question then is: Under what real world circumstances might these large losses be expected to occur?

Manifestly, if there is only one trade, the paradox is impossible because the gainers on that trade cannot suffer external costs on another, which does not exist. Consequently, if trades are occasional and isolated and not part of a system of interrelated bargains, losses are probably less than gains for many members of the legislature. This kind of sporadic logrolling is often found in American legislatures, but it is not the kind so earnestly recommended by the several scholars cited in the beginning of this essay. The kind they recommend is, rather, that in which members engage in a constant round of bargaining in order to arrive at optimal outcomes over the whole set of issues in the legislative session. Unfortunately, it is exactly this kind that produces the possibility of the paradox.

If one can separate out his reformist concerns, this is what Schattschneider seems to have perceived about the writing of the Smoot-Hawley Tariff. Each member who joined the tariff combination for a price on some issue (usually a tariff on something manufactured in his district) gained an advantage for himself and his constituents. Such a large number joined, however, that protection was made indiscriminate—nearly everybody got something. Thereby international trade was discouraged and disrupted—to the disadvantage of everybody in the society. To the extent that the Smoot-Hawley tariff deepened and extended the Great Depression, even the gainers probably suffered more than they gained.

One can see the same process at work in, for example, the Rivers and Harbors bills of the twentieth century when they had ceased to have the character of vital internal improvements that they had in the nineteenth. A majority gain from the pork in such barrels, else the bills would not pass. But there are also losses, external costs. Once taxes are paid, the net gains of the gainers are probably pretty small. And if the projects are for the most part economically useless (like locks on a canal only a few pleasure boats use), then opportunity costs probably exceed the gainers' gains. And if the projects are positively harmful (like dikes on a river that would otherwise sup-

¹⁵ For the case of cardinal utility, R. E. Park has shown that there is in general no stable equilibrium if there is a majority that can, through vote trading, improve the payoffs to all its members over the payoffs without vote trading. Furthermore, he has shown that if there is a stable equilibrium with vote trading, it must be exactly the same as the outcome without vote trading. R. E. Park, "The Possibility of a Social Welfare Function: Comment," *American Economic Review*, vol. 57 (December 1967), 1301-04. In "The Possibility of a Social Welfare Function: Reply," *ibid.*, pp. 1311-17, however, James Coleman argues that Park's assumptions are unrealistically restrictive.

port wet lands and a high water table), then their absolute losses almost certainly exceed the gainers' gains.

Still other possible cases come readily to mind: As a long-term example, there is the system of income tax exemptions and deductions, which are so generally distributed they actually provide savings for only an unidentifiable few and which at the same time probably assess very high costs against nearly everybody in the form of distortions of the market for many goods. For another—and even longer term—example, there is the proliferation of army bases throughout the country, each providing a small economic benefit to its neighborhood, but by their inefficiency considerably increasing in sum the military cost for everybody.

As these examples suggest, the bills and circumstances likely to occasion paradoxes are those which bring together an interrelated set of issues, many of which are of interest to only a few legislators. Tax bills, internal improvement bills, redistributions of income, etc. seem especially prone to providing the kind of situation in which the paradox can easily arise. Since these kinds of bills and business occupy a good part of any legislature's time, it must be concluded that the paradox of vote trading is a real fact of political life. One suspects that, because of it, logrolling leads more

often away from than toward Pareto-optimal social allocations.

With a bow in the direction of reformers, both those who recommend and those who execrate logrolling, it might be asked whether or not occurrences of the paradox can be prevented. The answer is, probably not, short of highly unpalatable restrictions. Legislatures do occasionally adopt self-denying ordinances, like the so-called closed rule in the House of Representatives, a rule that prohibits amendments from the floor. While this probably prevents the grosser and more thoughtless types of logrolling, the anticipation of it probably forces the trading back off the floor into committee. Given all the rational pressures for trading, it is not really to be expected that a policy of self-denial will work. Stronger measures, like the system of responsible and disciplined parties, may well make trading impossible in the legislature, but they certainly cannot prevent it in the executive branch or in the party caucus.

It seems, then, that so long as we have the open kind of legislature in which any pair of members may trade, then trading cannot be eradicated. Nevertheless, speaking normatively, the paradoxical consequences are unpalatable and one probably ought to try—by such devices as closed rules and popular condemnation—to discourage the kinds of logrolling that generate paradoxes.

Schema Theory: An Information Processing Model of Perception and Cognition*

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I. Introduction

A. The Idea of a Schema

The world is complex, and yet people are able to make some sense out of it. A national or international political arena, for example, is so huge and so complex that to make any sense out of it seems to be a superhuman task. And yet national leaders and even the man in the street do make more or less intelligent interpretations about political events and relationships. How do they do this?

One of the most important tools that people use is a schema. A formal definition will be given later, but informally a schema is a "pre-existing assumption about the way the world is organized."¹ When new information becomes

available, a person tries to fit the new information into the pattern which he has used in the past to interpret information about the same situation. If the new information does not fit very well, something has to give.

Balance is an important example of a commonly used schema, and its use can be illustrated by friendship relationships. A person has a balanced set of beliefs about his acquaintances if he believes that all of his friends like each other, all of his enemies also like each other, and each of his friends dislikes and is disliked by each of his enemies. Thus a balanced set of friendship relationships provides a tidy view of the world in "black and white" terms. It implies that friends of friends are friends, enemies of friends are enemies, and enemies of enemies are friends. More formally, balance can be defined in terms of a set of objects and a set of relationships between them. The whole collection is balanced if the objects can be classified into no more than two non-overlapping sets (e.g., "friends" and "enemies") so that all the relationships which exist between members of the same set are positive (such as "likes") and all the relationships which exist between members of differing sets are negative (such as "dislikes").

Suppose a person interprets the relationships between his acquaintances as fitting a balanced schema. Then if he receives new information about a particular relationship or about several relationships he can try to fit this new information into the old interpretation. If the new information fits exactly into his previous expectations, there is no problem. If it does not fit exactly, he has a variety of tactical choices. He can try to use the same schema as before with the same interpretation of who are his friends and who are his enemies. This will require discounting the new discrepant information. He might also use the same schema of balance, but specify which people are his friends and which are his enemies. Or he may use another schema entirely, and interpret the information as indicating (for example) that there are three different clusters of people (rather than two)

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In reviewing the psychological literature I have relied heavily on Gardner Lindzey and Elliot Aronson, eds., *The Handbook of Social Psychology*, second edition (Reading, Mass.: Addison-Wesley, 1968, 1969), especially the chapters by Berger and Lambert "Stimulus-Response Theory in Contemporary Social Psychology," I, 81-178; Seymour Rosenberg, "Mathematical Models of Social Behavior," I, 179-244; Robert B. Zajonc, "Cognitive Theories in Social Psychology," I, 320-411; Leonard Berkowitz, "Social Motivation," III, 50-135; Henri Tajfel, "Social and Cultural Factors in Perception," III, 315-394; and George A. Miller and David McNeill, "Psycholinguistics," III, 666-794.

¹ Jerome E. Singer, "Consistency as a Stimulus Process Mechanism," in *Theories of Cognitive Consistency: A Sourcebook*, ed. Robert P. Abelson, Elliot Aronson, William McGuire, Theodore Newcomb, Milton Rosen-

berg and Percy Tannenbaum (Chicago: Rand McNally, 1968).

who all like other members of the same cluster and dislike anyone not in their cluster.²

This paper is written with three audiences in mind. For the general political scientist interested in the policy-making process, the paper integrates a variety of psychological research into an organizing framework which can be used as an introduction to a number of relevant subfields of psychology. For the political scientist who is specifically concerned with attitudes and beliefs or the role of perceptual and cognitive processes in politics, the paper is offered in the hope of providing new insights into how people can and should cope with the highly complex environments typical of political arenas. For the experimental and social psychologists whose work is being used, this paper offers an information-processing model of perception and cognition which I call "schema theory." This schema theory has theoretical implications that go beyond the particular findings used. For all readers, the schema theory is meant to raise new questions that can guide further empirical as well as theoretical work. In this way the inaccuracies and overgeneralizations of the current model can be discovered, alternatives proposed, and our knowledge advanced.

At the start it is best to have clearly in mind what the schema theory is about and what it is not about. It is about how a single person observes and makes sense out of a complex environment. Therefore it describes the perceptual and cognitive processes of a single person; it does *not* describe the functioning of a small group, let alone a social movement or a government. Furthermore, it describes how a person processes information and tries to make sense out of it, but it does not describe how he makes policy decisions. This paper is designed to stand alone, but it can also be regarded as part of a larger project which deals with groups as well as individuals and with decision making as well as information processing. The structure of such a larger project is suggested in the author's *Framework for a General Theory of Cognition and Choice*.³ An application of part of that framework to the decision-making pro-

cess in an actual governmental committee is given in the author's "Psycho-algebra: A Mathematical Theory of Cognition and Choice with an Application to the British Eastern Committee in 1918."⁴ The present paper focuses on the information-processing aspects of the overall framework. It employs findings from experimental and social psychology to help formulate a schema theory which provides falsifiable substance to that part of the framework.

The schema theory itself takes the form of a partially specified model of how people process new information. Once the model is presented with the help of a flow-chart, a variety of findings from experimental psychology will be cited to support some of the decisions which went into the construction of this version of the model. Then further empirical findings will be cited to show that some of the predictions which the schema theory makes are valid at least in certain circumstances. The approach used here will then be compared to some other theoretical approaches, and consideration will be given to some of the outstanding research questions raised by the theory. Finally, a few illustrations from international relations and especially foreign policy formation show that this model of how people make sense out of a complex world can be directly relevant to the study of important political processes.

B. The Elements of the Model

The input to the model is a *message*, which contains several types of information. Each message has a known *source*, which may be one's own direct observation or another person. The heart of the message is some *information* about a particular case, such as the state of the friendship relationships between a certain group of individuals. The *type of case* (for example "friendship relationships") is also known. Thus a message in this model might take the form, "Mr. Smith says, 'Joe likes Bill, and Bill likes Sam.'" A full description of this particular case would indicate who likes whom and who dislikes whom among the whole set of relevant people, although the message itself need not contain a complete specification.

A *schema* in this model is defined as a subset of all the possible specifications of cases. A schema can be arbitrarily defined as any subset of the specifications, but typically a schema is defined in terms of the set of all specifications which have certain stipulated properties. For

² For a formal treatment of balance see Frank Harary, Robert Norman and Dorwin Cartwright, *Structural Models* (New York: Wiley, 1965). For a treatment of clusters see James Davis, "Clustering and Structural Balance in Graphs," *Human Relations*, 20 (May, 1967), 181-187; and James A. Riley, "An Application of Graph Theory to Social Psychology," in *The Many Facets of Graph Theory*, Lecture Notes in Mathematics, No. 110 (New York and Berlin: Springer Verlag, 1969).

³ Robert Axelrod, *Framework for a General Theory of Cognition and Choice* (Berkeley: Institute of International Studies, University of California, 1972). Also to appear in *Theories of Collective Behavior*, ed. Julius Margolis and Henry Teune, forthcoming.

⁴ Robert Axelrod, "Psycho-Algebra: A Mathematical Theory of Cognition and Choice with an Application to the British Eastern Committee in 1918," *Papers of the Peace Research Society (International)*, 18 (1972), 113-131.

example, the schema of *balance* is defined as the collection of all specifications which divide objects into two nonoverlapping groups with positive relationships only within groups and negative relationships only between groups. A larger schema would be *symmetry*, in which the only requirement is that whatever A's relationship is to B, B has to have the same relationship to A. Obviously, any complete specification which is balanced is also symmetric, but not vice versa.

A *case* is a specific instance in time. A particular case might represent, for example, the friendship relationships among a given group of people at the present time. An *interpretation* of a case in terms of a schema is a treatment of the case as if it belonged to that schema, perhaps by ignoring the information that might indicate otherwise.

A *complete specification* of a case is the listing of all the details of a case using an interpretation of the case in terms of a given schema. For example, a complete specification of a case of friendship relations using a balanced schema would list all the liking and disliking relationships among the set of people. Notice that if the case were interpreted as fitting a balanced schema, the complete specification could be determined simply by listing to which of the two groups each person belonged.

A specification of a case is never completely certain for a person, so each specification carries a certain degree of confidence; thus a specification of a case represents a belief which is held with more or less confidence by the person.⁵ The *accessibility* of a schema for a particular type of case is the order in which that schema is checked for its fit to the information. A highly accessible schema is checked before a fit with a less accessible schema is attempted. The critical assumption of this model is that the person tries to find a schema which provides a satisfactory fit to the case at hand. How this is done is described in the next section.

C. The Dynamics of the Model

Figure 1 is a flow-chart of the model. The process starts when a message is received about a case (box 1). The first question is then whether there is already an interpretation of this case (box 2).⁶ If there is, then the new in-

formation is checked to see if it fits sufficiently well any of the old specifications based on that interpretation (box 3). If it does fit the old interpretation, there is no problem, and the new information gets interpreted with the help of the old schema (box 11). If the new information does not fit the old interpretation, then blame is affixed by comparing the credibility of the source of the new information with the confidence of the old interpretation (box 4). If the new message is blamed, the credibility of the new source is downgraded for future reference, and the process ends with the old interpretation and specification maintained unchanged (box 5). If the old interpretation is blamed, the source of the old information has its credibility downgraded, and the old interpretation is cancelled (box 6). Whatever can be remembered of the old information is then taken at face value without any schema to interpret it and is combined with the new information in the new case to provide a partial specification of the case at hand (box 8). This combining process (box 8) would also occur if there had been no previous interpretation of the case but there had been some old uninterpreted information on the case (a "Yes" after 7).

From either path (whether from box 6 or 7), the person gets to the step of seeking a schema which will provide a satisfactory fit to the available information at hand (box 9). This step uses a "satisficing subroutine" which will be described separately because it is so important to the model. If the search process fails to find a schema which provides a sufficiently good fit, the source is downgraded (box 10), and the process ends without an interpretation of the case: i.e., the person is baffled.

If the search for a satisfactory schema is successful, this schema is then used to specify further the case at hand (box 11). The specification stage (box 11) is also reached if an interpretation of the case at hand already existed in terms of some schema and if the new information fit the old specification sufficiently well (i.e., "Yes" after box 2 and "Yes" after box 3). The interpretation is made by first modifying the new information if necessary so that it will coincide exactly with the selected schema. For example, if the selected schema is *balance*, then all the inconsistencies (such as "A likes B and C, but B doesn't like C") are resolved to correspond to the schema of *balance*. This gives a modified partial specification of the case. Next this perfectly fitting partial specification is extended with the further use of the selected schema. For example, if the partial specification includes the information that Joe likes Sam, and Sam likes Bill, and the selected

⁵ For simplicity, the present model makes the unrealistic assumption that each of the separate relationships within a specification is believed with the same degree of confidence.

⁶ The present model assumes that new information can be assigned to a case and that each case can be assigned to a known case type. Quite possibly these assignments involve a process similar to the interpretation process described within the model.

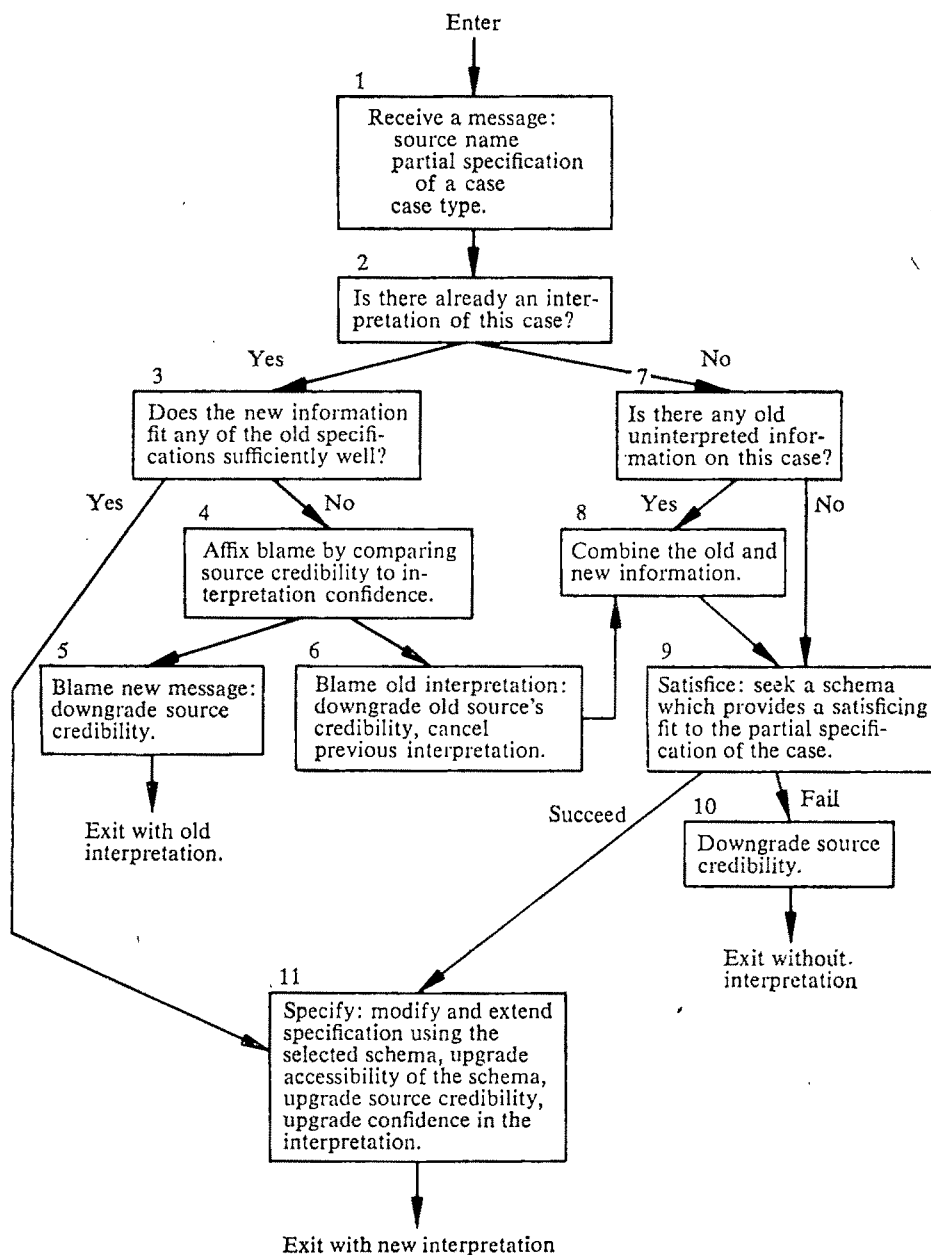


Figure 1. Process Model for Schema Theory.

schema is one of balance, then several inferences can be drawn. One is that Sam likes Joe (since relationships in a balanced schema are symmetric) and another is that Joe likes Bill (since positive relationships in a balanced schema are transitive). These additional inferences allow the person to extend his specification of the current case.

Together with this modification and exten-

sion of the partial specification of the case there are some bookkeeping operations which allow the person to benefit in the future from his experience of finding a satisfactory fit (box 11). He upgrades the accessibility of the selected schema, he upgrades the credibility of the source of the message, and he upgrades the confidence in the current interpretation of the case. If there was already an interpretation of

this case and the new information fits this interpretation, then the confidence in that interpretation will be increased. On the other hand, if there was no previous interpretation of the case, then the certainty of the present interpretation depends directly on the credibility of the source of the current message and the goodness of the fit of the new information to the selected schema. Once the interpretation is made and the bookkeeping is performed, the process ends with the new interpretation and specification as the primary output.

The satisficing subroutine (box 9) is presented in greater detail in Figure 2. This subroutine describes how a person seeks a schema

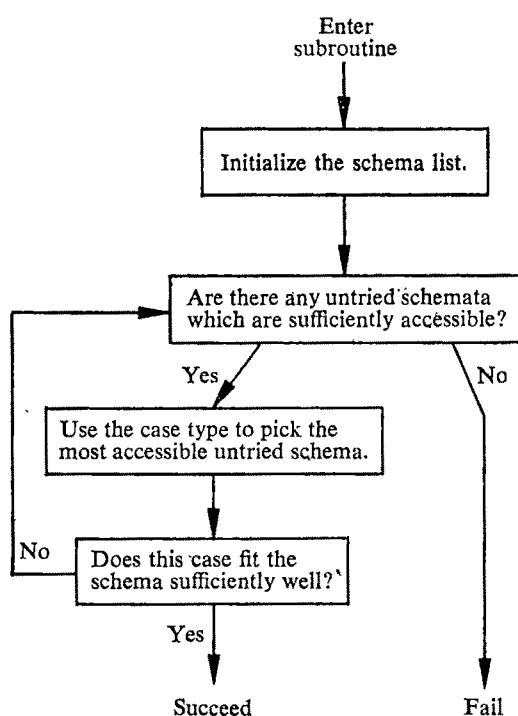


Figure 2. The Satisficing Subroutine.

which will provide a satisfactory fit to the available information on the case at hand. A person does not try out every conceivable schema in an attempt to find the best fit of all. Instead he uses his knowledge of the type of case it is to pick out the most accessible schema for that type of case. Then if the fit with this schema is not sufficiently good, he tries the next most accessible schema, and so on. He continues this process until either he finds a schema which provides a satisfactory fit or he exhausts the schemata which are sufficiently accessible for this type of case. Thus the process of trying to

fit the information about a case to a schema is a process of "satisficing" (to use the apt term of Simon)⁷ rather than maximizing.

This, then, is the model of how a person makes sense out of a complex world. Once he has interpreted a case in terms of a schema he achieves four things:

(1) He modifies some of the information in the light of his old interpretation (if there was one) and the rest of his information. This allows him to change his beliefs about those parts of the information which may have been wrong for one reason or another. Whether these changes are corrections or distortions depends upon the accuracy of the new interpretation and specification.

(2) He usually achieves a more complete specification of the case than he was given by the information in the original message or messages. This allows him to make predictions about previously unobserved aspects of the case. These predictions can be helpful for decision making since the partial specification of the case (even when modified) might not have constituted an adequate guide for behavior. The degree to which the extended specification can serve as a guide for action depends upon whether the selected schema contains parameters that can help a person identify and choose among future courses of action. For example, a balanced interpretation of a set of friendship relationships which includes oneself is a powerful guide to action, since it allows one to orient oneself positively or negatively toward each of the other people without any ambiguity.

(3) He reduces his memory requirements by interpreting the separate bits of information about the current case in terms of the parameters of a schema. For example, it takes n^2 bits of information to remember an arbitrary set of binary friendship relationships between n people, but it takes only $n-1$ bits of information to remember a set of friendship relationships which fits a balanced schema of friends and enemies. The amount of the reduction in the memory requirement depends upon the "size" of the schema, a characteristic which is further explored in VI, A below.

(4) He uses the results of the current case to provide feedback which will affect the processing of information in the future. In particular, he makes adjustments for the future interpretations of this or other cases by updating the accessibility of the schema, the credibility of the source, and the confidence in the specification of the current case. Whether these modifi-

⁷ Herbert Simon, *Models of Man* (New York: Wiley, 1957).

cations will be a help or a hindrance in the future depends upon whether he has drawn the right conclusions from the current message.

Now that the model has been presented, the relevant evidence for this schema theory will be discussed in the next two sections. The first of these sections (Part II) offers the evidence which deals with specific features of the process, as indicated by references to specific boxes in the flow-chart of Figure 1. Part III offers evidence on the overall system performance of the model. These two parts of the paper and the two parts that follow them are written in outline form in order to better display the structure of the presentation.

II. Evidence Relating to Specific Parts of the Model

A. Use of Prior Interpretations (Figure 1, Boxes 2 and 3)

1. *Claim:* A prior interpretation of a case (if one exists) is highly accessible.

Evidence: There is a primacy effect in impression formation, which means that a first impression carries more weight than later impressions.⁸

B. The Blaming Process (Boxes 4, 5, and 6)

1. *Claim:* When new information gives a bad fit to the old interpretation, one possibility is that the old interpretation can be maintained by downgrading the credibility of the source of the new information.⁹

Evidence: Information which is discrepant with previous beliefs tends to be perceived as less informed and less fair.¹⁰

2. *Claim:* The greater the source credibility, the more likely the old interpretation is to be blamed for any discrepancy.

Evidence: When the same message is attributed to sources of differing credibility, differing amounts of attitude change are produced

with the more credible sources producing the greater attitude change.^{11,12}

3. *Claim:* The greater the source credibility, the less likely the source is to be blamed for any discrepancy.

Evidence: A given message is judged more fair and more fully documented when it comes from a high credibility source rather than a low credibility source.¹³

4. *Claim:* Sources whose messages are likely to have been interpreted with an accessible schema tend to have high credibility.

Evidence: Attribution of knowledge, intelligence, education and social status give credibility to an unknown source.¹⁴

But it is not clear whether the old interpretation and the new source can both be blamed at once.¹⁵

5. *Claim:* When an old interpretation is blamed, the schema used in the old interpretation becomes somewhat less accessible for future cases of the same type and perhaps for other types of cases as well.

Evidence: Children can learn with experience to switch the order in which they test hypotheses.¹⁶

But even adults have greater difficulty developing nonlinear hypotheses to explain evidence when linear hypotheses repeatedly fail.¹⁷

C. Combining Information (Boxes 7 and 8)

1. *Claim:* Old and new information about

¹¹ L. R. Anderson, "Discrediting Sources as a Means of Belief Defense of Cultural Truism," *American Psychologist*, 21 (July, 1966), 708.

¹² A more complex model would take account of other source factors as well as credibility. The new information could then be regarded as having a confidence parameter which was determined by factors including the attractiveness and the power of the source as well as the source's credibility. For a review of source factors in attitude change see William J. McGuire, "The Nature of Attitudes and Attitude Change," in *Handbook of Social Psychology*, ed. Lindzey and Aronson, III, 136-314.

¹³ C. I. Hovland and W. Mandell, "An Experimental Comparison of Conclusion-drawing by the Communicator and by the Audience," *Journal of Abnormal and Social Psychology*, 47 (July, 1952), 581-588; and C. I. Hovland and W. Weiss, "The Influence of Source Credibility on Communication Effectiveness," *Public Opinion Quarterly*, 15 (Winter, 1951), 635-650.

¹⁴ Carl I. Hovland, Irving L. Janis, and Harold H. Kelley, *Communication and Persuasion* (New Haven: Yale University Press, 1953).

¹⁵ C. E. Osgood and P. H. Tannenbaum, "The Principle of Congruity in the Prediction of Attitude Change," *Psychological Review*, 62 (January, 1955), 42-55.

¹⁶ Neal E. Miller and John Dollard, *Social Learning and Imitation* (New Haven: Yale University Press, 1941).

¹⁷ Mats Bjorkman, "Predictive Behavior: Some Aspects Based on Ecological Orientation," *Scandinavian Journal of Psychology*, 7 (1966), 43-57.

⁸ S. E. Asch, "Forthcoming Impressions of Personality," *Journal of Abnormal and Social Psychology*, 41 (July, 1946), 258-290; and N. H. Anderson, "Primacy Effects in Personality Impression Formation Using a Generalized Order Effect Paradigm," *Journal of Personality and Social Psychology*, 2 (July, 1965), 1-9.

⁹ Note that the present model does not treat the effects of limited or inaccurate memory.

¹⁰ J. E. Dietrich, "The Relative Effectiveness of Two Modes of Radio Delivery in Influencing Attitudes," *Speech Monographs*, 13 (1946), 58-65; C. I. Hovland, O. J. Harvey, and M. Sherif, "Assimilations and Contrast Effects in Communication and Attitude Change," *Journal of Abnormal and Social Psychology*, 55 (September, 1957), 242-252; and Anne S. McKillop, *The Relationship Between the Reader's Attitudes and Some Types of Reading Response* (New York: Columbia University Press, 1952).

the same case are combined using a weighted average.

Evidence: The impression of a sequence of trait adjectives is the weighted average of the impressions of the separate adjectives and of an assumed nonzero initial impression.¹⁸ The existence of the initial impression is supported by the finding that even without specific information, people have non-neutral expectations, e.g., that two people will like rather than dislike each other.¹⁹

D. Selecting a Schema (Boxes 9 and 10)

1. *Claim:* The selection of a schema with which to interpret information about a case is a process of satisficing rather than maximizing the fit.

Evidence:

a. Some pictures are multistable so that the same person can give several different but temporarily stable interpretations of the same ambiguous picture.²⁰

b. The Socratic effect seems to exist in which a person who is asked to state his opinions on related issues changes his opinions to achieve logical consistency.^{21,22}

c. Nonrandom distortions are introduced when a person is asked to reproduce a message.²³ For example, incongruous or unusual details are either forgotten or exaggerated (presumably because either no schema was found to fit them or a relatively inaccessible schema was selected).

Note: There is as yet little evidence to indicate just what type of suboptimal process is used to select a schema from among those which are more or less accessible and which provide a better or worse fit to the initial partial specification of the current case. The use of a satisficing process in a model of schema selec-

tion seems suitable but must be regarded as conjectural. For the use of satisficing processes in the choice of alternative courses of action see Simon,²⁴ March and Simon,²⁵ and Cyert and March.²⁶

2. *Claim:* The accessibility of a schema is independent of the input and output modes.

Evidence: The accessibility of a schema is relatively insensitive to the manner in which the message is presented²⁷ or the manner in which the response is elicited.²⁸

3. *Claim:* The accessibility of a schema depends in part on the case type of the current case.

Evidence: Two identical patterns of relationships are processed differently if different labels are given to them. For example, a pattern of interpersonal relationships which is completely ordered (asymmetric, transitive, and complete) is much easier to learn if the relationships are labeled "influences" rather than "likes."²⁹ See also III, A, 1.

E. Modifying the Initial Partial Specification (Box 11)

1. *Claim:* The initial partial specification is modified in a manner which improves the fit between the partial specification and the selected schema.

Evidence:

a. An assimilation/contrast effect exists in which a subject tends to underestimate the distance between a stimulus and a standard of reference when the distance is small, and overestimate it when the distance is large.³⁰ The effect has been applied to attitude change.³¹

¹⁸ Simon, *Models of Man*.

²⁴ James G. March and Herbert A. Simon, *Organizations* (New York: Wiley, 1958).

²⁵ Richard M. Cyert and James G. March, *A Behavioral Theory of the Firm* (Englewood Cliffs, N.J.: Prentice-Hall, 1963).

²⁶ James Kuethe, "Pervasive Influence of Social Schemata," *Journal of Abnormal and Social Psychology*, 68 (May, 1964), 248-254.

²⁷ James Kuethe and Herbert Weingartner, "Male-Female Schemata of Homosexual and Non-Homosexual Penitentiary Inmates," *Journal of Personality*, 32 (March, 1964), 23-31.

²⁸ Clinton B. DeSoto, "Learning a Social Structure," *Journal of Abnormal and Social Psychology*, 60 (May, 1960), 417-421.

²⁹ Muzafer Sherif and Carl I. Hovland, *Social Judgment: Assimilation and Contrast Effects in Communication and Attitude Change* (New Haven: Yale University Press, 1961); Carolyn W. Sherif, Muzafer Sherif, and R. E. Nebergall, *Attitude and Attitude Change* (Philadelphia: Saunders, 1965); and Carolyn W. Sherif and Muzafer Sherif, eds., *Attitude, Ego-Involvement and Change* (New York: Wiley, 1967).

³⁰ Sherif and Hovland, *Social Judgment*.

¹⁹ Anderson, "Primacy Effects"; Zajonc, "Cognitive Theories"; and Rosenberg, "Mathematical Models."

²⁰ Clinton B. DeSoto and James L. Kuethe, "Subjective Probabilities of Interpersonal Relationships," *Journal of Abnormal and Social Psychology*, 59 (September, 1959), 290-294.

²¹ Fred Attneave, "Multistability in Perception," *Scientific American* (December, 1971), pp. 63-71.

²² This effect was found by William J. McGuire, "Cognitive Consistency and Attitude Change," *Journal of Abnormal and Social Psychology*, 60 (May, 1960), 345-353.

²³ Only weak indications of such an effect were found by R. C. Dillehay, C. A. Insko, and M. B. Smith, "Logical Consistency and Attitude Change," *Journal of Personality and Social Psychology*, 3 (June, 1966), 646-654.

²⁴ Frederic Bartlett, *A Study in Experimental and Social Psychology* (Cambridge: Cambridge University Press, 1969).

personality functioning,³² and social perception.³³

b. A stereotyping effect exists in which a subject underestimates the relevant differences within certain groups and overestimates the differences between contrasting groups. Stereotyping occurs not only in the perception of social groups, but even of abstract stimuli such as lengths of variously classified lines³⁴ and stimuli classified by their positions in an array.³⁵

c. Regularization of speech occurs in which children treat strong verbs as if they were weak verbs (e.g., by saying "comed" instead of "came" for the past tense of "come").³⁶

d. A common error in recalling an experience is to recall the part that fits a schema and forget the part that does not fit.³⁷ In general, material is recalled in a manner which indicates that it has been selected, organized and interpreted.³⁸

e. Stimuli which have separate names can be more accurately remembered than stimuli which do not (e.g., highly codable colors can be accurately selected from a collection of colors). This holds for the same colors between different languages³⁹ and for different colors within one language.⁴⁰ It may be due to covert verbalization.⁴¹

f. The extent to which actual friendship groups fit the schema of balance is overperceived.⁴²

F. Expansion of the Partial Specification (Box 11)

1. *Claim:* The modified partial specification is extended in a manner which does not worsen the fit between the specification and the schema.

Evidence:

a. Guesses based on limited information provide the best possible fits to the appropriate schema (e.g., if "A likes B" then "B likes A" is guessed),⁴³ and in general missing information about friendship patterns is guessed to fit a balanced schema.⁴⁴

b. Guesses in signal detection experiments are consistent with a model of a person as a receiver who is uncertain about the signal rather than as a receiver who has a variable response criterion or who has noise inherent in his sensory system.⁴⁵

c. Errors in long-term recall of liking relationships contained in stories are more likely to be made in recalling imbalanced as balanced patterns than the other way around.⁴⁶

d. The previously noted effect of overperception of balance in actual friendship groups⁴⁷ may be due to the extension of the partial specification beyond the information available.

Journal of Verbal Learning and Verbal Behavior, 1 (January, 1963), 289-299.

⁴² Nathan Kogan and Renato Tagiuri, "Interpersonal Preference and Cognitive Organization," *Journal of Abnormal and Social Psychology*, 56 (January, 1958), 113-116.

⁴³ Clinton E. DeSoto and James L. Kuethe, "Perception of Mathematical Properties of Interpersonal Relations," *Perceptual and Motor Skills*, 8 (December, 1958), 279-286; and DeSoto and Kuethe, "Subjective Probabilities." For a quantitative extension, see A. Rodney Wellens and Donald L. Thistlethwaite, "Comparison of Three Theories of Cognitive Balance," *Journal of Personality and Social Psychology*, 20 (October, 1971), 82-92.

⁴⁴ J. Morrisette, "An Experimental Study of the Theory of Structural Balance," *Human Relations*, 11 (August, 1958), 239-254; and Elizabeth G. Schrader and D. W. Lewit, "Structural Factors in Cognitive Balancing Behavior," *Human Relations*, 15 (August, 1962), 265-276.

⁴⁵ J. A. Swets, "Is There a Sensory Threshold?" *Science*, 134 (July 21, 1961), 168-177; and D. M. Green, "Psychoacoustics and Detection Theory," *Journal of the Acoustical Society of America*, 29 (1960), 1180-1203. For a review see Clyde H. Coombs, Robyn M. Dawes, and Amos Tversky, *Mathematical Psychology* (Englewood Cliffs, N.J.: Prentice-Hall, 1970).

⁴⁶ Harold B. Gerard and Linda Fleischer, "Recall and Pleasantness of Balanced and Unbalanced Structures," *Journal of Personality and Social Psychology*, 7 (November, 1967), 332-337.

⁴⁷ Kogan and Tagiuri, "Interpersonal Preference."

³² Leonard Berkowitz, "Judgmental Processes in Personality Functioning," *Psychological Review*, 67 (March, 1960), 130-142.

³³ Leonard Berkowitz and R. E. Goranson, "Motivational and Judgmental Determinants of Social Perception," *Journal of Abnormal and Social Psychology*, 69 (September, 1964), 296-302.

³⁴ Henri Tajfel and A. L. Wilkes, "Classification and Quantitative Judgement," *British Journal of Psychology*, 54 (May, 1963), 101-114.

³⁵ D. T. Campbell, "Enhancement of Contrast as Composite Habit," *Journal of Abnormal and Social Psychology*, 53 (November, 1956), 350-355.

³⁶ Susan Ervin, "Imitation and Structural Change in Children's Language," in *New Directions in the Study of Language*, ed. Eric H. Lenneberg (Cambridge: M.I.T. Press, 1964), pp. 163-190.

³⁷ R. S. Woodworth, *Experimental Psychology* (New York: Holt, 1938); George A. Miller, "Some Psychological Studies of Grammar," *American Psychologist*, 17 (November, 1962), 748-762.

³⁸ Bernard R. Berelson and Gary A. Steiner, *Human Behavior* (Burlingame: Harcourt, Brace and World, 1964).

³⁹ Eric H. Lenneberg and John M. Roberts, *The Language of Experience* (Baltimore: Waverly Press, 1956). Also appeared as *International Journal of American Linguistics*, 22, No. 2; and in Sol Saporta, ed., *Psycholinguistics* (New York: Holt, Rinehart and Winston, 1961).

⁴⁰ Roger Brown and Eric H. Lenneberg, "A Study in Language and Cognition," *Journal of Abnormal and Social Psychology*, 49 (July, 1954), 454-462.

⁴¹ Murray Glazer and William H. Clark, "Accuracy of Perceptual Recall: An Analysis of Organization,"

III. Predictions of System Performance

A. Ease of Learning

1. *Prediction:* Where the initial information provides a good fit to an accessible schema, learning to reproduce the information in a message accurately will be easy.

Evidence:

a. The greater the imbalance in a liking structure, the more errors in learning a semicycle in it.⁴⁸

b. As previously noted, a hypothetical social structure is easiest to learn when it possesses the structural properties expected of it.⁴⁹

c. As previously noted, errors tend to improve the fit between the recalled material and the selected schema (see Evidence for Claim II, E, 1 above).

2. *Prediction:* When an excellent fit is required, the less accessible the appropriate schema is, the more difficult the problem will be to solve.

Evidence:

a. The unfamiliar use of the familiar is a difficult solution to achieve.⁵⁰

b. When a prior success is inapplicable, the problem is difficult to solve.⁵¹

3. *Prediction:* When the initial information provides a good fit to an accessible schema there will be little manifestation of surprise.

Evidence:

a. Messages which provide a good fit to a balanced liking schema are judged to be persuasive, accurate, and pleasant.⁵²

⁴⁸ R. B. Zajonc and E. Burnstein, "Structural Balance, Reciprocity, and Positivity as Sources of Cognitive Bias," *Journal of Personality*, 33 (December, 1965), 570-583. For related results on the learning of liking structures, see R. B. Zajonc and E. Burnstein, "The Learning of Balanced and Unbalanced Social Structures," *Journal of Personality*, 33 (June, 1965), 153-163.

⁴⁹ DeSoto, "Learning a Social Structure." For more recent verification that learning a set of relationships is easier when they fit the expected structure, see Clinton B. DeSoto, Nancy M. Henley, and Marvin London, "Balance and the Grouping Schema," *Journal of Personality and Social Psychology*, 8 (July, 1968), 1-7; Nancy M. Henley, Robert B. Horsfall and Clinton B. DeSoto, "Goodness of Figure and Social Structure," *Psychological Review*, 76 (1969), 194-204; and Robert B. Horsfall and Nancy M. Henley, "Mixed Social Structures: Strain and Probability Ratings," *Psychonomic Science*, 15 (May, 1969), 186-187; Donald L. Mosher, "The Learning of Congruent and Noncongruent Social Structures," *Journal of Social Psychology*, 73 (December, 1967), 285-290.

⁵⁰ Berelson and Steiner, *Human Behavior*, p. 204.

⁵¹ Berelson and Steiner, *Human Behavior*, p. 204.

⁵² Milton J. Rosenberg and Robert P. Abelson, "An Analysis of Cognitive Balancing," in *Attitude Organiz-*

b. Messages which are incongruous cause physiological arousal (e.g., as measured by galvanic skin response).⁵³

B. Attitude Change

1. *Prediction:* Old beliefs are changed in accordance with the model, i.e., when information in a new message does not fit the old interpretation, a comparison of the source credibility of the new message with the confidence in the old interpretation can result in blaming the old interpretation.

Evidence:

a. Repeating the same argument is no help in attitude change.⁵⁴

b. Internalization occurs in attitude change, which means that if the source of a persuasive message is forgotten, the changed attitude is still retained. Even if the source is known to switch his position, the attitude change remains.⁵⁵

c. Over a wide range, the larger the advocated change, the greater the change in attitude, provided the source is regarded as credible.⁵⁶

C. Effect of Experience

1. *Prediction:* A highly accessible schema will only be downgraded if reliable information is acquired which does not fit the schema (since no other schema will get priority in the search process).

Evidence: A dog taught to jump at regular intervals to avoid a shock continues to jump long after the shock mechanism is turned off. In order to unlearn the lesson, he has to be physically prevented from jumping.⁵⁷

2. *Prediction:* When the interpretation of a sequence of cases repeatedly uses the same schema, the schema becomes more accessible.

tion and Change, Milton J. Rosenberg, Carl I. Hovland, William J. McGuire, Robert P. Abelson and Jack W. Brehm (New Haven: Yale University Press, 1960), pp. 112-163.

⁵³ D. E. Berlyne, *Conflict, Arousal, and Curiosity* (New York: McGraw-Hill, 1960), and Evgenii N. Sokolov, *Perception and the Conditioned Reflex*, trans. Stephen W. Waydenfeld (Oxford and New York: Pergamon, 1963).

⁵⁴ McGuire, "The Nature of Attitudes and Attitude Change," p. 169.

⁵⁵ Sherif, "A Study of Some Social Factors in Perception."

⁵⁶ See McGuire, "The Nature of Attitudes and Attitude Change," p. 223, for a review.

⁵⁷ R. L. Solomon, L. J. Kamin, and L. C. Wynne, "Traumatic Avoidance Learning: The Outcomes of Several Extinction Procedures with Dogs," *Journal of Abnormal and Social Psychology*, 48 (April, 1953), 291-302.

Therefore, it will eventually be selected even when other less accessible schemata might provide better fits.

Evidence: As previously noted, children can learn to switch the order in which they test hypotheses.⁵⁸

D. Resolution of Cognitive Inconsistencies

1. *Prediction:* When some information (whether old or new) fits none of the sufficiently accessible schemata precisely, employable tactics include the following: blame the source, use the other old and new information about the case to select a schema which gives a sufficiently good fit to all the available information, or exit without an interpretation.

Evidence: These modes are among the many which have been suggested (with varying degrees of empirical support) as ways of resolving cognitive inconsistencies.⁵⁹

E. Measuring Accessibility of a Schema

The accessibility of a schema for a particular type of case is not directly observable. It can be measured indirectly using any one of the four methods described above: ease of learning, attitude change, effects of experience, and resolution of cognitive inconsistencies. When studying a new person or a new type of case, one method can be used to validate another method's results.

IV. Comparison with Other Theoretical Approaches

A. Computer Simulations

To my knowledge, no computer simulation of human perception or cognition treats the differential accessibility of alternative schemata, which is the heart of the present model.⁶⁰

⁵⁸ Miller and Dollard, *Social Learning and Imitation*.

⁵⁹ William J. McGuire, "The Current Status of Cognitive Consistency Theories," in *Cognitive Consistency: Motivational Antecedents and Behavioral Consequents*, ed. Shel Feldman (New York: Academic Press, 1966); Robert P. Abelson, "Psychological Implication," in Abelson et al., *Theories of Cognitive Consistency*, pp. 112-139; Abelson et al., *Theories of Cognitive Consistency*, chapters 64-71; and Percy H. Tannenbaum, Jacqueline R. Macaulay, and Eleanor L. Norris, "Principle of Congruity and Reduction of Persuasion," *Journal of Personality and Social Psychology*, 3 (February, 1966), 233-238.

⁶⁰ But for some interesting simulations of mental processes see Silvan Tomkins and Samuel Messick (eds.), *Computer Simulation of Personality* (New York: Wiley, 1963); Robert P. Abelson and J. Douglas Carroll, "Computer Simulation of Individual Belief Systems," *American Behavioral Scientist*, 8 (May, 1965), 24-30; John C. Loehlin, *Computer Models of Personality* (New York: Random House, 1968); U. Moser, I. S. von Zeppelin, and W. Schneider, "Computer Simulation of a Model of Neurotic Defence Processes," *Behavioral Science*, 15

B. Gestalt School of Psychology

The Gestalt School⁶¹ provides some of the fundamental ideas which the present information processing model tries to develop. The common criticism that the Gestalt approach has lacked precision is well founded.

C. Information Theory

Information theory⁶² provides some useful mathematical tools, including a way to measure the size of a schema (for a few details see Part VI, A). The application of information theory to formal problems in statistical inference is by now quite well developed.⁶³

D. Balance Theory

The predictions of balance theory⁶⁴ are consistent with the schema theory for those and only those situations in which the balanced schema is the one selected (see VI, B).⁶⁵

1. *Claim:* Balance is a highly accessible schema in certain contexts, such as friendship relationships.⁶⁶

Evidence (all previously cited):

- a. Balanced messages are more acceptable than unbalanced messages.⁶⁷
- b. Incomplete specifications are extended with the use of a balanced schema.⁶⁸
- c. Balance is overestimated.⁶⁹
- d. A balanced message is easier to learn and to retain over time.⁷⁰

(March 1970), 194-202; and Allen Newell and Herbert Simon, "Simulation of Human Thought," in *Computer Simulation of Human Behavior*, ed. John M. Dutton and William Starbuck (New York: Wiley, 1971).

⁶¹ Max Wertheimer, *Productive Thinking* (New York: Harper, 1945).

⁶² Wendell R. Garner, *Uncertainty and Structure as Psychological Concepts* (New York: Wiley, 1952).

⁶³ Satoshi Watanabe, *Knowing and Guessing: A Quantitative Study of Inference and Information* (New York: Wiley, 1969).

⁶⁴ Robert P. Abelson and Milton J. Rosenberg, "Symbolic Psychologic: A Model of Attitudinal Cognition," *Behavioral Science*, 3 (January, 1958), 1-13.

⁶⁵ For a basic introduction to balance theory, see Roger Brown, *Social Psychology* (Glencoe, Ill.: The Free Press, 1956); for a review of most of the experimental evidence see Zajonc, "Cognitive Theories"; for a fund of source material, see Abelson et al., *Theories of Cognitive Consistency*.

⁶⁶ In fact, much of the experimental work on schemata cited in this paper has been inspired more or less directly by the study of interpersonal relationships contained in Fritz Heider, *The Psychology of Interpersonal Relations* (New York: Wiley, 1958).

⁶⁷ Rosenberg and Abelson, "An Analysis of Cognitive Balancing."

⁶⁸ Morrisette, "An Experimental Study."

⁶⁹ Kogan and Tagiuri, "Interpersonal Preference."

⁷⁰ Clinton B. De Soto, Nancy M. Henley and Marvin London, "Balance and the Grouping Schema," *Journal of Personality and Social Psychology*, 8 (January, 1968),

e. Balanced messages are viewed as less likely to change than unbalanced messages.⁷¹

f. Balance is an accessible schema in perceived friendship relationships among a set of countries.⁷²

g. Balanced situations are imagined more vividly, rapidly and elicit higher affective ratings than unbalanced situations.⁷³

2. *Claim:* The limitations of balance theory can be understood in terms of the availability of schemata more accessible than the balance schema which can provide adequate fits to the information on a given case.

Evidence:

a. The schema of positivity (consisting of nothing but positive relationships) can explain some of balance theory's failures, such as

(1) positive links are easier to learn than negative links,⁷⁴

(2) positive semicycles are easier to learn than negative semicycles,⁷⁵ and

(3) there is not a large negative correlation between the first two links of a triad when the third is negative.⁷⁶

(4) subjects' willingness to change the relationships between themselves and a fictitious person is determined by positivity forces.⁷⁷

b. If a subject likes chicken, and chickens like chicken feed, the subject is not expected to like chicken feed.⁷⁸

c. If subjects are told that two boys like a girl, the subjects do not expect them to like each other as much as if they both like a third boy.⁷⁹

d. If told that two people like *Newsweek* magazine, subjects do not mistakenly learn that they like each other, but this mistake is made when two people are said to like racial integration.⁸⁰

3. *Claim:* A balanced schema is selected when appropriate, even outside of the context suggested by balance theory.

Evidence: The concepts used by decision makers in describing their images of external reality are balanced.⁸¹ A balanced schema is applied in tasks of recall and attribution of responsibility.⁸²

4. The schema theory is consistent with all of the following explanations for the tendency toward cognitive consistency: a motivational drive toward consistency, limited cognitive capacity resulting in the need for simplification of incoming information, and inadequacies in the incoming information which must therefore be processed in a manner that allows predictions to be made and later tested.

V. Development of Schema Theory

A. General Principles of Schema Accessibility

1. *Claim:* People seek logical consistency.

Evidence:

a. Arguments against the premise of a syllogism will affect the belief in the conclusion a week later.⁸³

b. Attitude change by posthypnotic sug-

1-7. Nicolas B. Cottrell, Larry H. Ingraham and Franklin W. Monfort, "The Retention of Balanced and Unbalanced Cognitive Structures," *Journal of Personality*, 39 (March, 1971), 112-131.

⁷¹ Eugene Burnstein, "Sources of Cognitive bias in the Representation of Simple Social Structures; Balance, Minimal Change, Positivity, Reciprocity, and the Respondent's Own Attitude," *Journal of Personality and Social Psychology*, 7 (September, 1967), 36-48. Harold Miller and Dennis Geller, "Structural Balance in Dyads," *Journal of Personality and Social Psychology*, 21 (February, 1972), 135-138.

⁷² Nicholas B. Johnson, "Some Models of Balance Applied to Children's Perceptions of International Relations," *European Journal of Social Psychology*, 2 (1972), 55-64.

⁷³ Edward B. Blanchard, Marilyn Vickers and Katina C. Price, "Balance Effects in Image Formation," *The Journal of Social Psychology*, 87 (June, 1972), 37-44.

⁷⁴ Zajonc and Burnstein, "The Learning of Balanced and Unbalanced Social Structures" and "Structural Balance."

⁷⁵ Zajonc and Burnstein, "Structural Balance."

⁷⁶ William M. Wiest, "A Quantitative Extension of Heider's Theory of Cognitive Balance Applied to Interpersonal Perception and Self-Esteem," *Psychological Monographs*, 79, No. 1 (Whole No. 607) (December, 1965).

⁷⁷ Aroldo Rodriguez, "Effects of Balance, Positivity and Agreement in Triadic Social Relations," *Journal of Personality and Social Psychology*, 5 (April, 1967), 472-476.

⁷⁸ R. B. Zajonc, "The Concepts of Balance, Congruity,

and Dissonance," *Public Opinion Quarterly*, 24 (Summer, 1960), 280-296.

⁷⁹ Robert B. Zajonc and Steven J. Sherman, "Structural Balance and the Induction of Relations," *Journal of Personality*, 35 (December, 1967), 635-650.

⁸⁰ Zajonc and Burnstein, "The Learning of Balanced and Unbalanced Social Structures." The authors explain this result by saying that one issue was less important than the other, but an alternative explanation is that the balance schema is not accessible for a case type involving readership of a general purpose magazine.

⁸¹ Axelrod, "Psycho-Algebra."

⁸² N. T. Feather, "Organization and Discrepancy in Cognitive Structures," *Psychological Review*, 78 (September, 1971), 355-379.

⁸³ William J. McGuire, "Cognitive Consistency and Attitude Change"; William J. McGuire, "A Syllogistic Analysis of Cognitive Relationships," in *Attitude Organization and Change*, Milton J. Rosenberg et al. (New Haven: Yale University Press, 1960), pp. 63-111; R. C. Dillehay, C. A. Insko, and M. B. Smith, "Logical Consistency and Attitude Change," *Journal of Personality and Social Psychology*, 3 (June, 1966), 646-654.

gestion causes cognitive reorganization for the maintenance of consistency.⁸⁴

c. People with inconsistent associative structures are easier to change by reinforcement.⁸⁵

d. Logical "mistakes" are often due to the use of different premises from the ones intended.⁸⁶

e. As previously mentioned (II, D, 1, b), there may be a Socratic effect in which a person who is asked to state his opinions on related issues changes his opinions to achieve logical consistency.

2. *Claim:* There is a great deal of culturally stored information and knowledge (especially in the language of a society as Whorf emphasizes).⁸⁷ Even beyond this culturally stored information, the accessibilities of schemata are determined in part by factors other than direct observation or interpersonally transmitted experience.

Evidence:

a. Linguistic and other types of cognitive development proceed in relatively fixed stages, which suggests that some types of general rules are easier to learn than others.⁸⁸

b. There seem to be some linguistic universals which are nonfunctional principles of grammar common to all languages. For example, grammatical operations apply in a cyclic fashion, first to the most deeply embedded structures, then to the structures that contain them, and so on.⁸⁹

c. There seems to be a working model of three-dimensional space within the human nervous system which provides the capacity to interpret visual images.⁹⁰

⁸⁴ Milton J. Rosenberg and C. W. Gardner, "Case Report: Some Dynamic Aspects of Posthypnotic Compliance," *Journal of Abnormal and Social Psychology*, 57 (November, 1958), 351-366.

⁸⁵ W. A. Scott, "Cognitive Consistency, Response Reinforcement, and Attitude Change," *Sociometry*, 22 (September, 1959), 219-229.

⁸⁶ Mary Henle, "On Error in Deductive Reasoning," *Psychological Reports*, 7 (August, 1960), 80.

⁸⁷ Benjamin L. Whorf, *Language, Thought and Reality*, ed. J. B. Carroll (Cambridge, Mass.: M.I.T. Press, 1956).

⁸⁸ For a review of linguistic development in children see Roger Brown, *Social Psychology* (Glencoe, Illinois: The Free Press, 1956). For a review of Piaget's work on cognitive development see John H. Flavell, *The Developmental Psychology of Jean Piaget* (Princeton: Van Nostrand, 1963).

⁸⁹ A recent formulation of the claim for linguistic universals which uses this example is Noam Chomsky, *Problems of Knowledge and Freedom* (New York: Pantheon Books, 1971). For a review of psycholinguistic research see Miller and McNeill, "Psycholinguistics."

⁹⁰ Attneave, "Multistability in Perception."

3. *Claim:* Schemata which are readily accessible for the interpretation of sets of relationships tend to have simple mathematical properties.

Evidence: There is empirical evidence for the accessibility of schemata with the following mathematical properties:

a. positive (see IV, D, 2, a above).⁹¹

b. symmetric.⁹²

c. transitive.⁹³

d. balanced (see IV, D, 1 and 3 above).

e. acyclic (no cycles, i.e., no set of relationships such that aRb , bRc , cRd , . . . , xRy , yRz , and zRa).⁹⁴

f. linearly ordered (irreflexive, asymmetric, transitive, and complete).⁹⁵

g. singly ordered (several dimensions collapsed into one linear order).⁹⁶

4. *Claim:* A general feature of schemata is that a new case can be extended by using the parametric values of the specification of old relevant cases (which is a kind of generalization process).

Evidence:

a. Subjects systematically go beyond assuming attitudes which are sufficient to explain specific actions they observe, imputing to the actor attributes of trans-specific generality.⁹⁷

⁹¹ Concept formation experiments provide further indirect evidence. See, for example, Jerome S. Bruner, Jacqueline J. Goodnow and George A. Austin, *A Study of Thinking* (New York: Wiley, 1956).

⁹² For example, DeSoto and Kueth, "Perception of Mathematical Properties"; DeSoto and Kueth, "Subjective Probabilities"; and DeSoto, "Learning a Social Structure."

⁹³ For example, DeSoto and Kueth, "Perception of Mathematical Properties"; DeSoto and Kueth, "Subjective Probabilities"; and DeSoto, "Learning a Social Structure."

⁹⁴ Henley, Horsfall and DeSoto, "Goodness of Figure," Axelrod, "Psycho-Algebra."

⁹⁵ Henley, Horsfall and DeSoto, "Goodness of Figure"; Clinton DeSoto and Frank Albrecht, "Conceptual Good Figures," in *Theories of Cognitive Consistency*, Abelson et al., pp. 504-511; and Clinton DeSoto and Frank Albrecht, "Cognition and Social Orderings," in *Theories of Cognitive Consistency*, Abelson et al., pp. 531-538; Jean Pierre Poitou and David Van Kreveld, "Is There a Position Bias in Learning of Influence Structures?" *European Journal of Social Psychology*, 2, No. 1 (1972), 75-85. For a related schema called proximity, see Edmund R. Peay, "Extensions of Clusterability to Quantitative Data with an Application to the Cognition of Political Attitudes" (Ph.D. diss., University of Michigan, 1970).

⁹⁶ Clinton B. DeSoto and John J. Bosley, "The Cognitive Structure of a Social Structure," *Journal of Abnormal and Social Psychology*, 64 (April, 1962), 303-307; DeSoto and Albrecht, "Conceptual Good Figure," and DeSoto and Albrecht, "Cognition and Social Orderings."

⁹⁷ C. Norman Alexander, Jr., and Joyce Epstein,

b. Subjects accept verbal generalizations even when they know of exceptions to them.⁹⁵

B. Unanswered Questions

Among the questions which need to be answered in order to construct a more precise theory of schemata are the following:

1. How does one set the threshold for the level of goodness of fit which is regarded as providing a satisfactory interpretation of a case by a given schema? (See V, C, 1 below on the role of personality and VI, B below on the role of the reliability of the initial information.)

2. If an old interpretation of a case is canceled, is the old schema made less accessible?

3. When an old interpretation does not fit new information, is the blame ever divided between the two (see II, B, 4 above)?

4. Can goodness of fit be treated as a metric (distance function) on the abstract space of specifications of cases (see VI, B below), and if so what are the characteristics of this metric?

5. When and how does a person invent an entirely new schema?

C. Psychological Implications

Some psychological results which might be dealt with by an expanded version of the present model are the following:

1. Personality variables. Individual differences in cognitive complexity,⁹⁹ authoritarianism,¹⁰⁰ intolerance for ambiguity¹⁰¹ and autism/realism¹⁰² might be due to different thresholds of minimum goodness of fit. For example, authoritarians are likely to resist mildly derogatory information about a respected partner,¹⁰³ thereby maintaining an accessible interpretation at the cost of a good fit.

2. Cognitive dissonance. Dissonance effects

"Problems of Dispositional Inference in Person Perception Research," *Sociometry*, 32 (December, 1969), 381-395.

⁹⁸ Robert P. Abelson and David E. Kanouse, "Subjective Acceptance of Verbal Generalization," in *Cognitive Consistency*, ed. Shel Feldman (New York: Academic Press, 1966), pp. 171-197.

⁹⁹ James Bieri and Edward Blacker, "The Generality of Cognitive Complexity in the Perception of People and Inkblots," *Journal of Abnormal and Social Psychology*, 53 (July, 1956), 112-117.

¹⁰⁰ Brown, *Social Psychology*.

¹⁰¹ Else Frenkel-Brunswik, "Intolerance of Ambiguity as an Emotional and Perceptual Personality Variable," *Journal of Personality*, 18 (September, 1949), 103-143.

¹⁰² Theodore M. Newcomb, "Stabilities Underlying Changes in Interpersonal Attraction," *Journal of Abnormal and Social Psychology*, 66 (April, 1963), 376-386, esp. p. 385.

¹⁰³ Ivan D. Steiner and Homer H. Johnson, "Authoritarianism and 'Tolerance of Trait Inconsistency,'" *Journal of Abnormal and Social Psychology*, 67 (October 1963), 388-391.

include the result that when a boring task is voluntarily chosen by a subject, the less the reward, the *more* the task is valued. This type of result might be attributable to the accessibility of a schema incorporating the consistency of self-behavior.¹⁰⁴

3. Wishful thinking. The correlation of one's expectations with one's desires¹⁰⁵ might reflect an accurate generalization of past experience even when it seems inappropriate in an artificial setting.

VI. Applications in Normative Analysis

An information-processing model can be very helpful for suggesting new questions for further research as well as for integrating a variety of findings from experimental and social psychology. But from the viewpoint of political science, perhaps the greatest benefit from this type of model will come from its ability to provide a framework for the analysis of actual behavior in important arenas of human action. If we are able to identify the characteristics of optimal performance and if we are able to compare actual performance to optimal performance, then we can both improve our understanding of people's actual performance and perhaps offer some helpful advice to those who wish to improve their own performance.

The process of selecting a schema with which to interpret a case offers two opportunities for this type of analysis. The selection process within the model is a satisficing procedure which does not necessarily yield the best possible fit between the selected schema and the information available about the current case—nor should it necessarily, as will be shown. In addition to the information about the current case, the best use of the satisficing procedure requires at least two types of parameters. These are (1) the relative accessibilities of alternative schemata and (2) the magnitude of what constitutes a satisfactory fit. Analyzing how these two parameters should be set will help to identify the characteristics of optimal performance.

A. Optimal Accessibilities of Schemata

A particularly illuminating question for normative analysis using schema theory is how to

¹⁰⁴ Leon Festinger, *A Theory of Cognitive Dissonance* (Stanford: Stanford University Press, 1957); Jack W. Brehm and A. R. Cohen, *Explorations in Cognitive Dissonance* (New York: Wiley, 1962); and Zajonc, "Cognitive Theories in Social Psychology."

¹⁰⁵ Hadley Cantril, "The Prediction of Social Events," *Journal of Abnormal and Social Psychology*, 33 (1938), 364-389; D. McGreor, "The Major Determinants of the Prediction of Social Events," *Journal of Abnormal and Social Psychology*, 33 (1938), 179-204; and McGuire, "Cognitive Consistency and Attitude Change."

assign relative accessibilities to a given set of schemata. In other words, given a set of schemata, which one should be tested first against new information of a specific type of case, which second, and so on. The answer involves the tradeoff between two competing values which are both desirable in an accessible schema. These two valued characteristics of a schema are veridicality and smallness of size.¹⁰⁶

The *veridicality* of a schema is the subjective probability that a case of a specific type is in fact an instance of that schema. Obviously, the person wants to interpret new information as fitting a highly veridical schema since such an interpretation would make it likely (at least in the sense of subjective probability) that his modification and extension of the initial partial specification of the case would be accurate rather than erroneous.

The second value desired in an accessible schema is smallness of size. The *size* of a schema is the number of different complete specifications which are instances of the schema. For example, the size of the balance schema is given by the number of different ways of dividing a group of people into two groups (such as friends and enemies). The smaller the size of a schema, the more can be deduced about a case which is interpreted in terms of that schema.

The veridicality of a schema depends on the type of case (see II, D, 3), but the size of the schema is solely a function of the nature of the schema itself. A useful measure of the smallness of a schema is given by the information theory concept of redundancy.¹⁰⁷ For present

¹⁰⁶ For a formal model with a similar tradeoff, see Shun-ichi Amari, "Theory of Learning Decision Systems," in *Unifying Study of Basic Problems in Engineering and Physical Sciences by Means of Geometry*, ed. Kazuo Kondo (Division 1), (Tokyo: Gakujutsu Bunkai Fukyu-kai, 1969), pp. 33-42 (which is RAAG Memoirs, 4, [1969], 567-576).

¹⁰⁷ With the assumption that each instance of a schema has equal subjective probability, size determines redundancy. The assumption is based on the principle that if some instances have a higher subjective probability than others, they should be treated by the model as belonging to different schemata. For applications of redundancy to psychology see Garner, *Uncertainty and Struc-*

purposes it is sufficient to note that interpreting a case in terms of a small schema has the advantage of allowing a greater amount of modification and extension of the original partial specification of the case.

A numerical illustration will help make these ideas clear. Consider a set of binary relationships between five objects (such as friendship relationships between five people). Any complete specification of such a case can be represented as a 5 by 5 matrix with 1 and -1 entries (although for convenience a -1 will be shown as a blank in Figure 3 below). For example, the entry in the second row and fourth column could represent whether or not the second person likes the fourth person.

Using this representation, a schema can be defined as the set of all matrices which satisfy certain specified properties. The number of different complete specifications for a schema can then be counted to give the exact size of that schema. Examples of specifications which fit different schemata are shown in Figure 3. The order of the rows and columns has been chosen to best display the defining characteristics of the different schemata.

The null schema is the one which includes all possibilities. There are 5^2 or 25 entries in the matrix, so there are a total of 2^{25} or about one hundred million possible specifications of the null schema. One of these is shown in Figure 3a.

The balance (or 2-cluster) schema has a size of 16, since there are $2^5/2$ ways of putting 5 people into one of no more than two groups, when the order of the groups does not matter. An example is shown in Figure 3b.

Figure 3c displays one of the specifications of the 3-cluster schema. There are 41 ways of putting 5 people into one of no more than three groups ($41 = (3^5 - 3)/3! + 1$).

The positive schema has only one specifica-

ture; and Wendell R. Garner and David E. Clement, "Goodness of Pattern and Pattern Uncertainty," *Journal of Verbal Learning and Verbal Behavior*, 2 (December, 1963), 446-452. For a recent critical review see D. W. J. Corcoran, *Pattern Recognition* (Harmondsworth, England: Penguin Books, 1971).

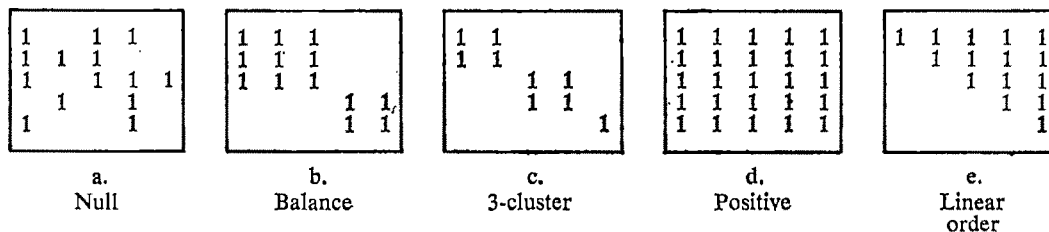


Figure 3. Sample Specifications of Schemata of Relationships Between Five People.

tion (shown in Figure 3d), since there is only one way that everyone can like each other.

The schema of linear order has 5! or 120 specifications since there are that many ways of arranging 5 people in order without ties. Such a schema would be appropriate if the case were about relationships of height rather than friendship. Figure 3e illustrates this schema with the order of the rows and columns chosen to display best the pattern.¹⁰⁸

The tradeoff between veridicality and smallness among schemata is illustrated in Figure 4 for a given type of case, namely friendship relationships. A highly accessible schema should be both veridical and small. The balance (or 2-cluster) schema is moderately small and has fairly good veridicality, since the person presumably knows that friendship groups do tend to be balanced.¹⁰⁹ The positive schema is very small indeed, but is not very veridical. The 3-cluster schema is assumed in this example to be more veridical than the positive schema and is certainly more veridical than the balance schema, but it is larger than either of them. The null schema is certain to be totally veridical, but it is no help in the modification and extension of a partial specification of a case, since it is so large as to include every possibility. The schema of linear order is of large size and is assumed to have low veridicality in this example since a linear order is asymmetric (if one individual has the relationship to a second, then the second can not have that relationship to the first), whereas actual liking relationships tend to be symmetric.

The dotted line of Figure 4 represents the frontier of knowledge about this type of case using these schemata. The growth of knowledge about a type of case could be reflected in the discovery of a schema which was above and to the right of this line and which thereby pushed out the frontier of knowledge.

Pushing out the frontier of knowledge may be one of the tasks of the social scientist, but the person who is actually trying to make sense out of a complex world is often more interested in just what he should do *today* when the frontier of knowledge is at a given place. The answer in terms of Figure 4 is that he should assign high accessibilities to schemata which are high and to the right in Figure 4, medium ac-

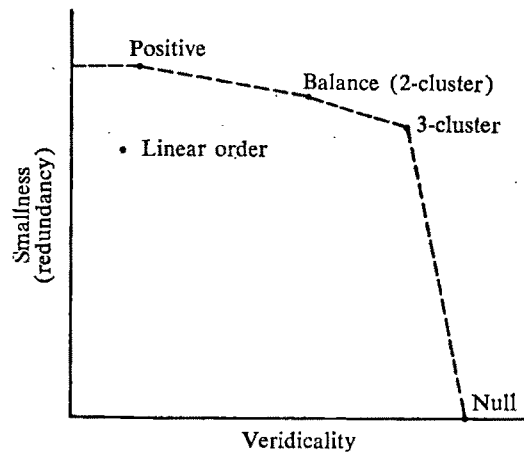


Figure 4. Illustration of the Tradeoff Between Smallness and Veridicality of Different Schemata for Cases of Friendship Relationships.

cessibilities to schemata which are in the upper left or lower right, and low accessibilities to schemata in the lower left.¹¹⁰

B. Optimal Threshold of an Acceptable Fit

Setting the relative accessibilities of schemata determines the order in which they will be checked for their fit to the current case. Given this order of checking, one may ask what should constitute a satisfactory fit to bring a successful end to the satisficing checking process? In other words, what should be the threshold of how bad a schema's fit can be and still be acceptable as the interpretation of the present case?

To deal with this question a geometric analysis is helpful. To start with, a complete specification of a case can be represented as a single point in an abstract space, to be called a Q -space. In this space there is a distance function (or metric) which measures how similar two complete specifications are to each other. Thus if x and y are two points in the Q -space, then $d(x,y)$ is a real number indicating the degree to which these two complete specifications are similar or dissimilar to each other.¹¹¹

¹⁰⁸ For schemata of infinite size (e.g., with continuous rather than discrete parameters), the appropriate measure of size uses degrees of freedom. For example, a linear equation has two degrees of freedom (namely a and b in $y = ax + b$), and a quadratic equation has three degrees of freedom (namely a , b and c in $y = ax^2 + bx + c$).

¹⁰⁹ Kogan and Tagiuri, "Interpersonal Preference and Cognitive Organization."

¹¹⁰ To carry the analysis further one could do the type of indifference curve analysis used in microeconomics. Negatively sloped indifference curves could be drawn in Figure 4 to represent the tradeoffs between smallness and veridicality. Then the schemata should be given accessibility in order of their intersection with the ordered set of indifference curves. If there are enough alternative schemata to allow for a continuous approximation of the frontier of knowledge, then the schema which should be given the highest accessibility is represented by the point at which one of the indifference curves is tangent to the frontier of knowledge.

¹¹¹ A metric function has the following properties:

The distance function can be applied to pairs of sets in Q by taking the minimum distance between members of the two sets. For example, let X be a partial specification of a case (i.e., a subset of Q consisting of all complete specifications which are consistent with the information in the partial specification), and let S be a schema (so that S is also a subset of Q by definition). Then define $d(X, S) = \min d(x, y)$ for x in X and y in S . Less formally, the distance between a partial specification and a schema is how close they come together.¹¹²

There are four immediate uses to which the distance function can be put. First, goodness of fit can be defined as the distance between an initial partial specification and a schema, namely $d(X, S)$. Second, a satisfactory fit can be defined by the condition that this distance is less than or equal to k for some fixed k . Third, the selected schema is the first schema such that $d(X, S_i)$ is less than or equal to k , where S_1 is the most accessible schema, S_2 is the second most accessible schema and so on. Fourth, the modification and extension of a partial specification is a set of points, X' , defined in the following manner. X' is the set of all x' in the selected schema, S' , such that $d(X, \{x'\}) = d(X, S')$. In other words, the modified and extended specification of a case is the set of all points in the selected schema which are at minimal distance from the initial partial specification. The idea is that the initial specification is altered as little as possible in order to make it fit the selected schema.

An abstract illustration of these ideas is provided by Figure 5. In this figure there are three schemata labeled in order of their accessibility, S_1 , S_2 and S_3 . There is also a partial specification of a case, X . Shown near the bottom is a scale of the distance, k , which is the maximum allowable for a satisfactory fit. (For convenience here, the metric in Figure 5 can be thought of as a normal Euclidean distance, but other metrics are certainly possible.)

Some of the key operations of the information-processing model can be illustrated with the help of this geometric example. The satisficing routine proceeds as follows. S_1 is not selected because all of its points are too far from any of the points in X . S_2 is selected as the interpretation of the case because some of the points in X are within a distance of k from some of the points in S_2 . S_3 is never even checked by the satisficing subroutine and there-

$d(x, y) = d(y, x)$; $d(x, y) \geq 0$; $d(x, y) = 0$ if and only if $x = y$, and $d(x, y) + d(y, z) \geq d(x, z)$.

¹¹² Although the distance between points is a metric function as defined in the previous footnote, the distance between sets is not.

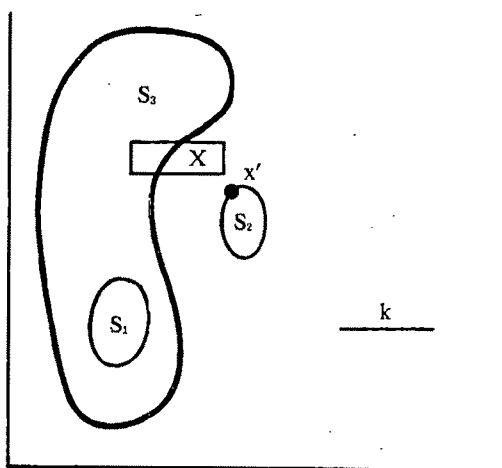


Figure 5. Illustration of the Q Space.

X is the initial partial specification of the case

S_1 , S_2 and S_3 are schemata

k is the maximum distance of a satisfactory fit

x' is the final specification of the case using the selected schema, S_2

fore cannot be selected even though S_3 provides the best fit in this example. The specification routine proceeds by picking the points in the selected schema, S_2 , which are closest to X . In this example, there is a unique such point, x' , and thus the specification process results in a complete specification of the case as well as an interpretation of the case as belonging to the S_2 schema.

The familiar case type of friendship relationships helps demonstrate some of the properties a Q -space can have. For simplicity consider a set of friendship relationships among three people which take on one of only two values (i.e., like and dislike). Assume for a moment that the relationships are reflexive (everyone likes himself) and symmetric (everyone's feelings are reciprocated). Then the Q -space is three-dimensional, with the dimensions representing the three relationships between A and B , B and C , and A and C . This space has exactly 2^3 or 8 points. The schema of balance is a set of exactly four points in this space, namely $(1, 1, 1)$, $(1, -1, -1)$, $(-1, -1, 1)$ and $(-1, 1, -1)$.

This particular type of Q -space can be applied to the line hypothesis of Abelson and Rosenberg, which forms the basis of what is known as the theory of cognitive balance.¹¹³ Their line hypothesis can be regarded as simply a special case of the specification routine of the present model for the situation in which the selected schema is balance and the distance func-

¹¹³ Abelson and Rosenberg, "Symbolic Psycho-logic."

tion is the so-called city block metric (in which the distance between two points is the arithmetic sum of the separate distances separating them on each dimension of the Q -space).

As another example, suppose that the relationships are still reflexive and symmetric but are now continuous (to allow degrees of liking and disliking). The Q -space is still three-dimensional, but now has an infinite number of points. The schema of balance is now the set of points of the pyramid that has for its four corners the four balanced points listed above.¹¹⁴

With this geometric background, it is easy to see the implication of changing the size of the maximum distance of a satisfactory fit, k . The smaller the maximum distance for a satisfactory fit, the greater the chance that the more accessible schemata will be rejected and a less accessible (or no) schema will be selected.

In a domain which has precise measurement (such as one of the natural sciences), it is best to set k relatively low. The reason is that if a highly accessible schema gives a mediocre fit in such a situation, it is best to keep searching for a better fit since it is unlikely that random error alone would be responsible for the discrepancy.¹¹⁵

Conversely, in a domain which has imprecise information (such as is common in politics) it is best to set k relatively high so that an accessible schema is not rejected when the observed discrepancy could easily have been due to random error.¹¹⁶

¹¹⁴ Wiest, "A Quantitative Extension."

¹¹⁵ Signal detectability theory treats the situation in which there is only one accessible schema. See Coombs, Dawes, and Tversky, *Mathematical Psychology*.

¹¹⁶ Geometric analysis of this section can be extended to yield a number of suggestive theorems about some of the more subtle consequences of the present information processing model. Here are four such results.

(1) *Potential Completeness*. If Q is a continuous space, then under a weak set of assumptions the modified and extended specification of a case will be a complete specification of the case. This corresponds to the situation in which the person can make an estimate about every feature of the case (e.g., every friendship relationship). For example, in Figure 5 the set of points in S_2 which are minimally distant from X is a single point, x' , and is therefore a complete specification.

(2) *Potential Enlargement*. Under certain circumstances the modified and extended specification can actually be larger than the initial partial specification. This occurs when there are few points in the partial specification, but more points in the selected schema which are at the minimum distance from the initial partial specification than there were in the initial partial specification itself. In such a situation the person has a schema which he can use to interpret the case, but he does not end up with a single unique specification of the case (even if he started with one). He believes something is wrong with the initial information, but he can not pin down what it is, even though he has selected a schema with which to interpret the case.

VII. Illustrative Applications to Politics

Now for a few illustrations to show that the model of how people make sense out of their complex worlds can be directly relevant to political phenomena. These examples are illustrative only, since the range of the potential political applications seems almost to coincide with the range of political science. Because my own interests are primarily in international relations and foreign policy formation, most of the examples are taken from that domain. The reader is invited to see if his favorite political actors try to make sense out of their own complex worlds, and if so whether it makes any difference how they try to do it, and how they might do it better.

Before turning to the illustrations, several points should be made about the operationalization of the schema theory. As a model of what happens in a controlled setting, the schema theory has already been operationalized by the same operations which have been used in the various experiments upon which the theory is based. For example, all of the standard ways of eliciting a subject's beliefs can be used to measure the details of the person's modified and extended specification of a case. As a model of what happens in a political arena, somewhat different measurement techniques are needed. For example, the problems of measuring the beliefs of a political actor are certainly difficult, but hardly unique to this model. Once again, there are a set of standard techniques which can be used to measure beliefs, this time involving the analysis of interviews, questionnaires, documents produced by the actor, and the actor's actual decisions. For the parameters of the model which are not "directly observable," the system level characteristics of the model itself provide alternative indicators that can be used as cross-validating measurement

(3) *Cancellation Effect*. Under a wide range of conditions, an initial partial specification with continuous parameters will be modified to achieve a fit to the selected schema by leaving most of the parameters unchanged and by giving the rest neutral values. This can be called the cancellation effect since the only changes in the modification are neutralizations. The cancellation effect is a consequence of the principle that the modification is made in such a way as to move the least distance to the selected schema.

(4) *Seriatim Effect*. Under a wide range of circumstances, the sequential presentation of the same information can reduce the amount of attitude change compared to a simultaneous presentation. The seriatim effect is due to the principle that a message is first evaluated for how well it fits into a previous interpretation of the case. Presenting discrepant information a little at a time can result in each message being blamed as it is received, whereas if the discrepant information were presented all at once, the blame might be affixed to the old interpretation.

techniques. A good example of the potential for cross-validation is schema accessibility which can be indirectly measured with any of four methods (see III, E).

While waiting for independent evaluations of the model in different political arenas, one can hopefully expect that the literature on perception (and perhaps cognition) will generalize quite well to field studies. People may bargain and exercise power differently in a laboratory than they do in a "real life" situation, but they probably use the same thought processes no matter where they are. This is another way of saying that perception and cognition may be more fundamental psychological processes than bargaining and the exercise of power. Because they may be more fundamental, they probably generalize better from one context to another. But even before the schema theory is empirically evaluated in political contexts, it can serve well heuristically by suggesting new ways of looking at some old political problems. In this spirit the illustrative examples are offered.

A. Conceptual "Models" of One's Adversaries

The first example is a direct application of the normative analysis of schema accessibilities (VI, A). As Allison points out, most foreign policy analysts explain and predict the behavior of national governments in terms of a rational policy "model."¹¹⁷ He also points out that more sophisticated "models," such as an organizational process "model" and a bureaucratic bargaining "model," can be formulated.

If these alternative "models" are treated as schemata which include some possibilities and exclude others, then the present information-processing model can be applied. In particular, the question of which schema should be given highest accessibility can be analyzed. As has been discussed in Part VI, A, there is a tradeoff between improved veridicality of a schema and smallness of size. The improvement of the conditions of the tradeoff comes from improved knowledge of the environment (e.g., by discovering a schema which is both small and veridical, a process that is similar to discovering a theory which is both parsimonious and accurate).

In the meantime (at any given level of knowledge about the environment) the conditions of the tradeoff have this implication: it may be better for the analyst to give higher accessibility to a small but "simple minded" schema (such as one based on the rational pol-

icy "model") rather than to a more sophisticated but larger schema.

B. Individual Belief Systems

The second example deals with individual belief systems. Like all political actors, a foreign policy maker has his own characteristic belief system which helps shape his perceptions and therefore his decisions. His belief systems can be studied through an analysis of speeches, documents, and interviews. For example, John Foster Dulles followed his usual mode of analysis when he interpreted a large cut in the size of the Russian Army as being due to Russian economic weakness, as possibly leading to an increased production of atomic weapons, and as not lessening world tensions.¹¹⁸ Thus even a large cut in the Russian Army did not disconfirm Dulles's interpretation of Soviet policy.

Perhaps this is no more than an unusually blatant example of a rather common problem. I conjecture that the schemata people use are often larger than they think, and therefore less easy to disconfirm. When they are actually larger than they think, people are more confident in their own interpretations of past cases and in their prediction of future events than they have a right to be.

C. Learning Lessons from History

People often interpret a current case as being analogous to a previous case which was in some ways similar. For example, President Johnson argued that the policy problem of Vietnam was similar to the policy problem of Munich. Historical analogies seem to be drawn by selecting the single previous case which provides the satisficing fit to the present case. This process is therefore a special case of the present model.

The more important to the person a previous case was, the more accessible it will be as an analogy for a future case.¹¹⁹ Thus "Munich" is a highly accessible analogy, since it is associated with the origins of World War II. This dependence of the accessibility of a prior case on its importance is helpful to the decision maker if (but only if) there is actually a correlation between the importance of a case and the likelihood that it will be a useful guide to future decisions.

D. The Intelligence Game

The intelligence/deception interaction is one of the most fascinating aspects of international

¹¹⁷ Graham T. Allison, "Conceptual Models and the Cuban Missile Crisis," *American Political Science Review*, 63 (September, 1969), 689-718.

¹¹⁸ Ole R. Holsti, "The Belief System and National Images: A Case Study," *Journal of Conflict Resolution*, 6 (September, 1962), 244-252.

¹¹⁹ Axelrod, *Framework for a General Theory of Cognition and Choice*.

relations. In terms of the model of how people make sense out of a complex world, three factors make it relatively easy for an intelligent actor (A) to be deceived by another intelligent actor (B) about B's future decisions. First, knowledge and information about the internal decision-making process of B is not well-known to A, and therefore no schema can be both small and veridical. Thus extensions of specifications are not likely to be reliable. Second, actor B can deliberately behave so as to make predictions even harder for A. For example, he can build up a record of consistent behavior and then break the pattern at a crucial moment (which is exactly what the British did to the Germans in the D-Day deception by building up the reliability of double agents under British control.)¹²⁰ Third, if actor B knows about the relative accessibility of A's schemata, and if B knows about the type of information available to A on each case, then B can adjust his behavior to increase the likelihood that his behavior will be interpreted by A as fitting a particular schema.

Actor A in trying to predict a future decision by actor B can adopt special tactics to deal with these problems. He can try to avoid being deceived by relying on information which B does not control or which is costly for B to manipulate. Which information has these characteristics, however, is also a matter for A's interpretation, and is therefore subject to the countermeasures by B discussed above. For example, in World War II the Germans trusted the spies they sent to Britain when in fact the British soon controlled every one of them.¹²¹

E. *The Slow Growth of Knowledge in the Policy Sciences*

In a policy domain each interpretation and decision is important to the actors involved. Be-

cause of this, the application of a policy science may develop a set of doctrines. In some cases, such as medicine in traditional societies, these doctrines provide a widely accepted interpretation (diagnosis) and decision in each possible type of case.¹²² Such a set of doctrines has three functions. It guarantees that each and every case will get the best interpretation and decision given the current state of the shared knowledge. It promotes confidence on the part of the practitioner and trust on the part of the client, and this confidence and trust can be important in securing results in a policy domain. Finally, the set of doctrines protects the practitioner from charges that he has made a mistake if a particular decision turns out badly.

On the other hand, if the set of doctrines is followed in every instance there will be no experimentation and therefore little gathering of information on cases which might disconfirm current theories. For example, in a disease which often has spontaneous remission (such as peptic ulcer), false beliefs about cures can easily arise and be maintained. This is like the dog (mentioned in III, C, 1, above) who got so good at jumping to avoid a shock that he could not discover when the shock mechanism was turned off.¹²³

An exclusive focus on getting the best outcome in each case can therefore retard the growth of discovery of smaller, more veridical schemata. By retarding the growth of knowledge, an exclusive focus on getting the best result in each case can prove a poor tactic in the long run. Therefore, practitioners of policy sciences (such as international relations) should value deliberate experimentation in suitable circumstances. Their clients (such as electorates in democracies) should be willing to tolerate a lower average success in the short run in order to attain a higher level in the long run.

¹²⁰ J. C. Masterman, *The Double-Cross System in the War of 1939 to 1945* (New Haven and London: Yale University Press, 1972).

¹²¹ Masterman, *The Double-Cross System*.

¹²² Joseph Ben-David, *The Scientist's Role in Society* (Englewood Cliffs, New Jersey: Prentice-Hall, 1971), pp. 25f.

¹²³ Solomon, Kamin and Wynne, "Traumatic Avoidance Learning."

Human Rights Without Discrimination

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The Charter of the United Nations requires members to promote human rights "without distinction as to race, sex, language, or religion." In principle, the requirement makes members internationally accountable for policies traditionally regarded as purely domestic. More than that, it offers a basis for the global development of standards for the equal and just treatment of individuals, and thus a basis for action contributing to the formation of a community of mankind.

So far, the record of the members is quite mixed with respect to action at the international level to promote implementation of the requirement. The General Assembly has adopted a number of declarations, conventions, and covenants spelling out human rights, thus reducing the vagueness of the Charter provision; but the record of members in ratifying the conventions and covenants is hesitant, the United States ratifying very few.¹ All of the principal organs of the United Nations as well as many of the associated independent agencies have been or are involved in the effort to obtain greater respect for human rights in Southern Rhodesia, South Africa, and Namibia. But the preoccupation with violations of human rights in these areas is accompanied by substantial neglect of violations elsewhere.

Nevertheless the fact that implementing action occurs means that the requirement has significance; and especially in the light of increasing concern through much of the world with ethnic and racial problems and with the status of women, an analysis of its meaning and potentialities is in order. That is what will be attempted here. The first question is whether "without distinction" means what it seems to say, that is, whether it means "without any differentiation." If it does mean this, then separate toilet facilities for men and women apparently

violate the obligation, and so do separate sleeping arrangements for male and female prisoners. If "without distinction" does not mean what it seems to say, then what does it mean? The second question is whether the naming of race, sex, language, and religion means that "distinction" on other bases remains permissible.

The answer to these questions turns out to be that "without distinction" means "without discrimination," and that what is ruled out is discrimination of *any kind* that affects human rights. And thus the third question concerns the implications of an international prohibition of discrimination. More broadly, the third question is whether the search for equality has not been internationalized, at least in principle, and what the implications of the internationalization might be.

"Without Distinction" and "Without Discrimination"

According to one of the common meanings, to make a distinction is to distinguish or to differentiate. It is thus natural to suppose that the "without distinction" requirement of the Charter means "without differentiation": members are to promote human rights "without differentiation as to race, sex, language, or religion." But the adoption of this meaning leads quickly to implications and consequences that could scarcely have been intended. Many kinds of differentiations on the bases named are commonly regarded as quite acceptable, if not desirable and necessary. The United Nations itself makes distinctions as to language, selecting "official" and "working" languages, and the choice entails relative advantage to some countries and delegations and disadvantage to others. The United Nations Commission on Human Rights has a Sub-Commission on Prevention of Discrimination and Protection of Minorities, which implies that, while "discrimination" is to be prevented, minorities may be distinguished and protected. When the General Assembly recommended the partition of Palestine, it surely intended to make distinctions as to language and religion, and the carving of Pakistan out of colonial India surely also reflected a distinction as to religion. Thus the very *raison d'être* of some states, or of the location of specific international boundaries,

¹ Among the instruments adopted by the General Assembly are the Universal Declaration of Human Rights (1948), the Genocide Convention (1948), the Convention on the Elimination of All Forms of Racial Discrimination (1965), the Covenant on Civil and Political Rights (1966), and the Covenant on Economic, Social, and Cultural Rights (1966). The United States has not ratified any of these conventions or covenants. It has ratified a Supplementary Slavery Convention. On the policies of the United States, see Vernon Van Dyke, *Human Rights, the United States, and World Community* (New York: Oxford University Press, 1970), pp. 129-156.

would be contradicted if "without distinction" were interpreted to mean "without differentiation." Moreover, such an interpretation would presumably rule out reverse or benevolent discrimination based on race or sex. And it would presumably mean that in multilingual communities, children could not be assigned to school on the basis of distinctions as to language.

If "without distinction" can scarcely mean "without differentiation," then what does it mean? A clue is provided in the name of the Sub-Commission on Prevention of Discrimination and Protection of Minorities. What is to be prevented is not *distinction* in the sense of differentiation, but *discrimination*. In fact, in many international instruments the terms *distinction* and *discrimination* are used indiscriminately. The Universal Declaration of Human Rights says "without distinction" in Article 2 and "without any discrimination" in Article 7.² The Covenant on Civil and Political Rights says "without distinction" in Article 2, and the comparable article in the Covenant on Economic, Social and Cultural Rights says "without discrimination." The Declaration on the Rights of the Child says "without distinction or discrimination."

A comparison of usage in English and in French yields further clues. The Treaty of Versailles called for a treaty between Poland and the Free City of Danzig which, among other things, was "to provide against any discrimination within the Free City of Danzig to the detriment of citizens of Poland and other persons of Polish origin or speech." The French version renders "any discrimination" as "*aucune discrimination*," but then the treaty that Poland and Danzig concluded (like all the minority treaties after World War I) uses the expression "without distinction" in English and "*sans distinction*" in French. The European Convention on Human Rights, signed in 1950, says "without discrimination" in the English version and "*sans distinction aucune*" in the French (Article 14). The Covenant on Economic, Social, and Cultural Rights, mentioned above, says "without discrimination" in English and "*sans distinction injuste*" in French.

Confusion about "distinction" and "discrimination" no doubt arises partly because *distinction* is the traditional French word, *discrimination* being a relatively recent addi-

tion to the French language.³ The French Declaration of the Rights of Man said "*sans distinction*," and not "*sans discrimination*." It proclaimed the principle that all should be equal in the eyes of the law and equally eligible to all honors and governmental positions, according to their ability, "*sans autre distinction que celle de leurs vertus et de leurs talents*." The *Grand dictionnaire universel du XIX^e siècle* (Larousse, 1870) does not contain the word *discrimination*. The *Larousse du XX^e siècle* (1929) includes the word, but defines it simply as "capacity to discern, to distinguish," whereas it illustrates the meaning of *distinction* with the statement that justice shall be administered "*sans distinction*." Similarly, the *Dictionnaire de l'Académie française* (1932) includes *discrimination*, defining it simply as "*action de distinguer avec précision*." The *Grand Larousse encyclopédique* (1961) offers approximately the same definition, and then adds "*discrimination raciale*," referring the reader to *racisme* and *ségrégation*. Only with the *Robert Dictionnaire alphabétique* (1967) does *discrimination* appear with a general meaning that is relevant here: separate and adverse treatment.

Presumably the use of *distinction* in French has influenced the choice of words in English, and now the use of *discrimination* in English is having a comparable influence on the choice of words in French. Everything suggests that *distinction* and *discrimination* have become synonyms. René Cassin took this view in 1948, assuring the United Nations Commission on Human Rights that "there was no difference of substance between the words 'distinction' and 'discrimination' in French."⁴ In 1962, when misgivings were expressed in the Third Committee of the General Assembly about the word *distinction*, an understanding was informally reached that it should not be construed to prohibit "the adoption of legitimate measures designed precisely to safeguard and promote the rights of certain categories of persons." In particular, "protective measures taken by various States for the benefit of certain socially and educationally backward groups of the population were aimed at re-establishing equality and could not therefore be violations [of the 'without distinction' requirement]."⁵ By general con-

² The Universal Declaration and most of the other international documents on human rights cited herein are conveniently available in *Basic Documents on Human Rights*, ed. Ian Brownlie (Oxford: Clarendon, 1971).

³ Cf. European Court of Human Rights, Series B: Pleadings, Oral Arguments, and Documents. Case "Relating to Certain Aspects of the Laws on the Use of Languages in Education in Belgium" (Strasbourg, 1967), Vol. II, pp. 79-80. (Hereafter cited as Eur. Court H. R., Series B, "Linguistic" Case.)

⁴ E/CN.4/SR.53.

⁵ A/5365, paras. 57-58.

sent, then, the requirement can be read as if the reference is to discrimination.⁶

Both the European Commission of Human Rights and the European Court of Human Rights take this same view in interpreting the European Convention, where (as indicated above) "without discrimination" in English becomes "*sans distinction aucune*" in French. Both denied that *discrimination* and *distinction* have different meanings in the given context. More specifically, they declared that it would lead to "absurd results" if "*sans distinction*" were interpreted to preclude differentiation, and they pointed to the many distinctions that are in fact accepted without being considered discriminatory, some of them designed to promote equality rather than to deny it.⁷ It is generally agreed, for example, that in multilingual communities it is permissible to distinguish between children by language so as to provide mother-tongue instruction.

"As to Race, Sex, Language, or Religion"

Now the question is whether the naming of race, sex, language, and religion means that discrimination is permissible as long as it occurs on other grounds. This question is perhaps not as significant as the first, since the grounds named are the ones that arouse the most objection. Further the answer cannot be quite as confident. Nevertheless, the question is important enough to justify a brief inquiry. Its importance is suggested by the increased attention given to economic factors in the United States in connection with the equal protection of the laws. Thus impecunious defendants are entitled to various forms of public assistance; and the question exists whether school children are not denied equal protection of the laws if their education depends on a tax base that differs considerably in various school districts.

International instruments other than the Charter differ in the breadth of their attack on discrimination. Some are narrower than the Charter, focusing, for example, on discrimination based on race or on sex. Others are broader. The Universal Declaration of Human Rights, adopted by the General Assembly three

years after the Charter came into effect, says "without distinction of *any kind* [my italics], such as race, colour, sex, language, religion, political or other opinion, national or social origin, property, birth or other status." Precisely the same words appear in the two Covenants, except that the Covenant on Economic, Social, and Cultural Rights (as already indicated) says "without discrimination" instead of "without distinction." In addition to the phrase quoted, the Universal Declaration goes on to say that "all are equal before the law and are entitled without any discrimination to equal protection of the law." Virtually the same words appear in the Covenant on Civil and Political Rights (Article 26), and it adds that "the law shall prohibit any discrimination and guarantee to all persons equal and effective protection against discrimination *on any ground* [my italics]. . . ."

Domestic constitutions and laws also differ in the breadth of their attack on discrimination. Some are like the Charter in ruling out a few potential grounds of discrimination; race and religion are more commonly named than sex and language. Others are like the Universal Declaration and the Covenants, ruling out discrimination in general or providing affirmatively for the equal protection of the law. The constitution of the United States is, of course, in the broader category.

The issue, then, is open to argument. Should lists of forbidden grounds of discrimination be presumed to be exhaustive? The best argument is in the negative. With respect to the Charter, the argument is that nondiscrimination is implied in the obligation to promote human rights "for all," and that the "without distinction" phrase is simply a partial and nonrestrictive elaboration of a meaning that is there in any case. This is borne out in the wording of the Universal Declaration, which regularly refers to "all," to "everyone," and to "no one." The Covenants contain similar language. Moreover, many of the domestic constitutions that list forbidden grounds of discrimination also provide affirmatively for equality or the equal protection of the laws, which suggests that the list was not intended to be exhaustive or restrictive. When such lists are made, as in the Charter, what apparently happens is that those grounds get included that are currently arousing the most resentment or objection; the purpose in naming them is to make it doubly sure that they are outlawed. But there is no reason to suppose that the authors of the lists intended them to be exhaustive; and even if there were doubt on this point, those who come along later are reasonably free to choose among the per-

⁶ Cf. W. A. McKean, "The Meaning of Discrimination in International and Municipal Law," *British Yearbook of International Law*, Vol. 44 (1970), 180.

⁷ Eur. Court H. R., Series B, "Linguistic" Case, Vol. II, 80. European Court of Human Rights. Series A: Judgments and Decisions, 1968. Case "Relating to Certain Aspects of the Laws on the Use of Languages in Education in Belgium" (Merits). Judgment of 23 July 1968 (Strasbourg, 1968), p. 34. (Hereafter cited as Eur. Court H. R., Series A, "Linguistic" Case, Judgment of 23 July 1968.)

missible interpretations, adopting the one that contributes most constructively to a desirable social and political order, domestic and global.

What Constitutes Discrimination?

If human rights are to be promoted without discrimination on any ground, it is important to know what constitutes discrimination. A thorough answer would be broad and complex, for relevant thought has many aspects. The question has been much explored already. What will be attempted here is a sketch of an answer, oriented toward developments at the international level. The main object is to indicate how significant the rule of nondiscrimination potentially is for the development of a community of mankind. Whether the potentiality is ever realized is another matter.

Negative and Positive Claims to Equality. It is essential to relate the idea of discrimination to the idea of equality, and helpful to think of claims to equality as either negative or positive. Claims to nondiscrimination are negative claims to equality. It is the negative claims that have been stressed in the western tradition. Thus Benn and Peters say,

The demand for equality has never been intended in practice as a general plea that all men be treated alike. Egalitarians have always been concerned to deny the legitimacy of certain sorts of discrimination resting on some given differences, i.e., they have challenged established criteria as unreasonable, and irrelevant to the purposes for which they were employed. Claims to equality are thus, in a sense, always negative, denying the propriety of certain existing inequalities.⁸

If claims to equality are "always negative" in the western countries, it is not surprising that negative expressions of the claim would be prominent at the international level. But affirmative claims are also recognized. Obligations are commonly qualified so as to permit affirmative action on behalf of equality, whether the action be protective, compensatory, or corrective; and sometimes affirmative action is required. In truth, both the negative and the positive aspects of the matter show up in the Charter itself, the "without distinction" requirement being the negative and the obligation to "promote" human rights, which include the equal protection of the laws, being positive.

The dual aspect of the matter shows up in an opinion of the Permanent Court of International Justice in 1935 in a case concerning the

right of the Greek minority community in Albania to have its own schools. Advising on the matter the Court pointed to the possibility that nominal equality in law might result in inequality in fact. "Equality in law," it said, "precludes discrimination of any kind; whereas equality in fact may involve the necessity of different treatment in order to attain a result which establishes an equilibrium between different situations."⁹ The relevant point here is that "different treatment" is not necessarily discriminatory; it may be a response to a positive claim to equality.

The negative and positive approaches to equality show up in many other connections. They are suggested by the very name of a sub-commission of the UN Commission on Human Rights: the Sub-Commission on Prevention of Discrimination and Protection of Minorities. In 1947 the Sub-Commission adopted statements defining key words in its name:

1. Prevention of discrimination is the prevention of any action which denies to individuals or groups of people equality of treatment which they may wish.
2. Protection of minorities is the protection of non-dominant groups which, while wishing in general for equality of treatment with the majority, wish for a measure of differential treatment in order to preserve basic characteristics which . . . distinguish them from the majority of the population.¹⁰

The two approaches appear in many other connections. In the case of indigenous peoples, it is a long-established rule that no discrimination against them shall occur but that, on the contrary, special measures for their protection and advancement are at least permissible if not required. In 1962 the Sub-Commission on Prevention of Discrimination and Protection of Minorities formulated a set of "General Principles on Freedom and Non-Discrimination in the Matter of Political Rights."¹¹ It reaffirmed the "without distinction" rule, went on to affirm various political rights, and then listed "measures which shall not be considered discriminatory." Among the latter are

special measures to ensure: (i) the adequate representation of an element of the population of a

⁸ Permanent Court of International Justice, XXXIVth Session, 1935, Series A./B, Judgments, Orders, and Advisory Opinions, Advisory Opinion of April 6th, 1935, Fascicule No. 64, *Minority Schools in Albania*, p. 19.

¹⁰ United Nations, Commission on Human Rights, Sub-Commission on Prevention of Discrimination and Protection of Minorities, *The Main Types and Causes of Discrimination* (1949.XIV.3), p. 2.

¹¹ United Nations, *Study of Discrimination in the Matter of Political Rights*, by Hernán Santa Cruz (New York, 1962; 63.XIV.2), pp. 96-99.

⁸ S. I. Benn and R. S. Peters, *The Principles of Political Thought* (New York: Free Press, 1965), pp. 131-132.

country whose members are in fact prevented by political, economic, religious, social, historical, or cultural conditions from enjoying equality with the rest of the population in the matter of political rights; (ii) the balanced representation of the different elements of the population of a country; provided that such measures are continued only so long as there is need for them, and only to the extent that they are necessary.

Illustrations of the negative and positive approaches to equality appear in various international conventions, for example, the Discrimination (Employment and Occupation) Convention (1958), sponsored by the ILO, the Convention Against Discrimination in Education (1960), sponsored by UNESCO, and the Convention on the Elimination of All Forms of Racial Discrimination (1965), sponsored by the United Nations. Discrimination is defined in approximately the same way in all three. The wording in the Convention Against Discrimination in Education is as follows:

The term 'discrimination' includes any distinction, exclusion, limitation or preference which, being based on race, colour, sex, language, religion, political or other opinion, national or social origin, economic condition or birth, has the purpose or effect of nullifying or impairing equality of treatment.¹²

Then comes the positive undertaking. Parties bind themselves affirmatively "to promote equality of opportunity and of treatment in the matter of education" in various specified ways. The Discrimination (Employment and Occupation) Convention likewise accompanies the ban on discrimination with the assertion of an affirmative obligation "to promote . . . equality of opportunity and treatment" and with a dispensation permitting "special measures of protection or assistance" for certain kinds of groups. The Convention on the Elimination of All Forms of Racial Discrimination includes comparable provisions, negatively banning discrimination and positively requiring promotional efforts.

States Parties shall, when the circumstances so warrant, take, in the social economic, cultural and other fields, special and concrete measures to ensure the adequate development and protection of certain racial groups or individuals belonging to them, for the purpose of guaranteeing them the full and equal enjoyment of human rights and fundamental freedoms.

¹² The convention makes certain exceptions explicit. "Separate but equal" education by sex is permissible; and separate educational systems established in keeping with the wishes of parents for religious or linguistic reasons are also permissible, if enrollment is optional and if appropriate standards are maintained.

The Individual and the Group. In the western tradition the rights of man are thought of as individual rights, and formally the spelling out of human rights is the spelling out of rights for individuals. The very notion of human rights "without distinction as to race, sex, language or religion" suggests an atomistic world in which the individual is the important unit, in which groups scarcely count, and in which differentiation among individuals on the basis of their membership in groups is suspect if not forbidden. Paradoxically, however, the greater concern for human rights is associated with a greater consciousness of kind. It is as if the individual can gain pride and dignity only through identification with a group whose status and role command respect. Even the agitation against discrimination based on sex is associated with signs of an affirmation of sexual solidarity.

Whatever the reason, negative protests against discrimination are counterpoised with positive demands relating largely to groups. The fact is illustrated most generally by the demand, which welled up outside the imperial countries, that self-determination be named as a human right. To a considerable extent, this was a demand that a major manifestation of racial discrimination should end. Beyond this, demands concerning racial, ethnic, linguistic, and religious groups show up almost everywhere in the world. Group struggles occur over linguistic problems, for example, in Belgium, Canada, Ceylon, and India. Canada has gone bilingual. Belgium has opted for a division into strictly unilingual zones, the claim being that bilingualism would "debase the traditional character and individuality of the two communities."¹³ The Cabinet in Belgium must consist of an equal number of French-speaking and Dutch-speaking ministers.¹⁴ Ceylon has swung to "Sinhala only," to the discomfiture of its Tamil minority. India has created new states on a linguistic basis, but problems remain acute.

Religious groups as such no longer play the role in politics that they once did, but they have not gone completely offstage. India's positive efforts to promote equality include preferential treatment for the Scheduled Castes—as well as for the Scheduled Tribes and the "backward." The very creation of Pakistan reflected a religious division, as did, in a sense, the creation of Israel. Lebanon allocates seats in its parliament

¹³ Robert Senelle, "The Revision of the Constitution 1967-1971," *Memo from Belgium*, Nos. 144-145-146, January-February-March 1972 (Brussels: Ministry of Foreign Affairs, External Trade and Cooperation in Development), p. 15.

¹⁴ Senelle, p. 186. Article 86b of the constitution.

and many other governmental positions to religious groups as such. Cyprus, divided into Turkish and Greek communities identified by both language and religion, follows a similar plan.

Ethnic politics is nearly universal. For example, in Somalia "the formula for establishing a new government was . . . found in clan balance, the establishment of an equilibrium of the major ethnic groups . . ." ¹⁵ The Malay constitution assures the Malays a "special position" in public and economic life. New Zealand assures the Maoris of a quota of four seats in parliament.

Fiji got independence only when an elaborate arrangement was worked out to reassure the various ethnic groups. Voters register on one of three rolls, depending on whether they are Fijian, Indian, or "other," and the three rolls together constitute a national roll. The Fijis, Indians, and "others" are respectively assigned 22, 22, and 8 members of the House of Representatives. Of the 22, 12 are elected in each case by those on the appropriate ethnic roll and 10 by those on the national roll; of the 8, the corresponding division is 3 and 5. ¹⁶

And so it goes. These illustrations of differentiations between groups, and of the assignment of rights to groups, are among the more outstanding ones, but many others exist.

Discrimination as Unreasonable Differentiation.

Given the combination of a rule against discrimination and a principle that "special measures" on behalf of various kinds of groups are permissible if not required, the classification of specific policies as discriminatory or egalitarian becomes a matter of judgment. No litmus paper, no strict rule, provides a generally applicable, objective test.

This position contrasts so sharply with that taken before the International Court of Justice in the South West Africa Cases in 1964 that the contrast deserves to be pointed out. In those Cases the Applicants claimed that "a generally accepted international human rights norm of non-discrimination or non-separation" now exists. They defined "non-discrimination" or "non-separation" as follows:

stated negatively, the terms refer to the absence of governmental policies or actions which allot status, rights, duties, privileges or burdens on the basis of membership in a group, class or race rather than

¹⁵ A. A. Castagno, Jr., "Somali Republic," in James S. Coleman and Carl G. Rosberg, Jr., eds., *Political Parties and National Integration in Tropical Africa* (Berkeley: University of California Press, 1964), pp. 546-7.

¹⁶ *The Fiji Independence Order 1970* (London: HMSO), p. 35.

on the basis of individual merit, capacity or potential: stated affirmatively, the terms refer to governmental policies and actions the objective of which is to protect equality of opportunity and equal protection of the laws to individual persons as such. ¹⁷

They insisted "that the allotment, by governmental policy and action, of rights and burdens on the basis of membership in a 'group,' irrespective of individual quality or capacity, is impermissible discrimination, outlawed by legal norms well established in the international community." Earlier in the proceedings they had asked the Court to pronounce that South Africa's policies of *apartheid* in South West Africa were "arbitrary, unreasonable, unjust and detrimental to human dignity," ¹⁸ which would have called for an exercise of judgment about the effects of the policies, but they dropped this request in favor of the stricter and more nearly automatic rule. Pressed about that rule, they later wavered, admitting that "protective" measures for groups were sometimes justified, but never providing a principle for reconciling the ideas of nondiscrimination and nonseparation with the idea of protection. The Court dismissed the case without ruling on the merits. Later, as will be noted in a moment, it declared that South Africa's policies violate its international obligations, but it did not subscribe to the more sweeping and general claim that any official allotment of rights or burdens on the basis of membership in a group is impermissible.

In a dissenting opinion in the South West Africa Cases, Justice Kotaro Tanaka struck the right chord when he said,

Different treatment is permitted when it can be justified by the criterion of justice. One may replace justice by the concept of reasonableness generally referred to by the Anglo-American school of law. Justice or reasonableness as a criterion for the different treatment logically excludes arbitrariness. . . . We must recognize . . . the legality of different treatment so far as justice or reasonableness exists in it. ¹⁹

The same chord, along with some others, was repeatedly struck in the Belgian linguistic case considered by the European Commission of Human Rights. The case concerned alleged discrimination in Belgian policies directed at

¹⁷ International Court of Justice. Pleadings, Oral Arguments, Documents. "South West Africa Cases" (1966), 4, 492-493.

¹⁸ International Court of Justice. Pleadings, Oral Arguments, Documents. "South West Africa Cases" (1966), 1, 20-21.

¹⁹ International Court of Justice. *Reports of Judgments, Advisory Opinions and Orders*, 1966, pp. 306, 309.

the language problem. No one contended that "distinction" in the sense of differentiation was proscribed. The question was whether the differentiations were reasonable or whether they were unfair or unjust or arbitrary. "Discrimination," said the Commission, "must not be confused with legitimate distinctions designed to re-establish rather than to destroy equality."²⁰ The Commission even took the view that especially favorable treatment for one group did not necessarily constitute discrimination; there also had to be correlated disadvantage or hardship for another group; and on top of that the disadvantage or hardship "should not seem justified by administrative, financial or other needs or, more generally, by considerations based on the general interest or the common good."²¹ The Commission referred to Justice Tanaka's criterion of reasonableness, and said that its own criterion was "not essentially different."²² The Counsel for the Belgian government took the view that in deciding whether legislation was discriminatory, one should ask whether it "is in accord with the general interest, whether or not there are valid motives for it"; one should ask whether actions are "arbitrary" or whether they can be "justified."²³ The Court itself took approximately the same line. Its question was whether the distinctions that Belgium was making had an "objective and reasonable justification," and whether there was a "reasonable relationship of proportionality between the means employed and the aim sought."²⁴ The Court ruled that the provision of the European convention on human rights

does not prohibit distinctions in treatment which are founded on an objective assessment of essentially different factual circumstances and which, being based on the public interest, strike a fair balance between the protection of the interests of the community and respect for the rights and freedoms safeguarded by the Convention.²⁵

As Justice Tanaka's reference to "the Anglo-American school of law" suggests, the doctrine of reasonableness brings the international problem of defining discrimination within the framework of the domestic argument over the equal protection of the laws. That doctrine is

obviously troublesome, and the troubles are not much alleviated by the use of such supplementary words as *unjust*, *unfair*, and *arbitrary*. Experience in American courts speaks eloquently on the point. Nor are the troubles reduced when consensus about reasonableness is sought not simply within the United States but over the world with its endless variety. This is suggested by the fact that the Counsel for the government of South Africa, defending *apartheid* before the International Court of Justice, had no objection at all to the "common attitude . . . that differentiation of the nature of unfair discrimination is impermissible but that measures of differentiation which are genuinely designed to promote the interests of all the groups concerned, is permissible."²⁶ His claim was that *apartheid* is fair and in the interests of all the groups concerned. The same kind of attitude shows up in various Rhodesian constitutions and in the "Proposals for Settlement" which Britain and Rhodesia worked out in 1971. After calling for a prohibition of discrimination, the "Proposals" went on to say that a law would not be considered discriminatory if it "is reasonably justifiable either in the interests of Rhodesia as a whole or in order to secure the protection, in an equitable manner as between the [groups] affected, of their respective interests."²⁷ But if disagreement about what is reasonable, just, and fair is inevitable, the words at least fix a common basis and framework for debate. Moreover, the record shows that some degree of international agreement is possible. The European Court of Human Rights found it possible to render judgment in the Belgian linguistic case. Further, as noted above, the International Court of Justice, in a 1971 advisory opinion, after noting that South Africa, like all members of the United Nations, is obliged to promote human rights for all without distinction as to race, went on to declare:

To establish instead, and to enforce, distinctions, exclusions, restrictions and limitations exclusively based on grounds of race, colour, descent or national or ethnic origin which constitute a denial of fundamental human rights is a flagrant violation of the purposes and principles of the Charter.²⁸

²⁰ Eur. Court H. R. Series B, "Linguistic" Case I, 212.

²¹ Eur. Court H. R., Series B, "Linguistic" Case, II, 47.

²² Eur. Court H. R., Series B, "Linguistic" Case, II, 82-83.

²³ Eur. Court H. R., Series B, "Linguistic" Case, II, 151.

²⁴ Eur. Court H. R., Series A, "Linguistic" Case, Judgment of 23 July 1968, p. 34.

²⁵ Eur. Court H. R., Series A, "Linguistic" Case, Judgment of 23 July 1968, p. 44.

²⁶ International Court of Justice. *Pleadings, Oral Arguments, Documents*. "South West Africa Cases" (1966), VIII, 699.

²⁷ Great Britain. Parliamentary Papers. *Rhodesia. Proposals for a Settlement*. November 1971. Cmnd. 4835, p. 36.

²⁸ Text of opinion in *American Journal of International Law*, 66 (January, 1972), 180-181. Cf. Egon Schwelb, "The International Court of Justice and the Human Rights Clauses of the Charter," *American Journal of International Law*, 66 (April, 1972), 348-349.

Minority Electoral Politics in a North Indian State: Aggregate Data Analysis and the Muslim Community in Bihar, 1952-1972*

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The practice of electoral democracy places a hard dilemma before minority groups. If the members of the minority want to participate as fully equal members of the polity, they must integrate themselves into the larger group and play the games of politics according to the majority's rules, but they do so at the risk of seeing their minority identity and culture disappear. If on the other hand they insist on retaining their solidarity and group identity, they must act as a cohesive unit, a tactic which will underline their separateness from the larger society but at the risk of continued isolation and political impotence for the group. The French Huguenots in the United States might be said to be a prototype of the first kind of minority, while the Amish of Pennsylvania could serve as an extreme example of the second.

For most minority groups the two strategies amount to unrealistic and undesirable endpoints on a continuum, and in practice both are pursued at the same time in diluted form: the group adopts enough of the behavioral practices of the larger polity to participate successfully in the political process, while at the same time retaining its own identity and in fact continuing to behave as a distinct entity in the political process. Such at least have been the findings of most of the recent literature on minority group politics in the United States.¹

Some groups are geographically concentrated and have found it possible to exercise local

domination through the political process, although often at the cost of equality in the political system as a whole. An example would be the French in Canada, who for many decades appeared quite content to dominate Quebec, especially in matters of language, education and religion, even though they did so at the price of second class status in the rest of the Dominion.²

Minority groups that are geographically dispersed, however, cannot pursue such a strategy, for they have no significant areas of local control into which the collective ego may retreat. Blacks in America are an obvious case in point. Only in a few rural counties in the deep South and in crumbling, ghettoized cities of the North do they have a majority of the population; everywhere else they are only a small portion of the body politic.

In British India there were a number of areas of the first type, where Muslims were dominant, even though the subcontinent as a whole had an overwhelming majority of Hindus. The Partition in 1947, however, insured that the Muslims remaining in the Indian Union would be a minority of the second kind, for virtually all the Muslim-dominant areas went to Pakistan in the Radcliffe award that determined the boundaries of the new countries.³ During the constituent assembly deliberations that began

* The author would like to thank Hugh S. Plunkett of Davidson College, Joseph E. Schwartzberg of the University of Minnesota, and Thomas M. Sykes of Colgate University for their helpful criticism of this article.

¹ See, for example, Edgar Litt, *Ethnic Politics in America: Beyond Pluralism* (Glenview, Ill.: Scott, Foresman, 1970), and Nathan Glazer and Daniel Patrick Moynihan, *Beyond the Melting Pot: The Negroes, Puerto Ricans, Jews, Italians and Irish of New York City*, 2nd. ed. (Cambridge, Mass.: MIT Press, 1970); also the volumes of readings edited by Brett W. Hawkins and Robert A. Lorinskas, *The Ethnic Factor in American Politics* and by Harry A. Bailey, Jr., and Ellis Katz, *Ethnic Group Politics* (both Columbus, Ohio: Charles E. Merrill, 1970 and 1969 respectively). Historically and comparatively in multiethnic societies, there have been a number of other situations beside those noted here; see Philip Mason's encyclopedic study *Patterns of Dominance* and also his *Race Relations* (both London: Oxford University Press, 1970).

² See D. Kwavnick, "The Roots of French-Canadian Discontent," *Canadian Journal of Economics and Political Science*, 31 (November, 1965), 509-523. On the strategies of various French Canadian political leaders, see Michael Oliver, "The Strategy of French Canadian Participation in Canadian Politics," a paper presented at the International Political Science Association Conference, Ann Arbor, Michigan, September 1960.

³ Kashmir has a population that is more than two-thirds Muslim, but its internal politics have been of a rather special character that does not typify the rest of the country. Otherwise Assam (as constituted in 1961) has the highest proportion of Muslims (23.3 per cent) of any of the states. As to districts, only Murshidabad in West Bengal and Malappuram in Kerala have a majority of Muslims. In a sense there did remain for a short time after Partition in 1947 an area of Muslim dominance, for the "communally reserved" constituencies set up under the Government of India Act of 1935 provided legislative representatives for the Muslim community. India continued to be governed under this act until the new Constitution came into effect in 1950.

after independence, it was thought by some that minority religious groups should be allotted legislative seats on a quota basis, but in the end it was decided that all groups would compete for the same seats, and the first general elections of 1951-52 were conducted on that basis.⁴ Since then the Muslims have had to compete with the majority Hindu community on the latter's own electoral ground.

This paper employs an aggregate data analysis based on mapping to examine that competition in the north Indian state of Bihar, a unit of about 56 million people in 1971 (roughly the same in population as France) and approximately 67,000 square miles (about the size of the state of Washington). First some consideration will be given to the problems involved in doing aggregate data analysis with the Indian data and the advantages and deficiencies of various methods of ecological analysis thus far evolved to cope with these problems. The method to be used in this paper will then be presented, and an analysis of Muslim representation and participation in six Bihar Legislative Assembly elections from 1952 to 1972 will be undertaken.

Aggregate Data Analysis and Indian Politics

It seems at first glance strange that there has been thus far relatively little aggregate data analysis of Indian elections. The two major requisites for such work—voting returns and socioeconomic data—are available in abundance. There have been five national elections with profuse accompanying numerical data; and the Indian Census of 1961, which will run to more than a thousand volumes when completed, holds tantalizing prospects of almost endless and rewarding research to the student of politics interested in ecological analysis. The abundant statistical coverage at all levels from state down to village may be compared with data from the five elections. For India such an opportunity has a particular appeal, because survey data are expensive, methodologically and administratively difficult to obtain in such a huge country, and therefore exceedingly rare.⁵

⁴On the decision to eliminate reserved seats for Muslims, see Granville Austin, *The Indian Constitution: Cornerstone of a Nation* (Oxford: Clarendon Press, 1966), pp. 144-156. Reserved seats were retained for the Scheduled Castes (Harijans, or Untouchables) and Scheduled Tribes.

⁵There are exceptions, unfortunately thus far few. The major effort to date has been the work of the Centre for the Study of Developing Societies in New Delhi and that of the opinion pollster E. P. W. da Costa. Some of the findings of the Centre's 1967 election survey have been published in *Asian Survey's* symposium issue on "Elections and Party Politics in India," 10 (Novem-

In spite of the resurgence of interest in "ecological" political analysis generally in recent years,⁶ though, the promised Indian feast of aggregate data analysis has for the most part proven lamentably meager.

There are two major reasons for the failure to exploit this resource. One is the political integrity of the Election Commission, which has insisted on redrawing the constituency boundaries after every census, so as to make representation as equitable as possible. The practice most certainly buttresses the structure of parliamentary democracy in India, but it does make life frustrating for the political scientist, who consequently cannot compare elections across redrawn constituencies without a great deal of trouble.

The other problem is the general lack of geographical fit between census tracts and electoral constituencies.⁷ The census units are almost invariably the administrative units of revenue village, police *thana/tehsil*, district, and so on; the constituencies, on the other hand, are drawn to ensure equal populations, and wander over administrative boundary lines with a fine contempt for the scholar's convenience.

A number of schemes have been devised in recent years to overcome this problem of mismatching census and constituency boundaries, all in varying ways unsatisfactory. One method is to rectify the political units with the census units, and this is the approach that has been employed most often. With seats for Members of the Legislative Assemblies of the Indian states (MLA's) it is easy to aggregate to the district level, for in only a very few of the 300-plus districts in India do political boundaries cross district lines.⁸ The continuity of district

ber, 1970). Ramashray Roy of the Centre analyzes a survey done for the 1969 election in his *The Uncertain Verdict* (Bombay: Orient Longmans, 1972). As an example of da Costa's work, see "The Indian General Elections 1967, the Structure of Indian Voting Intentions: January 1967, A Gallup Report with Analysis" (New Delhi: Indian Institute of Public Opinion, 1967).

⁷See for instance Mattei Dogan and Stein Rokkan, eds., *Quantitative Ecological Analysis in the Social Sciences* (Cambridge: M.I.T. Press, 1969), and Richard L. Merritt and Stein Rokkan, eds., *Comparing Nations: The Use of Quantitative Data in Cross-National Research* (New Haven: Yale University Press, 1966).

⁸R. Chandidas examines the problem in considerable detail in "Ecological Correlates of Politics: Problems of Data Equivalence," a monograph presented to the Data Confrontation Seminar held at the Inter-University Consortium for Political Research, University of Michigan, 1969 (mimeo.).

⁶This is the procedure used by W. H. Morris-Jones and B. Das Gupta in "India's Political Areas: Interim Report on an Ecological Investigation," *Asian Survey*, 9 (June, 1969), 399-424; also by Donald S. Zagoria, "The Ecology of Peasant Communism in India," *Amer-*

boundaries over time lends added attractiveness to the use of the district level, making possible the comparison of results for all elections, not just those held in the periods between constituency redelineations. The existing socioeconomic data are much richer for the districts than for subordinate units; published data by district are readily available on agricultural output, acreage under different crops, per capita income, industrial undertakings and electricity use, but are available either not at all or only with great difficulty for smaller units.⁹

The procedure also has important shortcomings, however.¹⁰ Most serious is the likelihood of the ecological fallacy.¹¹ To aggregate a number of seats into one district (average population per district: more than 1½ million) and then treat the district as a unit involves the assumption that there is a common underlying behavior in the district as a whole; that is, that the various parts of the district are behaving electorally like the district. When the "parts" are constituencies for MLA's, the assumption is dubious, for each constituency is at least in some sense *sui generis*. In the larger districts, which may contain up to 30 MLA seats, the homogeneity assumption becomes virtually un-

tenable. By using such data we can find out a great deal about district voting behavior, and this knowledge is in itself significant, but any inferences about constituency voting behavior are very tenuous indeed. The tenuousness is compounded by the fact that most of the non-Congress parties do not run candidates in every MLA seat, so that a party that runs just four candidates, all of whom win, might well have the same percentage of the district level vote (and the same statistical value) as a party that runs 15 losing candidates.

It is, of course, possible to aggregate to levels above the district. Elkins has found six regions in South India that differ significantly among themselves, while state-level ecological analyses have recently been published by Heginbotham and Weiner.¹² The state is a natural unit in the political sense, and is logically better suited for comparison with equivalent units than is the district. Also it is considerably easier to obtain current socioeconomic data at the state level.¹³ But the states are scarcely electorally homogeneous within themselves; they vary hugely in size, and they are so few as to make statistical treatment extremely precarious.

It is also possible to aggregate data at levels below the district. One method is to use the constituency rather than the census/administrative area as the basic unit of analysis, as Benjamin and Blue have done.¹⁴ Using seats for Members of the national Parliament (MP's) as their units, they have employed a method of fitting 330 districts to 480 constituencies in such a way that they end up with ten deciles of constituencies. This technique permits the use of the full range of socioeconomic data available at district level, while avoiding the previously mentioned problem of mixing several constituencies into a single unit. The disadvantage is that the resulting data must then be treated as deciles rather than as individual constituencies. One is left in effect with a sample size of ten, and must use rank-order comparisons like the Wilcoxon-Mann-Whitney

ican Political Science Review, 65 (March, 1971), 144-160; and David J. Elkins, "Social Mobilization, Social Structure, and Politics: Evidence and Qualifications," a paper presented at the annual meeting of the American Political Science Association, Chicago, 1971. An earlier exercise along these lines is reported on in Government of India, *Census of India 1961*, Vol. I, India, Part I-A(i) and (ii), *Levels of Regional Development in India*, by A. Mitra (Delhi: Manager of Publications, 1965 and 1966). Much of the Chandidas project was devoted to fitting seats for members of the national Parliament to district areas, a much more difficult task, because these parliamentary boundaries do overlap district lines; see his "Ecological Correlates."

⁹ See, for instance, the lists of predictor variables employed in Morris-Jones and Das Gupta, "India's Political Areas," and Zagoria. The large number of variables available at district level also permits the use of statistical techniques like principal components analysis to ameliorate the problems of multicollinearity among the predictors. See Biplab Das Gupta, "Socio-Economic Classification of Districts: A Statistical Approach," *Economic and Political Weekly*, 6 (August 14, 1971), 1763-1774; and also W. H. Morris-Jones and B. K. Das Gupta, "More Dealings with Districts," a paper presented at the annual meeting of the Association of Asian Studies, New York City, March 1972.

¹⁰ Many of these shortcomings are pointed out by Morris-Jones and Das Gupta in "India's Political Areas."

¹¹ Since W. S. Robinson's classic essay in 1950, the literature on the "ecological fallacy" has become fairly extensive. Hayward R. Alker, Jr. surveys this literature in his "A Typology of Ecological Fallacies," in Dogan and Rokkan, pp. 69-86. For Robinson's original piece, see "Ecological Correlations and the Behavior of Individuals," *American Sociological Review*, 15 (August, 1950), 351-357.

¹² Elkins; Stanley J. Heginbotham, "The 1971 Revolution in Indian Voting Behavior," *Asian Survey*, 11 (December, 1971), 1133-1152; Myron Weiner, "Political Development in the Indian States," in *State Politics in India*, ed. Myron Weiner, (Princeton: Princeton University Press, 1968), pp. 3-58.

¹³ For instance, from the yearbook put out by the Publications Division of the Ministry of Information and Broadcasting, Government of India: *India: A Reference Annual*.

¹⁴ Roger W. Benjamin and Richard N. Blue, with Stephen Coleman, "Modernization and Political Change: A Comparative Aggregate Data Analysis of Indian Political Behavior," *Midwest Journal of Political Science*, 15 (May, 1971) 219-261.

test or graphic analyses,¹⁵ eschewing more precise statistical techniques such as multiple regression that are possible with larger populations and more exact measurements of each unit of observation.

If one is willing to forsake the rich lodes of district level data for the sparser socioeconomic deposits at subordinate levels, a good deal of worthwhile analysis is still possible. Brass has used as his unit of measurement the subdivision, an administrative (and census) area intermediate between the district and the MLA constituency.¹⁶ There is an initial problem of some severity here, as MLA seats often cross the subdivisional boundaries, but Brass overcomes it by aggregating subdivisions up to the point where constituency and administrative boundaries do coincide. The resulting population of units is a good deal larger than would be the case if districts were used, and thus the individual unit is much closer to the MLA constituency level. The disadvantages are that the units vary considerably in size (depending on how many subdivisions are clustered together to make up any given unit), and as before, a number of MLA constituencies are being consolidated as one unit.

Matching Census and Electoral Geography

For some types of socioeconomic data, it is possible to overcome virtually all of the problems mentioned in the preceding section and to obtain a good fit between census and electoral units. The method used here is based on isoplethic mapping, which connects areas of equal concentration of population. Such an approach permits the researcher to use both the census data and all the voting data by individual MLA constituency.

Before getting into the details of isoplethic analysis, it would be useful to have a picture of Muslim distribution in the state as a whole. Bihar state had a Muslim minority of 13.48 per cent at the time of the 1971 census, as against an all-India figure of 11.21 per cent. This statistic represented a substantial growth for the

Muslim community in Bihar, for it had increased its numbers by 31.3 per cent over the ten years since the 1961 census, while the state population as a whole grew by 21.3 per cent.¹⁷

The Muslims of Bihar are definitely not distributed evenly over the state. The seventeen districts of Bihar show considerable variation around the mean of 13.5 per cent, ranging from 4.0 per cent for largely tribal Singhbhum District in the southern Chotanagpur plateau to 39.9 per cent for Purnea District, which almost borders the new nation of Bangladesh in the east.¹⁸

As analysis proceeds to lower levels, the unevenness of the Muslim population becomes much more pronounced. For the 1961 census, communal data were published for the development block, or *anchal*, of which there are almost 600 in Bihar, averaging around 80,000 in population at the time of the census. Unlike many Indian socioeconomic variables, the Muslim distribution among these anchals is geographically continuous and so lends itself to the isoplethic mapping technique. Figure 1 offers an isoplethic map of Bihar's Muslim population based on data for the individual anchals.¹⁹ Points of equal Muslim "density" are connected by lines, similar to the elevation lines on a topographic map or the isobars on a weather map.

The most striking aspect of the map is the extremely heavy concentration of Muslims in the most easterly part of Purnea District, where a number of blocks are more than 50 per cent

¹⁷ Government of India, *Census of India 1971*, Series I—India, *Paper 2 of 1972: Religion*, by A. Chandra Sekhar (Delhi: Manager of Publications, 1972), pp. 16, 95. Bihar's Muslim population grew by 13.9 per cent during 1931–41, decreased by 7.3 per cent during 1941–51, then increased by 32.3 per cent during 1951–61. Apparently what had occurred during the 1950s was a significant reverse migration from East Pakistan back into Bihar after the large outward flow during and after the Partition of 1947. See Government of India, *Census of India 1961*, Vol. IV, Bihar, Part I-A(i), *General Report on the Census*, by S. D. Prasad (Patna: Bihar Secretariat Press, 1968), pp. 484–485. The Hindu-Muslim ratio of about 7-to-1 in 1961 and 1971 fits roughly into what Mason would call a "competitive" type; cf. *Patterns of Dominance*, pp. 60–65.

¹⁸ Purnea did in fact border East Pakistan after the Partition, but a slice of about 700 square miles was transferred to West Bengal in 1956, as a part of the States Reorganization Act. See Marcus F. Franda, *West Bengal and the Federalizing Process in India* (Princeton: Princeton University Press, 1968), pp. 8–61.

¹⁹ Data from which the map was derived were obtained from the district census handbooks that formed part of the 1961 census, e.g., Superintendent of Census Operations, Bihar, *Census 1961, Bihar District Census Handbook 1: Patna*, by S. D. Prasad (Patna: Secretariat Press, 1966), pp. 90–93. Data for the 1971 census by block had not become available by the time of writing this article.

¹⁵ *Ibid.* This method is also limited to Parliamentary seats, as the state Legislative Assembly seats are far too small (usually 1/6 of a Parliamentary seat) to be compared individually with districts.

¹⁶ Paul R. Brass, "Ethnic Cleavages and the Punjab Party System," a paper presented at the annual meeting of the Association of Asian Studies, New York City, March 1972. Yet another method is to select a sample of Legislative Assembly seats that do fit the census boundaries exactly; the difficulty here is that most of the seats do not fit and so must be thrown out of the analysis. See Harry W. Blair, "Elections and Aggregate Data Analysis at the Block Level in Bihar, 1967–1969," also a paper presented at the March 1972 meeting just noted.

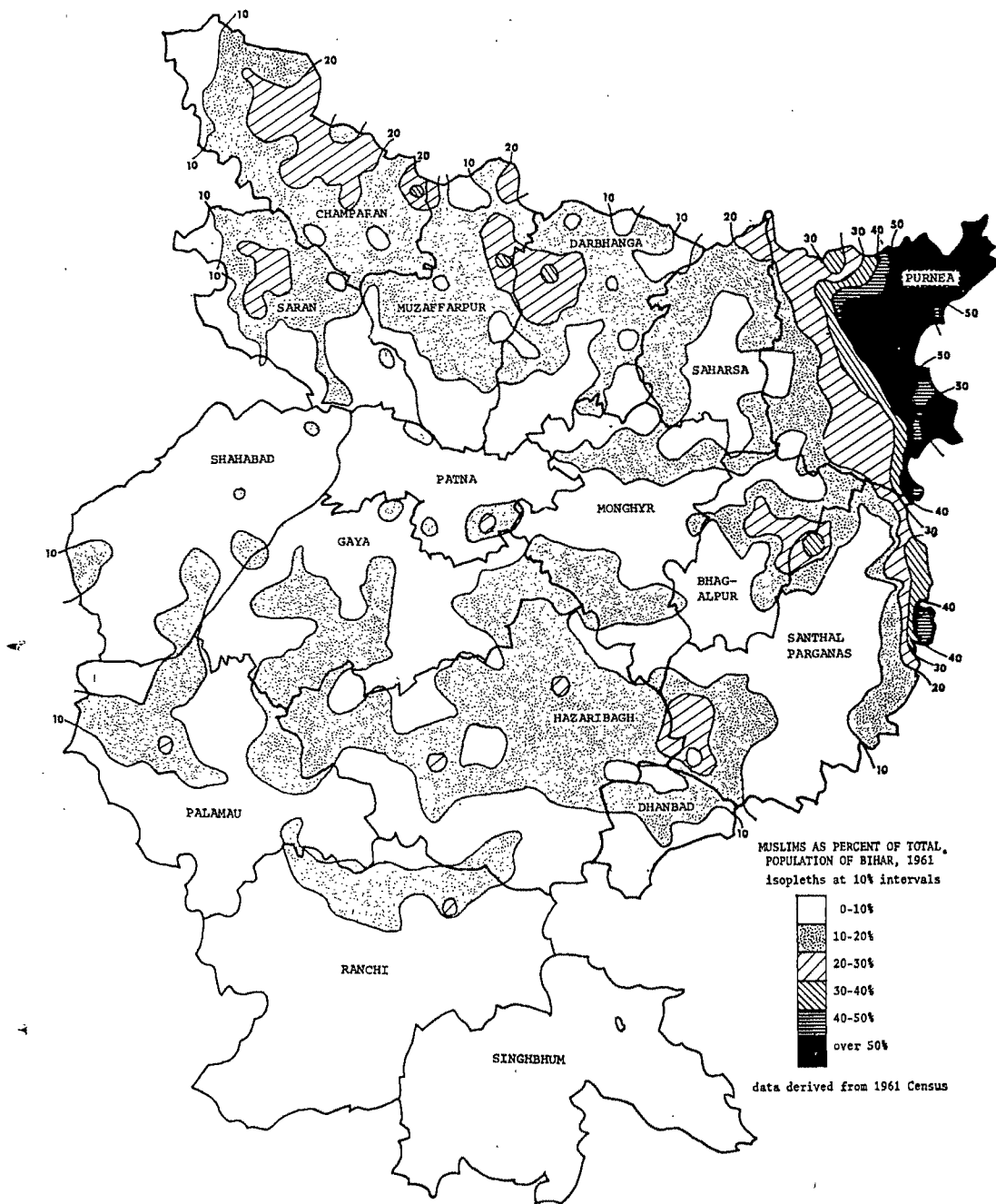


Figure 1. Muslim Population in the Districts of Bihar.

Muslim. Though they are less immediately apparent, there are other considerable concentrations as well. The northern districts of Champaran, Muzaffarpur, Darbhanga, and Saran all have sizable concentrations that are more than 20 per cent Muslim, and the first three have small pockets that are more than 30 per cent.

Perhaps the most interesting distribution is that found in Santhal Parganas, which has one area that is more than 40 per cent Muslim, and two others that exceed 30 per cent, while at the same time more than half the district is less than 10 per cent Muslim. Also of interest is the broad swath of territory running through the

north central part of the state from west to east (roughly along the path of the Ganges River) in which the Muslim concentration is very low, in fact less than one per cent in many blocks. The southern districts of Chotanagpur have few Muslims, except for a few small areas with concentrations of more than 20 per cent.

Muslims in the Electoral Process

When examined at the statewide level, the data on voting for Muslim candidates, presented in Table 1, show no general secular trend over the six elections that have been held since 1952.²⁰ As might be expected from the Western experience, the minority Muslim community has in every election at both state and national level polled substantially fewer votes for its candidates than its 12.5 per cent of the population. Likewise, the number of Muslims

²⁰ The first four general elections included both the state Legislative Assembly and the national Parliament. Because of the chronic instability that characterized state politics after the 1967 elections, the Assembly was dissolved in 1968 and new elections were held in 1969. Elections for the Parliament, due by the five-year constitutional requirement to be held at least by 1972, were called a year early by Mrs. Gandhi and thus were held in 1971. The Assembly held its sixth election in March 1972.

elected to office has been far fewer than would be the case if they were proportionately represented.²¹

The vote given to Legislative Assembly candidates rose during the first three general elections,²² and then peaked out at just under eight per cent in the 1962 poll, after which it

²¹ The United States House of Representatives elected in 1972, for example, had only 11 black members out of the total 435, while the population of the country is about 11 per cent Negro. For an analysis of Muslim representation in another state of India (Andhra Pradesh), see Rasheeduddin Khan, "Muslim Leadership and Electoral Politics in Hyderabad: A Pattern of Minority Articulation," *Economic and Political Weekly* 6 (April 10, 1971), 783-794 and (April 17, 1971), 833-840, esp. 837.

²² A study based on the first three elections concluded, partly on the basis of data similar to those given here for the 1952-1962 period (i.e., showing a rising proportion of the vote for Muslim candidates), that the Muslim community was being successfully integrated into the Indian political system. See Gopal Krishna, "Electoral Participation and Political Integration," *Economic and Political Weekly*, 2 (Annual Number, February 1967), 179-190. See also, in rebuttal, Imtiaz Ahmed, "Indian Muslims and Electoral Politics," *Economic and Political Weekly*, 2 (March 11, 1967), 521-523; and more recently Ahmed's "Secularization," *Seminar*, 144 (August 1971), 22-26; and in addition, Rajni Kothari *Politics in India* (Boston: Little, Brown, 1970), pp. 245-247.

Table 1. Muslim Candidates in Bihar: Vote Polled, Candidates and Winners, 1952-1972

Legislative Assembly (MLA's)	1952 ^a	1957	1962	1967	1969	1972
% of valid vote polled by Muslims	4.53	5.84	7.88	6.80	6.27	7.45
Muslims as % of candidates (n) ^a	4.70 (72)	6.73 (90)	7.19 (110)	8.00 (162)	6.18 (133)	8.17 (162)
Muslims as % of winners (n) ^b	7.01 (22) ^d	7.86 (25)	6.60 (21)	5.66 (18)	5.97 (19)	7.86 (25)
Parliament (MP's)	1952 ^a	1957	1962	1967	1971	
% of valid vote polled by Muslims	2.35	4.39	6.31	4.20	3.37	
Muslims as % of candidates (n) ^a	2.03 (4)	5.29 (10)	8.15 (19)	5.08 (16)	6.41 (27)	
Muslims as % of winners (n) ^b	5.66 (3)	5.66 (3)	3.77 (2)	3.77 (2)	5.66 (3)	

^a Number of Muslim candidates.

^b Number of Muslims elected.

^c In the 1952 Legislative Assembly election, there were a total of 330 seats in Bihar. The major part of the area composing 14 of these seats (4 in Purnea District and 10 in the former Manbhum District) was transferred to West Bengal in 1956, and data on these seats (2 of them won by Muslims) are not included here. Since 1957 there have been 318 seats.

^d Includes one Muslim Congress MLA returned in an uncontested election.

^e In the 1952 Parliamentary election, there were 55 seats allotted to Bihar. The major part of one double-member constituency (i.e., 2 seats) in Manbhum District was transferred to West Bengal in 1956, and data on it are excluded here. Since 1957 there have been 53 seats.

Source: Official returns through 1969, newspaper reports for 1971 and 1972 elections.

declined to a bit over six per cent in 1969, then rose again in 1972. The national Parliament vote for Muslims has lagged behind the Assembly vote by at least $1\frac{1}{2}$ –2 per cent in each election, with the gap increasing to more than 4 per cent if the 1971 and 1972 contests are compared. The disparity may be due to the difference in scale of constituency. Six seats for members of the Assembly are contained within each seat for a member of the Parliament, so that while the MLA candidates may be able to capitalize on small pockets of Muslim concentration, the MP seats cover such a wide area that these pockets are of negligible importance. Thus more Muslims are willing to contest at the Assembly level and, whether as cause or effect of this tendency, Muslim candidates collectively do better at the lower level.

The number of Muslim candidates particularly at the MLA but also at the MP level has gone up as the overall number of entrants has increased.²³ In 1952, there were only four Muslim MP candidates, about two per cent of the total, the proportion rose a bit over each succeeding election through 1962, then leveled off at around five or six per cent.

The number of candidates, though, may not be a very accurate measure of minority political strength, for anyone who wants to put up the required deposit of 250 rupees may declare himself an MLA candidate in an election, and for twice that sum he may file nomination papers as an MP contestant. This is not a great deal of money, and consequently there are large (and apparently increasing) numbers of publicity seekers and "nuisance candidates" who are sponsored by more powerful political operators to drain off a few votes from an opponent, but who have not the remotest chance to win.²⁴ In the 1967 Assembly election, 49.3 per cent of the entrants each polled less than 10 per cent of the vote, in 1969 the portion rose to 56.7 per cent, and in 1972 dropped very slightly to 54.0 per cent.²⁵ Muslim candidates reflected this general pattern, with 53.1 per cent of the MLA contestants each receiving under 10 per cent of the vote in 1967, 54.1 per cent in 1969, and 56.8 per cent in 1972.

²³ From 1532 to 1983 for MLA's during 1952–1972 (the maximum was 2153 in 1969), and from 197 to 421 for MP's during 1952–1971.

²⁴ On nuisance candidates in Bihar, see Harry W. Blair, "Caste, Politics and Democracy in Bihar State, India: The Elections of 1967" (Ph.D. thesis, Duke University, 1969), pp. 146–148.

²⁵ To cut down on the number of petty candidates, the government requires that the nomination deposit be forfeited if a candidate does not get at least one sixth (16⅔%) of the vote. This discouragement seems ineffective, however, for 1301 MLA entrants lost their deposits in 1967, 1366 in 1969, and 1254 in 1972.

Considerably more important than the number of candidates in the race, of course, is the number that win. Here the Muslims evidence roughly the same patterns as in the two categories already considered. In the Assembly, the number of MLA's elected began at 22, or about 7 per cent, increased to almost 8 per cent, dipped to a little less than 6 per cent in 1967 and 1969, then recovered to the 1957 level in the last election. The MP's returned from the Muslim community have held steady at 2 to 3 per cent throughout the period.

In terms of proportional equity, Muslims have had for most of the period about half as many representatives at both levels as they would be entitled to on the basis of strict proportionality. It might be pointed out, though, that they are by no means the most slighted group in Bihar in the way of underrepresentation. As has been shown elsewhere, lower caste Hindu communities such as the Kahars, Nais, Dhanuks, Tatwas, and Telis, all of whom have significant populations in Bihar, have been represented in the Vidhan Sabha much more poorly than the Muslims, while upper caste groups of the "twice-born" strata, like Brahmans, Bhumihars and Rajputs, have been and are grossly overrepresented.²⁶

The Geography of Muslim Voting

The geographical pattern among the MLA seats won by Muslims emerges in Figure 2. In the 1950s, when the Congress was at the zenith of its power, both in national and state politics, it regularly won with overwhelming majorities at the polls. In 1952 and 1957 the Congress swept more than two-thirds of Bihar's MLA contests, including in both cases all but one of the Constituencies carried by Muslim candidates. The 1952 and 1957 maps in Figure 2 indicate some victories in the heavily Muslim area of eastern Purnea District (cf. Figure 1 for district names and Muslim population), also in those regions of Champaran, Saran, Darbhanga and Bhagalpur districts where Muslims amounted to more than 20 per cent of the population.²⁷ There were almost as many seats

²⁶ Harry W. Blair, "Ethnicity and Democratic Politics in India: Caste as a Differential Mobilizer in Bihar," *Comparative Politics*, 5 (October, 1972), 107–127. This pattern of inequality has continued through the 1972 election as well; see N. K. Singh, "Bihar: Many Faces of Caste Politics," *Economic and Political Weekly*, 7 (April 8, 1972), 748–749.

²⁷ The 1961 population data are being projected backward in time, a practice which necessarily entails the possibility of inaccuracies, just as does the forward projection to 1972 later on in this paper. In effect, it is being assumed that the Muslim population of Bihar is expanding at roughly the same rate as the population as a whole. This was distinctly not the case in the 1940s

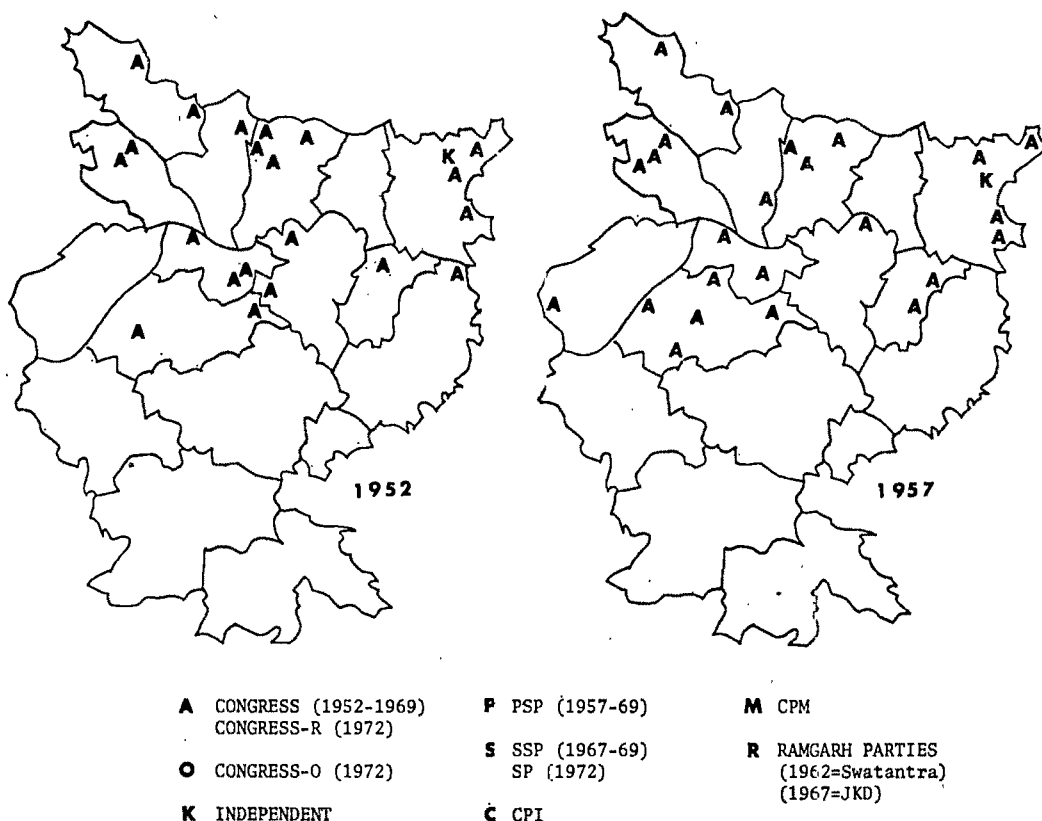


Figure 2. Muslims Elected to Bihar Legislative Assembly, 1952-1972.

Note: Boundaries for 1952 map have been adjusted to compensate for changes made after that election (cf. note c to Table 1). Between the 1962 and 1967 elections, a small change was made in the boundary dividing Monghyr and Saharsa Districts (cf. Figure 1); this change is reflected in the maps here.

won, however, in areas of very negligible Muslim concentration, such as the seats in Shahabad, Patna, Gaya, and Monghyr districts. Altogether 21 of the 47 seats won by Muslims in the two elections were located in areas where there were fewer than 20 per cent Muslims.

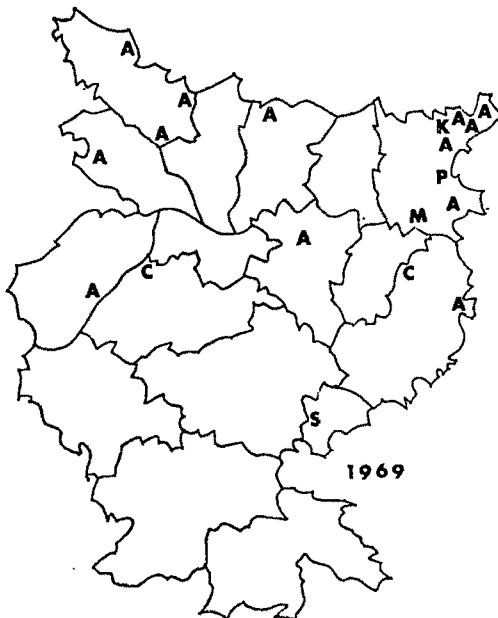
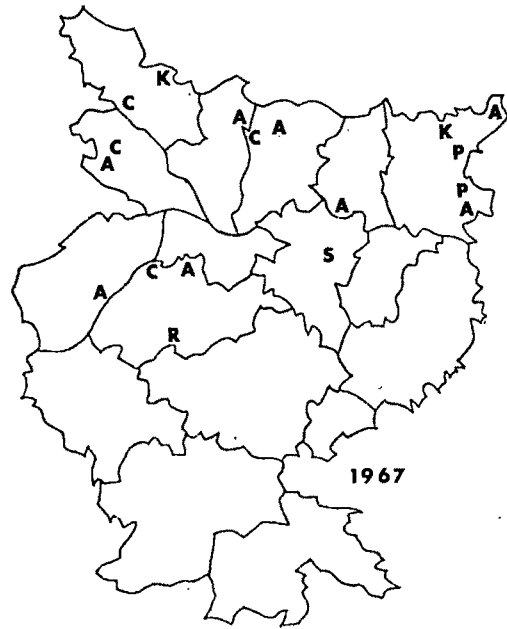
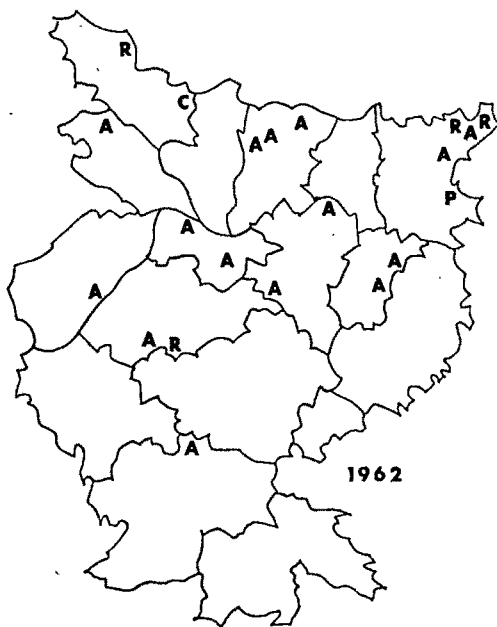
One could point to the halcyon days of communal toleration during the 1950s as the reason behind this success, and indeed that decade does seem in retrospect a calm interlude between the violence of the Partition and the communal riots that were to bedevil Bihar in 1964 and 1967. A more likely explanation lies

and early 1950s (cf. note 17, above) but is, one hopes, not an unreasonable assumption for the period since then. A caveat should also be added concerning constituency boundary changes over time. Because the geographical delimitation of seats was changed for the 1957 elections and then again for the 1967 elections, statistically precise comparisons cannot be made across all six elections for all seats, though a great many seats have remained almost intact geographically ever since 1952.

in the Congress hegemony of the time; it could and did elect almost anybody it chose to run anywhere.²⁸

In 1962, Congress strength diminished somewhat in the state as a whole, and we find corroborating evidence in the map, for this time only 15 of the 21 Muslim MLA's were returned by the Congress. The new Swatantra Party under the leadership of the Raja of Ramgarh won 50 seats in Bihar (giving it second rank in the state assembly), including four filled by Muslims. All told, nine seats were won

²⁸ It might be pointed out that while the Congress was electing so many Muslim MLA's in the 1950s, Muslim representation on the vitally important Bihar Pradesh Congress Executive Committee was significantly diminishing. In the 1930s and 1940s, Muslim membership on this committee averaged about 20 per cent, but in the 1950s it decreased to the level of 5-6 per cent. See Ramashray Roy, "Dynamics of One-Party Dominance in an Indian State," *Asian Survey*, 8 (July, 1968), 553-575; and his "Caste and Political Recruitment in Bihar," *Caste in Indian Politics*, ed. Rajni Kothari (New Delhi: Orient Longmans, 1970), pp. 228-258 at 242-244.



by Muslims in areas of low (under 20 per cent) Muslim dominance.

The fourth general elections of 1967 saw the eclipse of Congress rule in Bihar, and the Congress share of the Muslim MLA's dropped as well, to only 8 of 18. The Communist Party of

India (CPI) picked up four, the Praja Socialist Party (PSP) two, and its rival, the Samyukta Socialist Party (SSP) took one. The Ramgarh group had collapsed the Swatantra Party by moving out, detouring through a dalliance with the Congress in 1966 and into a new party

called the Jana Kranti Dal (JKD), which returned one Muslim MLA.

In the midterm poll of 1969, the Congress again failed to capture a majority of seats in the Legislative Assembly, but it did improve its Muslim holdings from 8 to 12 seats; the CPI picked up two; while its rival, the Communist Party of India (Marxist)—usually abbreviated CPM—took one; and the Praja and Samyukta Socialists held at one each. More significantly, a trend that had begun in the 1962 election had now become quite apparent. There were now only 6 Muslim MLA's from areas that had less than 20 per cent Muslim population, while 13 were returned from regions that were more than 20 per cent Muslim, and 8 of these held seats from areas where the minority comprised more than 40 per cent.

Between the 1969 and 1972 elections came the split in the Congress that resulted in a majority group, Prime Minister Indira Gandhi's Congress (Ruling), or Congress-R, and a smaller dissident group, the Congress (Opposition), or Congress-O. In Bihar, as in most of the states, the Congress-R claimed the major share of the former united Congress MLA's. In the 1972 election the Prime Minister's Congress improved its position greatly by winning a majority of 167 seats in the Assembly, an outcome reversing that of the 1967 and 1969 polls.

The return to Congress hegemony also saw a return in Muslim representation to the levels of the 1950s. Altogether 25 Muslims became MLA's, the same number as in 1957 (see Table 1). Some 14 Muslims won MLA seats on the Congress-R ticket, against only 4 for the Congress-O, and 7 for other parties and Independents. The 1972 geographical pattern shows some degree of resemblance to the 1957 distribution but more similarity to that of 1969: a concentration of MLA's in the heavily Muslim areas of Purnea and Champaran districts, with fewer scattered elsewhere. As in 1969, 8 of the 25 were elected from areas more than 40 per cent Muslim, against 5 in 1957, while only 8 of the 1972 contingent came from areas of less than 20 per cent Muslim population, as opposed to 12 in the 1957 contest.

From the maps in Figure 2, it would appear that across the six elections Muslims are becoming not more but less integrated within the political system. It is becoming easier for Muslims to get elected from areas where their coreligionists are strong in numbers, but at the same time it is becoming more difficult (though there are significant exceptions) for members of the minority to win office where their num-

bers are weak. This conclusion will be strengthened in the next section.

Statistics of Muslim Population and Voting Patterns

We now move to a consideration of the statistical relationships between the measures of Muslim population developed from Figure 1 and voting patterns at the constituency level. Upon a map similar to that used in Figure 1 (but larger in scale, so as to permit a greater degree of exactness) a second map, containing an outline of the MLA constituencies for the 1957 and 1962 elections was superimposed. An estimate of the Muslim population for each seat could then be gained by observing the pattern of isopleths within its area, and each seat was then given a code for one of six ordinal categories.²⁹ The process was repeated for the 1967 delimitation, which was in effect for the 1969 and 1972 elections as well,³⁰ thus making it possible to compare a measure of Muslim population directly with election results for individual Legislative Assembly constituencies.

Correlations between Muslim population and vote for Muslim candidates over the last five elections appear in Table 2.

Here we find confirmation of the data previously explored in Figure 2, though the shift is not evident until the end. There is a barely perceptible rise in the correlation for 1962 over that for 1957, meaning that vote for Muslim

²⁹ The ordinal categories used were 0-5 per cent, 5-10 per cent, deciles up to 40 per cent over 40 per cent. A test for accuracy was made by comparing the estimates arrived at in this fashion with direct census data on Muslim population for 69 MLA seats scattered around the state that actually do match up with the census units. The correlation (r) between the Muslim population estimated from the isoplethic map and the actual Muslim population for the 69 seats was .939. Using r^2 , we could say that the degree of "congruence" was 89 per cent.

³⁰ Because of difficulties in the delimitation for the 1952 election, this analysis was not performed for the first general elections. No attempt was made to perform the same technique for Parliamentary constituencies, which are each six times the size of a State Assembly seat, because it was thought that the problem of "ecological fallacy" was too great. A number of techniques have been developed by geographers for working out correspondences between maps using different areal units. See, for instance, Arthur H. Robinson, "Mapping the Correspondence of Isarithmic Maps," *Annals of the Association of American Geographers*, 52 (December, 1962), 414-425. (The terms "isarithmic" and "isoplethic" are often used interchangeably.) There has, however, been little interest in using such techniques on electoral data. For an exception, see Peirce F. Lewis, "Impact of Negro Migration on the Electoral Geography of Flint, Michigan, 1932-1962: A Cartographic Analysis," *Annals of the Association of American Geographers*, 55 (March, 1965), 1-25.

Table 2. Correlations Between Muslim Population and Vote for Muslim Candidates for Bihar Legislative Assembly, 1957-1972^a

	1957	1962	1967	1969	1972
Pearson <i>r</i>	.568	.574	.545	.684	.678
Number of seats ^b	59	84	100	95	96

^a All correlations are significant at the .001 level.

^b This figure represents the number of MLA seats which Muslim candidates contested. In 1957 there were Muslim entrants in ten additional seats, which are excluded here because they were double-member constituencies and could not be compared on the same basis as the single-member seats. Double-member seats were eliminated after 1957.

candidates was slightly more closely related to Muslim percentage of population in the second election.³¹

³¹ It cannot, of course, be inferred from the increase in correlation that a greater proportion of Muslims voted for Muslim candidates in the 1962 election than

In 1967 there was a very slight drop and then in 1969 a considerable jump to .684, yielding an r^2 which accounts for about 47 per cent of the variance, as against the 32 per cent provided by the 1957 data. The 1972 data show a persistence of the 1969 pattern. There does indeed seem to have been a shift in Muslim vote-getting power to the Muslim areas of the state.

This change in voting patterns for Muslim candidates has not been paralleled by any sweeping shifts in the relationship of Muslim population to the electoral fortunes of a particular party, as is seen in Table 3.

In 1952, 21 of the 22 Muslim MLA's were Congressmen, and in 1957 the number rose to 24 of 25. Muslims were generally viewed as a

in 1957, though that is probably the case. Given the statistics of Table 2, it is possible (albeit unlikely) that Muslims as individuals turned away from Muslim candidates in 1962. All we can say definitely is that Muslim candidates received more votes in general in areas more heavily populated with Muslims.

Table 3. Correlations Between Legislative Assembly Vote by Party and Muslim Population in Bihar, 1957-1972

	1957 ^a	1962	1967	1969	1972
Congress ^b	.233** (210) ^c	.139* (318)	-.007 (318)	.276** (318)	.093 (261)
Congress-O					-.072 (269)
CPI	-.401** (40)	-.076 (85)	-.064 (97)	-.101 (165)	-.055 (55)
Socialist/SSP ^d		-.116 (131)	-.135 (200)	-.037 (193)	.022 (252)
PSP	.076 (150)	.166* (199)	.332** (181)	.132 (94)	
Jana Sangh	-.250 (21)	.045 (75)	-.052 (268)	.153** (301)	-.005 (265)
Ramgarh Parties ^e	.001 (78)	.053 (259)	-.055 (164)	.012 (137)	
Shoshit Dal				-.103 (125)	

* Indicates correlation significant at .025 level.

** Indicates correlation significant at .001 level.

^a Includes only single-member seats for 1957. Subsequent elections eliminated the double-member constituencies.

^b Indian National Congress through 1969, Congress-R in 1972.

^c Number in parentheses indicates candidates fielded by that party in that year (number on which correlation is based).

^d Socialist Party in 1957 and again in 1972, SSP in 1967 and 1969.

^e Janta Party in 1957 and 1969, Swatantra in 1962, JKD in 1967.

special constituency of the Congress, under the protection and patronage of Maulana Azad and others at the national level. The correlation of .233 for the 1957 election is evidence of this special relationship, in that the Congress did do significantly better in the Muslim areas than the other parties did.³² The correlation dropped considerably in the 1962 election, however, and even more five years later.

There were numerous rumors in Bihar at the time of the 1967 election that Muslims were deserting their traditional support of the Congress.³³ At least some of these rumors had a foundation in the activities of the Majlis-e-Mushawarat, a politically oriented Muslim association formed in 1964. Although the Majlis backed Congress candidates for about half the seats in the Bihar Assembly, it openly supported non-Congress contestants for the other half.³⁴ The Majlis' effect on Muslim voters cannot be discerned from the present data, but it is the case that Congress support in Muslim areas dropped to the extent that the correlation between Muslim population and Congress electoral support completely vanished in 1967.

In 1969 the trend reversed itself. Though the Congress won ten fewer seats in the state as a whole and polled almost three percentage points less in the popular vote than in 1967, it did quite well in the Muslim areas. The *r*-value of .276 corresponds well with the impression given by the 1969 map in Figure 2, which showed Congress winning most of the Muslim seats in northeastern Bihar.

Whatever relationship was restored in the 1969 election disappeared in 1972, perhaps as part of the fallout from the Congress split. The Congress-R managed to retain a slight correlation with Muslim population, though not a statistically significant one, while the Congress-O wound up with a negative relation, even though

it ran 30 Muslim candidates, as against only 28 for the Congress-R.

Both the Communist Party and Samyukta Socialist (in 1972 simply the Socialist) Party claim in their propaganda to be especially interested in the welfare of the Muslim minority, but support for both parties has been negatively correlated with Muslim population in all five elections, except in the case of the Socialists in 1972, when the correlation was a scarcely noteworthy correlation of +.022. The Shoshit Dal (literally, "exploited movement") arose as a short-lived splinter group from the SSP in late 1967, announcing that it, unlike its parent party, would devote itself to the betterment of the "backward" communities of Bihar, including Muslims.³⁵ In electoral behavior, however, it followed the ancestral pattern in finding its votes negatively correlated with Muslim population in its only time at the polls in 1969.

The Communists, on the other hand, were thought by some to be getting support from unexpected Muslim quarters in 1969, for there were a number of rumors to the effect that the Muslims of Bihar had received directions from the "ruling group" in Pakistan to vote for the CPI.³⁶ The correlation of -.101 between Muslim population and CPI vote in Table 3 gives little support to this belief.

Alone among the leftist parties, the PSP's electoral support has a positive correlation with Muslim population, reaching a high-water mark of .332 in 1967, when two PSP Muslims were returned as MLA's. Between 1969 and 1972 the PSP and SSP merged to form the new Socialist Party (SP), and it may very well have been the legacy of PSP popularity in Muslim areas that accounted for the very slight positive correlation between Socialist vote and Muslim population in 1972.

On the right, the various parties led into the electoral fray by the late Raja of Ramgarh have never displayed any degree of voting strength related to Muslim density, though the Raja's 1962 political vehicle, the Swatantra Party, did elect four Muslim MLA's. The primary reason for the negligible correlations probably lay in the source of the Raja's electoral strength, his home base and vast landholdings in Hazaribagh District, where he invariably won most of the seats (in many cases with his own relatives)

³² Because of the constituency delimitation problem mentioned above, correlations were not calculated for the 1952 elections. Again, it must be emphasized that Table 3 does not measure the relationship between voting by Muslims and voting for particular parties. It only measures the relationship between Muslim population and voting for the individual parties.

³³ As reported to the author, who was in Bihar at the time of the 1967 elections.

³⁴ Zaheer Masood Quraishi, "Electoral Strategy of a Minority Pressure Group: The Muslim Majlis-e-Mushawarat," *Asian Survey*, 8 (December, 1968), 976-987. Whatever success the Majlis had was rather short-lived, for it had more or less collapsed by the time of the 1969 poll. See the same author's "Emergence and Eclipse of Muslim Majlis-e-Mushawarat," *Economic and Political Weekly*, 6 (June 19, 1971), 1229-1234; also Theodore P. Wright, Jr., "Muslims as Candidates and Voters in 1967 General Elections," in *Fourth General Elections in India*, ed. S. P. Varma and Iqbal Narain, vol. 2 (Bombay: Orient Longmans, 1970), pp. 207-224.

³⁵ On the Shoshit Dal, see Blair, "Caste, Politics and Democracy," pp. 334-338.

³⁶ Vishwanath Prasad Varma, "Analysis of Results," in *A Study of Mid-Term Elections in Bihar (1969)*, ed. V. P. Varma (Patna: Institute of Public Administration, Patna University, 1970), pp. 76-77; also his "District-wise Classification of Results," in the same volume, pp. 94-95 and 101.

and where the Muslim population is relatively small.

It has almost become part of the conventional political wisdom in India that the Hindu-oriented Jana Sangh does very well in Muslim areas, because the more Muslims that are visible, the more easily the Jana Sangh can employ its ability to arouse communal feeling among the Hindus and translate this animosity into votes for itself. This notion finds superficial support in the observation that in both 1967 and 1969, urban MLA seats in eastern Patna, Gaya, Bhagalpur and Ranchi towns, all of which have some Muslim concentration, were carried by the Jana Sangh.³⁷ But when we look at the state as a whole, the relationship tends to disappear.³⁸ Through 1967 the correlation is weak, and even negative in two out of three cases. In 1969 there is a significant though low correlation, but in 1972 the pattern returns to that displayed in previous elections.

Conclusion

In terms of representation in the Legislative Assembly, Muslim fortunes have oscillated, up to a high of 25 MLA's in 1957, down to 18 or 19 in the late 1960s and then back up to 25 in the 1972 poll. The portion of the vote obtained by Muslim candidates has undergone a similar oscillation. At the same time, however, there has been a more definite one-way trend in the geographical distribution of vote for Muslim candidates, who have been getting their votes more and more from areas of substantial Muslim population. In statistical terms, the Muslim population "explained" a good deal more of the vote for Muslim candidates at the end of the period than at the beginning. Muslim numbers

have been making themselves felt politically in the last two elections, but what appears to be increasing Muslim cohesiveness in these areas is coming at the cost of Muslim candidates' ability to exert appeal in regions of little Muslim numerical strength. In terms of the dilemma posed at the beginning of this paper, solidarity appears to be winning over integration.³⁹

Still, there is considerable evidence that integrative factors have not completely disappeared. For one thing, not all the Muslim MLA's get elected from Muslim strongholds. Some Muslim politicians do continue to poll high percentages of the vote in areas where their co-religionists are few. Indeed, the fact that in this paper we can refer to areas of 20 and 30 per cent Muslim population as "strongholds" indicates that any real communal polarization is a long way off. Equally important, the low correlations between Muslim population and vote for any particular party, especially in the most recent election, mean that no party is wedded in its electoral fortunes to the statistics of communal demography. Further, there is no evidence from past elections that any party would stand to gain greatly if at all by an appeal to communal feelings.

In all political systems the dilemma between integration and sociocultural identity that faces minority groups is at bottom an insoluble one, for generally both goals are deeply desired, but the complete realization of either one must necessarily mean the repudiation of the other. The most satisfactory arrangement would appear to be that employed by many ethnic groups in America: pursuit of a portion of both goals simultaneously, so that while neither is totally achieved, both are in part accomplished. If that is so, Bihar's Muslims are probably making the best of their opportunities; more Muslims are getting elected in Muslim areas, but Bihar politics has not become polarized along communal lines, for some Muslims can still be elected from non-Muslim areas.

³⁷ There are only 13 seats out of the total 318 in Bihar that could be called in any way genuinely urban. The Jana Sangh won 2 of the 13 in 1972.

³⁸ Craig Baxter disputes the existence of such a relationship in *The Jana Sangh: A Biography of an Indian Political Party* (Philadelphia: University of Pennsylvania Press, 1969), pp. 234-235. Angela Burger also takes issue with the thesis in her Uttar Pradesh study, *Opposition in a Dominant Party System* (Berkeley: University of California Press, 1969), pp. 199-200. But see also Morris-Jones and Das Gupta, "India's Political Areas," 416; B. D. Graham, "A Report on Some Trends in Indian Elections: The Case of Uttar Pradesh," *Journal of Commonwealth Political Studies*, 5 (November, 1967) 179-199 at 191; and Imtiaz Ahmad, "Religion in Politics: A Comment," *Economic and Political Weekly*, 7 (January 8, 1972), 81-86 at 84-85.

³⁹ For other electoral systems, evidence for the salience of religious cleavage appears mixed; in some it continues to be of major importance, while in others it is barely noticeable in electoral behavior. See Arend Lijphart's study of 10 Western systems, "Class Voting and Religious Voting in the European Democracies," Survey Research Centre, Occasional Paper No. 8 (Glasgow: University of Strathclyde, 1971).

Fear, Apathy, and Discrimination: A Test of Three Explanations of Political Participation*

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When *The New York Times* pointed out on June 5, 1969, that "voting apathy among blacks" was to blame for the defeat of all but one black candidate for Mayor in Mississippi's 1969 municipal elections, it was reflecting quite accurately the accepted political science wisdom about voter participation. In a rare show of unanimity, political scientists have generally taken as given that nonparticipation in politics is a result of disinterest or apathy, and have focused their research on specifying the exact social, economic, and attitudinal correlates of such apathy. And even in this search for correlates of apathy, there is substantial agreement, for virtually every study of political participation has concluded that participation is lowest (because apathy is highest) among the poor and the less well educated, whether in the United States or elsewhere. The only debate has been about why this should be so, with imaginative social scientists advancing explanations as diverse as "class differences in child rearing practices" and "differential political role expectations."¹

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¹ The literature on political participation is voluminous. Seymour Martin Lipset summarizes much of the empirical work linking nonparticipation to low income and education in "Who Votes and Who Doesn't?" in Lipset, *Political Man* (Garden City, New York: Anchor Books, 1963), pp. 183-229; and Robert Lane summarizes the theories purporting to explain this link in *Political Life* (Glencoe, Ill.: The Free Press, 1959), pp. 220-234. See also: Bernard Berelson, Paul F. Lazarsfeld and William N. McPhee, *Voting* (Chicago: University of Chicago Press, 1954); Philip K. Hastings, "The Voter and the Non-Voter," *American Journal of Sociology*, 62 (November, 1956), 303-04; Lester Milbrath, *Political Participation* (Chicago: Rand-McNally Co., 1965).

For all its impressive empirical support, however, the "income-education-apaty thesis" may have a potentially fatal flaw: It assumes that people fail to participate in politics chiefly because they do not think it is worth the time or because they fail to understand what is at stake. It begins, in other words, with a conceptual framework attuned to political life in a modern democracy, where participation is truly open.

But how appropriate is this framework for understanding political participation patterns where participation involves more than mere time and understanding—that is, where it involves serious risks as well? In such situations, the real explanation of variations in political participation rates may not be apathy at all, but *fear*—fear of violence, or more subtle forms of fear related to social and economic coercion.

Significantly, however, most students of political participation have neglected the impact of fear on participation, and have downplayed the coercive aspects of political life. Robert Lane, for example, identifies no less than eleven factors that purport to account for lower-class nonparticipation in politics, but *fear* is not among them.² Lipset and Milbrath develop their own catalogues of causes, but with the same result: virtually no mention of fear.³ Apathy, it seems, comes in 57 varieties, but fear in none.

The one recent empirical analysis of political participation that even treats the subject of fear is the massive Matthews and Prothro analysis of Negro politics in the American South. But even Matthews and Prothro manage to dispense with fear in a hasty eleven pages out of 488, concluding, on the basis of measures of fear that we seriously question for reasons elabo-

² The closest Lane comes to acknowledging a role for fear is in his discussion of the low sense of political efficacy and limited self-confidence among the poor. Lane, *Political Life*, pp. 222-34.

³ Lipset, "Who Votes?" in *Political Man*; and Milbrath, *Political Participation*, particularly pp. 48-141.

rated below, that "intimidation and fear" are "not major causes" of Negro failure to participate in politics as much as whites do.⁴ This finding is all the more surprising because it flies in the face of virtually every recent participant-observer account of southern black politics. According to one such study, voter registration workers in the South during the 1960s quickly discovered that "Apathy was too easy a catchword"—it disguised more than it revealed, and thus misrepresented the underlying social reality. "To understand 'apathy' in its fullest meaning," Reese Cleghorn and Pat Watters therefore concluded:

was to know that once all the South was totally resistant to the Negro vote, and that still none of the South was free of some form of resistance, and that just as the worst resistance of 1962-4 cast its shadow on areas where things were easier, so the memory and shadow of all the past resistance would be some part of "apathy" in 1962-4 and, probably, for some time to come . . . violence and intimidation not just last year but twenty years ago take their toll.⁵

Clearly, it is time to reconsider empirically the role of fear in political participation.

In addition to "apathy" and "fear," however, there is a third potential explanation of variations in political participation. An impressive body of social science literature links protest and agitation to the degree of discrimination a disadvantaged group experiences. The greater the discrimination, this literature suggests, the greater the likelihood of protest, particularly when a period of relative improvement creates expectations that are then frustrated by a period of stagnation or decline.⁶

⁴ Donald R. Matthews and James W. Prothro, *Negroes and the New Southern Politics* (New York: Harcourt, Brace & World, Inc., 1966), p. 308 (emphasis added).

⁵ Pat Watters and Reese Cleghorn, *Climbing Jacob's Ladder: The Arrival of Negroes in Southern Politics* (New York: Harcourt, Brace and World, 1967), pp. 124-25 (emphasis added).

For other evidence from participant observers about the role of fear in southern black politics, see: Sally Belfrage, *Freedom Summer* (New York: Viking Press, 1965); Anne Moody, *Coming of Age in Mississippi* (New York: The Dial Press, 1968); Polly Greenberg, *The Devil Has Slippery Shoes: A Biased Biography of the Child Development Group of Mississippi* (London: The Macmillan Company, 1969); William McCord, *Mississippi: The Long Hot Summer* (New York: W. W. Norton and Co., 1965); James Silver, *Mississippi: The Closed Society* (New York: Harcourt, Brace and World, 1963).

⁶ Tanter, for instance, found that domestic violence is greatest in countries where income inequality is greatest. Raymond Tanter, "Toward a Theory of Conflict Behavior in Latin America," paper prepared for delivery at the Annual Meeting of the International Political Science Association, 1967.

Instead of a single "explanation" of political participation, in other words, there may be three: one based on *fear*, the second on *discrimination*, and the third on *apathy*. The purpose of this article is to evaluate these three potential explanations of political participation patterns by analyzing how well they account for the variations in political participation among blacks in the 29 black-majority counties of Mississippi during the first half-decade following the 1965 Voting Rights Act. This analysis will require the formulation of a suitable measure of political participation or mobilization, the translation of each of the three "explanations" or models into "operational" terms, and the application of an appropriate "test" to measure the explanatory power of each. Before turning to these matters, however, we must look briefly at the overall record of black political participation in the Deep South since 1965, for this record poses the problem the remainder of this article seeks to unravel.

The Problem: Variations in Black Voter Turnout

When Congress passed the Voting Rights Act in 1965, hopes ran high that the law would soon usher in a new day of democracy in the Deep South. With one surgical stroke, Congress cut away the elaborate legal growth that Southerners had nurtured for nearly a century to keep blacks from voting, and guaranteed that for five years the federal government would continue to root out any reappearances of the cancer.⁷ "They came in darkness and they

This discrimination factor corresponds very closely to the concept of "relative deprivation" formulated by students of domestic violence. In its more refined forms, however, the "relative deprivation" notion focuses not simply on the actual distribution of resources between the lower classes and their "betters," but also on the perceptions of the lower classes about what they have a right to expect and on the relationship between these expectations and the possibility of fulfilling them. Most empirical tests of this notion to date, however, have had to measure these aspirations indirectly. See: James Davies, "Toward a Theory of Revolution," *American Sociological Review*, 27 (February, 1962), 5-19; Harry Eckstein, "On the Etiology of Internal Wars," *History and Theory*, 4 (1965), 133-63; Ted Gurr with Charles Ruttanburg, *The Conditions of Civil Strife: First Tests of a Causal Model* (Princeton, N.J.: Center of International Studies, April 1967); Neil J. Smelser, *Theory of Collective Behavior* (New York: The Free Press, 1963). For an opposite view, albeit on a slightly different point, see Crane Britton, *The Anatomy of Revolution*, rev. ed. (New York: Vintage Books, 1952), especially pp. 264-65.

⁷ The Voting Rights Act of 1965 was the culmination of extended efforts by civil rights forces to secure federal assistance in their efforts to eliminate the barriers to black political participation in the South.

came in chains," President Johnson declared when he signed the law. "Today we strike away the last major shackle of those fierce and ancient bonds."⁸

From afar, the record of the first half-decade under this law seems to confirm this euphoria. In the South as a whole, black registration rose from slightly more than 2 million in 1964 to 3¼ million by the end of the decade, and the number of black elected officials jumped from 72 to 473. In Mississippi, the results were even more dramatic: black voter registration soared from a mere 28,500 prior to the Voting Rights Act to 281,000 five years later, while the number of black elected officials rose from zero to 72.⁹

Closer scrutiny, however, reveals a far less hopeful picture of black political progress. While the number of blacks registered to vote in the South rose by one million between 1964 and 1970, for example, the number of white registrants rose by two million, so that blacks ended the decade with little gain in relative power.¹⁰ More important, black voter turnout has rarely exceeded 50 per cent of those eligible, except in the more urban counties. As a result, although 85 southern counties had black voting age population majorities as of 1960, only four have elected black sheriffs and only four have elected black majorities on their governing boards. In Mississippi alone, 29 counties have black majorities, yet none elected a black sheriff or a black majority on its governing board, and only one elected a black legislator in the first half decade under the Act.¹¹ Nor does

this situation seem to be changing. In the recent November 1971 elections in Mississippi, 309 blacks ran for public office at either the state or county level. Of these, 259 lost, including 28 out of 29 candidates for state legislature, 10 out of 10 candidates for Chancery Clerk (county executive), 14 out of 14 candidates for Sheriff, 10 out of 10 candidates for county School Superintendent, and 67 out of 74 candidates for county Boards of Supervisors, even though virtually all of these candidates were running in districts with black majorities. The only victories, in fact, were in the least consequential offices—Justice of the Peace, Constable, etc.—while blacks running for more significant posts went down to defeat almost without exception.¹² Apparently, something other than the legal barriers removed by the Voting Rights Act is keeping blacks from participating fully in the southern political process.

Even more puzzling than the weakness of this overall performance, however, have been the variations in black political participation among counties. Consider the case of Holmes and Humphreys counties in Mississippi, for example. On November 6, 1967, the newly enfranchised black voters of Holmes County elected the first black in this century to the Mississippi House of Representatives in the first local elections after the Voting Rights Act. On the same day, blacks in adjoining Humphreys County did not even bother to run a candidate for political office, even though they comprised 70 per cent of the county's population, and even though the white candidates running were no more popular among blacks than those in neighboring Holmes County. Nor can this contrast be explained by any obvious difference between the two counties; for the two appear, on the surface, to be virtual twins. A visitor would notice that Holmes to the east is a bit more hilly, but both are overwhelmingly rural; neither contains a town exceeding 3,000 persons; both grow primarily cotton and soybeans on their fields; and in both the visitor would encounter black and white faces in almost exactly the same proportions (about 2½:1). Residents in both counties, moreover, are exceedingly poor (the median income was \$1580 in Humphreys and \$1453 in Holmes in 1960) and ill-educated (the median adult education was 6.2

The new law outlawed the literacy tests and other discriminatory voter registration procedures, authorized the Attorney General to appoint voting registrars in counties where less than half of the voting age blacks were registered, and created a procedure to review any changes in election law in five southern states, including Mississippi. *Voting Rights Act of 1965*, 79 Stat. 437, 42 U.S.C., 1973. The Act was extended in 1970 for another five years.

⁸ Quoted in James Sundquist, *Politics and Policy: The Eisenhower, Kennedy and Johnson Years* (Washington, D.C.: The Brookings Institution, 1968), p. 274.

⁹ Voter Education Project, *Black Elected Officials in the Southern States*, mimeo (1969). p. ii; *V.E.P. News*, Vol. 3, No. 7, July 1969. p. 1; *V.E.P. News*, Vol. 4, Nos. 1 & 2, January-February 1970, p. 3.

¹⁰ *V.E.P. News*, Vol. 4, Nos. 1 & 2, January-February 1970, p. 3. In Mississippi, the black registration gain was more substantial: 242,500 black registrants were added compared to 147,000 whites.

¹¹ This information comes from the Voter Education Project, Atlanta, Georgia. The four counties with black sheriffs as of July 1972 were Greene, Lowndes, Macon, and Bullock—all in Alabama. The four counties with black majorities on their governing boards were Greene in Alabama, Hancock in Georgia, and Surrey and Charles City in Virginia. In addition, at this writing,

blacks have a majority on the Election Commission in Holmes County, Mississippi.

¹² Millsaps College, The Institute of Politics in Mississippi, "General Election 1971: Some Notes on Elections Involving Black Candidates." mimeographed. For an analysis of this election, see: Lester M. Salamon, "Mississippi Post-Mortem: The 1971 Elections," *New South*, 27 (Winter 1972), 43-47.

years in Humphreys and 6.9 years in Holmes in 1960). Finally, the white communities of both counties have been equally unfriendly to black political aspirations. In both, blacks were totally excluded from the political process before 1965. And in one test of white racial feeling, the 1964 presidential election, an identical 96 per cent of the white voters in both counties bolted from the Democratic party and from the first southern candidate for president in memory in order to support a candidate who voted against the 1964 Civil Rights Act.¹³

Were this contrast between Holmes and Humphreys counties unique, it would naturally attract little interest. But it is not unique. The same puzzling contrasts occur between other seemingly identical counties in Mississippi—like Marshall and DeSoto, or Issaquena and Sunflower—as well as between similar counties elsewhere in the Deep South. While in some areas blacks have been able to take advantage of the opportunity afforded by the Voting Rights Act to build significant bases of political power, in others they have not, and the reasons for the difference are not immediately apparent.

These baffling variations in black political participation may, of course, be purely accidental. On the other hand, however, they may hold important clues about the surviving barriers to full black political participation in the Deep South, and, by implication, about the barriers to the participation of disadvantaged groups in politically underdeveloped areas elsewhere in the world. The remainder of this article seeks to determine which of these alternatives is correct by systematically examining the variations in black political participation rates in Mississippi in the light of the three models outlined above.

Methodology and Data

Two methods are potentially available to analyze political participation patterns: survey research and aggregate analysis. Survey research has the important advantage of avoiding the so-called "ecological fallacy," the problem of inferring individual behavior from aggregate data,¹⁴ because it permits a researcher to ex-

plain individual-level decisions to participate or not to participate in terms of individual-level data. For the purposes of this research, however, the survey method had serious drawbacks that argued against its use. Not only would a questionnaire have been prohibitively costly, but also there was serious reason to doubt the trustworthiness of the data it would generate. One of the hypotheses we proposed to test, after all, was that nonparticipation is a result of fear. But using a questionnaire to measure the degree of fear experienced by blacks in rural Mississippi is like using an electron microscope to examine an electron: in both cases, the measuring instrument knocks the object to be measured out of sight. What is more, questionnaire data could not provide an answer to the question of why black political action was more successful in some *places* than others, at least not without an extremely large sample. Because of these serious inadequacies of the survey method for our purposes, and in view of the recent literature refuting earlier criticisms of the aggregate approach,¹⁵ we adopted the ag-

the Behavior of Individuals," *American Sociological Review*, 15 (June, 1950), 351-57. See also, Austin Ranney, "The Utility and Limitations of Aggregate Data in the Study of Electoral Behavior," in *Essays on the Behavioral Study of Politics*, ed. Austin Ranney (Urbana, Ill.: University of Illinois Press, 1962), pp. 91-102.

¹⁵ The literature on "ecological inference" has come virtually full circle since Robinson's statement of the problem in 1950. In a recent volume on ecological analysis, for example, Erik Allardt argues that preoccupation with the question of faulty inference has distracted attention from the more important question of the fruitfulness of various hypotheses. And in Allardt's view, the ecological approach may "facilitate fruitful causal interpretations better than the corresponding individual data, if such are available." Erik Allardt, "Aggregate Analysis: The Problem of its Informative Value," in *Quantitative Ecological Analysis in the Social Sciences*, ed. Mattei Dogan and Stein Rokkan (Cambridge, Massachusetts: The M.I.T. Press, 1969), p. 8.

Even more important, several scholars have identified ways to minimize the dangers of ecological inference without sacrificing the entire approach. These methodological clues have been incorporated in the research reported here. In a 1967 article in this *Review*, for example, W. Phillips Shively pointed out that the dangers of "ecological inference" can be minimized so long as "individuals have been grouped in such a way that their scores on the dependent variable are unrelated to the aggregation in which they fall, except indirectly through their scores on the independent variable." As will become clear below, the explanatory models used here meet this condition. See W. Phillips Shively, "'Ecological' Inference: The Use of Aggregate Data to Study Individuals," *American Political Science Review*, 63 (December 1969), 1186; and Hubert M. Blalock, Jr., *Causal Inferences in Non-experimental Research* (Chapel Hill: University of North Carolina Press, 1965), pp. 95-126.

The dangers of "ecological inference" have been

¹³ Demographic data is from U.S. Department of Commerce, Bureau of the Census, *1960 Census of Population: General Social and Economic Characteristics*, PC (1)-26C (Washington, D.C.: U.S. Government Printing Office, 1960).

Voting statistics are from F. Glenn Abney, *Mississippi Election Statistics, 1900-1967* (University, Mississippi: The University of Mississippi Bureau of Governmental Research, 1968), p. 28.

¹⁴ The classic statement of this problem can be found in W. S. Robinson, "Ecological Correlations and

gregate approach here, using "ecological regression" analysis. Instead of focusing on the individual potential black voter as the unit of analysis, therefore, we focused on the potential black electorate in an entire county. The research task, then, was to explain variations in black political participation among Mississippi's 29 black majority counties in terms of the characteristics of the potential black electorates in these counties.¹⁶

The first step, naturally, was to devise an index of black participation for each county (the dependent variable). Until recently, students of black politics in the South have used voter registration as the basic participation measure because registration figures were the only ones kept by race.¹⁷ In Mississippi, however, registration figures are not kept by race. What is more, the real power lies in the vote, so that registration figures can be seriously misleading. Registering without voting is an empty gesture at best: it gives the outward appearance of activity but leaves the distribution of power essentially unchanged.

In the present study, therefore, an effort was made to measure voting power directly. In par-

ticular, two measures of black voter participation were developed. First, statistics were collected on the votes received by all black candidates who ran for countywide elected office (either in the primaries or in the general elections) during the first half decade after the passage of the 1965 Voting Rights Act.¹⁸ The votes of the top two black vote-getters in each county were then averaged, in order to reduce the influence of special circumstances or unusual personalities, and the average was divided by the black voting age population in the county to give an overall mobilization index. Counties without two black candidates for countywide office were assigned indices on the basis of the assumption that had black candidates run in these counties, they would have done no better, but also no worse, than the weakest candidate who did run in another county.¹⁹

Strictly speaking, of course, this "mobilization" index may not be a precise measure of total black voter turnout since some blacks undoubtedly voted for the white candidates. In the context of Mississippi in the latter 1960s, however, those black voters who did so were effectively voting against the Civil Rights Movement, since all but a handful of the white candidates were outspoken opponents of the Movement. In fact, in the few cases where white candidates indicated their support for black suffrage and solicited black votes, black candidates did not run against them.²⁰ If not a precise measure of black turnout, therefore, the vote totals of the black candidates nevertheless constitute an excellent index of black political mobilization, of black electoral turnout in support of the Civil Rights Movement. This is particularly true, moreover, because the tense climate of Mississippi racial politics in the 1960s made it highly unlikely that very many white votes escaped to these black candidates, except by accident. The votes for these black candidates thus probably reflect quite well the number of blacks in each county who turned out to vote for a change in the status quo.

As a check on this mobilization index, how-

reduced even further here by adopting the suggestion made recently by Dogan and Rokkan to "develop statistical models for the exploration of *alternative linkages* between individual distributions and higher unit characteristics" as a way to avoid the "ecological fallacy." In this article, three "alternative linkages" are explored. See Mattei Dogan and Stein Rokkan, "Introduction," *Quantitative Ecological Analysis*, ed. Dogan and Rokkan, p. 8 (emphasis added).

¹⁶ The county was chosen as the unit of analysis because it is the smallest unit for which political, economic, and social statistics are readily available.

Only the counties with black majorities were used because they are, by and large, the only ones where black candidates have run for countywide office and where civil rights activity has occurred, both of which are important in measuring the dependent variable, as will become clear below. Only the black majority counties in one state are used to eliminate the effects of different state voting laws and political histories. Mississippi was chosen because it is the state with which we are most familiar and because, except for Georgia, it is the only one with a large enough number of black majority counties to permit a statistically significant test.

The "explanations" developed here were tested using correlation and multiple regression analysis, two standard statistical measures of relationship. The data analysis was performed on the Vanderbilt University Sigma-7 computer using standard Product Moment Correlation and Multiple Regression programs. We are indebted to Laird Heal of the Vanderbilt University Computer Center for assistance with these programs.

¹⁷ See, for example, Donald R. Matthews and James W. Prothro, "Social and Economic Factors and Negro Voter Registration in the South," *American Political Science Review*, 57 (March, 1963), 24-44, especially p. 26.

¹⁸ Countywide offices were at stake in two Mississippi elections during this period—in 1967 and 1968. The posts for which candidates ran county-wide in 1967 were: Sheriff, Chancery Clerk, State Senator, and School Superintendent. In 1968, they were: Election Commission (five members per county) and School Board (five members per county).

¹⁹ Four of the 29 counties analyzed here had only one black candidate for countywide office, and nine had none.

²⁰ This was the case, for example, with regard to the white candidate for Chancery Clerk in Wilkinson County, and the white candidate for State Senator in Holmes County.

Table 1. Index of Black Voter Mobilization by County, Twenty-Nine Mississippi Counties

County	Black Voter Mobilization Index	County	Black Voter Mobilization Index
Jefferson	53%	Leflore	29%
Claiborne	50	Sharkey	29
Wilkinson	49	Washington	25
Holmes	43	Bolivar	26
Issaquena	43	DeSoto	21
Jefferson Davis	43	Humphreys	20
Marshall	43	Jasper	19
Amite	42	Noxubee	19
Madison	40	Quitman	19
Copiah	39	Tate	19
Panola	34	Yazoo	18
Coahoma	30	Tallahatchie	17
Clay	30	Kemper	16
Carroll	30	Sunflower	15
		Tunica	15

ever, a second one was constructed on the basis of the votes in the 1968 presidential election, for which uniform returns are available for all counties. From the point of view of measuring black political participation, the 1968 presidential election provided an unusual opportunity. Where the 1964 presidential election had required southern whites to leave the Democratic party and vote for an arch-conservative non-southerner over a semi-Populist southern Democrat in order to register their opposition to civil rights, the 1968 election afforded them a greater array of more palatable alternatives. Both Nixon and Wallace appealed to the southern white vote. The Democratic candidate, on the other hand, was the very northerner whose civil rights stand at the 1948 Democratic election prompted the southerners to storm out in protest. If Lyndon Baines Johnson was able to attract no more than 5 or 6 per cent of the white votes in these black-belt counties in 1964, therefore, it seems likely that Hubert H. Humphrey received far fewer in 1968. As a result, the Humphrey vote totals very probably represent the clearest possible approximation of the black voter turnout in this election, or at least of the turnout of blacks who support the struggle for civil rights.

Since these two indices of black voter mobilization turned out to be very closely related (correlation = +.924), a single, combined index was computed by taking an average of the two.²¹ The results, noted in Table 1 above,

²¹ In mathematical terms, this participation index was computed as follows:

$$\text{Country Black Participation Rate} = \frac{\text{Humphrey Vote}}{\text{County Black VAP}} + \frac{\text{Vote for top two black candidates for countywide office/2}}{\text{County Black VAP}}$$

demonstrate the wide variation in black political mobilization among Mississippi counties.

The task now is to try to explain these variations, using the three models of participation mentioned earlier. To do so, it is necessary to translate each model into operational terms and test it against the data on black political participation. The following three sections treat the fear, discrimination and apathy models in turn, showing how each model was measured and what proportion of the variation in black participation rates it is able to account for. In each case, the Humphrey vote, the black candidate vote, and the combined black mobilization index were all tested separately. Since the results were virtually identical, however, only the results using the combined index are reported.

Fear and Black Political Participation

Fear is an exceptionally difficult phenomenon to measure, which helps to explain why students of political participation have generally ignored it. It may also help to explain why Matthews and Prothro, the only recent students of political participation who have analyzed the role of fear, reached a conclusion that differs so fundamentally from virtually all the first-hand, participant-observer reports available about black politics in the South in the late 1950s and early 1960s.²²

Matthews and Prothro measured the impact of fear in two ways: first, by comparing participation rates to the incidence of overt racial vio-

²² See note 5, *supra*.

lence in southern counties (as measured by the Tuskegee lynching statistics and the more recent Southern Regional Council's statistics on civil rights violence); and second, by relating participation rates to questionnaire responses from blacks who were asked if they "had ever heard of anything happening to Negroes around here who have voted or taken some part in politics or public affairs."²³ In both cases, they hypothesized that participation would be low where the incidence of violence, or the perception of violence, was high.

Both of these measures have serious drawbacks, however—so serious, in fact, that they throw into question Matthews's and Prothro's whole conclusion that "intimidation and fear" are "not major causes" of Negroes' failure to participate in politics as much as whites.²⁴ In the first place, by hypothesizing an inverse relationship between violence and participation, Matthews and Prothro misread seriously the role of violence in southern society, and the relationship between violence and fear. In particular, they assume that the absence of violence signifies the absence of fear. In the rural Deep South, however, the opposite seems closer to the truth. As the United States Civil Rights Commission pointed out as late as 1968, a "generalized atmosphere of intimidation" prevailed in many areas of the Deep South "even in the absence of specific threats or reprisals."²⁵ Historically, violence in the Deep South has been the instrument-in-reserve—perpetually available, but not always used. Violence, after all, is an expensive form of social control, and those in positions of influence have every incentive to restrict its use.²⁶ The Southern caste system was most successful, therefore, when it was least violent—i.e., when the fear of violence made the use of violence unnecessary. To equate the nonoccurrence of violence with the absence of fear, as Matthews and Prothro do, is thus to misinterpret badly the underlying social reality. Indeed, far from expecting the most violent places to be the *least* active politically, there is reason to expect them to be the most

active; for violence in the rural South has historically not been unleashed without a cause, and that cause has usually been black unrest and agitation. Matthews and Prothro, in other words, may be confusing cause and effect. Fear may be the cause of low participation; but the presence of overt violence may really signify the breakdown of fear.

A second problem with Matthews and Prothro's treatment of fear is that by focusing exclusively on overt violence, it ignores some of the more important, day-to-day sources of fear. Most ubiquitous of these is economic coercion. While violence, or the fear of violence, may have created the desired climate of apprehension, economic intimidation has usually provided the more reliable and economical instrument of control. In the rural and small town setting of the Deep South, most blacks could be controlled through the simple expedient of economic pressure without resort to physical violence. This was so because virtually all blacks were dependent to some extent on local whites for their income, and because the small-town setting facilitated intimate social control. If a black became "uppity" and began demanding his "rights," it was easy to organize a comprehensive program of coercion: to have him fired, deprived of a loan at the bank or store, barred from receiving welfare, and harassed by the authorities. Violence could therefore be reserved for the unusual cases, while economic pressure did its job day by day. When one national civil rights group developed a standardized form for checking off the excuses local Negroes gave for refusing to register to vote, therefore, it discovered that "economic pressure" easily led the list of items checked.²⁷ And this was so not only for the poverty-stricken plantation laborers and maids, but also for the better-off teachers and professionals who worked for white-dominated county School Boards. A study commissioned by the U.S. Civil Rights Commission in 1964 to determine why so few black teachers registered to vote in Mississippi thus discovered that "fear of loss of job was the most common response," outdistancing even "fear of violence."²⁸ On the basis

²³ Matthews and Prothro, *Negroes and the New Southern Politics*, pp. 166 and 303.

²⁴ Matthews and Prothro, p. 308.

²⁵ U.S. Commission on Civil Rights, *Political Participation: A Study of the Participation by Negroes in the Electoral and Political Processes in Ten Southern States Since the Passage of the Voting Rights Act of 1965* (Washington: U.S. Government Printing Office, 1968), p. 127.

²⁶ For a revealing discussion of the role of violence and intimidation of blacks in the southern caste system, see: Allison Davis, Burleigh B. Gardner, and Mary R. Gardner, *Deep South: A Social Anthropological Study of Caste and Class* (Chicago: University of Chicago Press, 1941), pp. 392–400.

²⁷ Watters and Cleghorn, *Climbing Jacob's Ladder*. Watters and Cleghorn report (p. 129) that Mississippi bankers used to tell black loan applicants in the 1960s that: "If you can afford to vote, you don't need a loan."

²⁸ U.S. Commission on Civil Rights, *Hearings*, Jackson, Mississippi, February 16–20, 1965 (Washington, D.C.: U.S. Government Printing Office, 1965), I, 209. Interestingly, this study was done by James Prothro, yet Prothro neglected to mention it in his book on Negro politics in the South published two years later.

of this and other evidence, the Civil Rights Commission concluded that "the economic dependence of Negroes in the South inhibits them from engaging freely in political activity and voting for candidates of their choice."²⁹ By focusing exclusively on physical violence and ignoring the relationship between economic intimidation and fear, Matthews and Prothro leave themselves open to serious questions about the credibility of their conclusion that fear is not a cause of black nonparticipation in politics.

Since violence is a poor index of fear, some other measure had to be found. The discussion above, and the general literature on southern society, suggested a solution to this problem by making it clear that fear in the rural South is not so much a result of specific incidents of repression as it is a result of a general condition of *vulnerability*. To the extent that the level of fear among blacks varies from place to place, therefore, it should vary in relation to the degree of black vulnerability to white coercion—whatever the form of that coercion. The search for a suitable measure of fear thus resolved itself into a search for suitable measures of black vulnerability. Two such measures seemed most promising, moreover—one reflecting the degree of black *economic dependence*, and the other the level of black *organizational development* in a county.

Economic Dependence and Participation: The "Simple Fear Model." So far as economic dependence is concerned, the discussion above, and the general literature on the southern caste system, suggest quite strongly that blacks in the Deep South are likely to be less fearful to the extent that they are not economically dependent on local whites and are therefore able to withstand at least a modicum of economic pressure. Economic dependence, however, does not seem to be merely a function of income. After all, the wealthiest class of blacks, the school teachers, are commonly acknowledged to be among the most dependent, for their income is controlled by white school officials who have not hesitated to keep the threat of expulsion constantly before potentially "uppity" black educators.³⁰ By contrast, black farm

owners, although generally far poorer, at least enjoy a modicum of economic independence. Not the *amount* of income, but the *source* therefore seems most crucial. Thus, the larger the proportion of blacks in a county who receive their incomes from sources relatively independent of local white control, presumably the greater the black political participation rate.

To test this idea, the U.S. Census' occupational classifications were divided into two basic groups according to how vulnerable the blacks holding these occupations would be to white economic coercion. Black farm owners and businessmen, for example, were placed in the "least vulnerable" category, and plantation laborers and maids in the "most vulnerable." (See Table 2 on the next page.) The number of blacks in each category was then computed as a percentage of the black voting-age population for each county. This figure became the basic index of vulnerability and formed the basis of what we will call the "simple fear model" of black participation.³¹

As a check on this occupation-related economic dependence figure, however, data were collected on another measure of black economic vulnerability—home ownership. The frequent stories of civil rights activists who were evicted from their homes in retaliation for registering to vote suggested quite strongly that black families who own their homes will feel less vulnerable, and hence less fearful, than

answer was an unequivocal yes. U.S. Commission on Civil Rights, *Mississippi Hearings*, 1965, I, pp. 207–215, especially p. 214.

³¹ It is important to note here that Matthews and Prothro, in their study of Southern black political participation, found little relationship between black occupational level and black political participation. However, Matthews and Prothro were content to divide occupations simply into the standard white-collar vs. blue-collar categories commonly used by sociologists and political scientists. In doing so, however, they were implicitly accepting a conceptual framework we have argued is inadequate to bring the key features of black politics in the South into focus. Matthews and Prothro, *Negroes and the New Southern Politics*, pp. 12–13.

Dividing the black labor force into "most vulnerable" and "least vulnerable" categories, however, is not a simple task. The Census classifications of occupations do not correspond very precisely to the distinction of interest to us here. For example, the occupations grouped together in the Census classification "Operatives and Kindred Workers" include highly vulnerable plantation tractor drivers and far less vulnerable railroad employees. The Census classification "Farmers and Farm Managers" includes both farm-owners and sharecroppers. To compile Table 2, therefore, it was necessary to make a number of difficult decisions, reflected in the Table notes. These decisions were based on the existing sociological literature on southern society, and on numerous interviews with civil rights activists in Mississippi.

²⁹ U.S. Commission on Civil Rights, *Political Participation*, p. 127.

³⁰ The most systematic evidence on the point is the four-county survey of black Mississippi school teachers conducted for the U.S. Civil Rights Commission. Commissioner Rankin summarized this study well when he asked the primary investigator if low teacher voter participation could not be attributed to "the advice of the superintendents and principals that 'you be careful or you might lose your job. . . .'" The

Table 2. Occupations of Nonwhite Labor Force Arranged According to Vulnerability to White Economic Pressure

Least Vulnerable	Most Vulnerable	Uncertain
Farmowners ^a	Farm Laborers	Clerical Workers
Manufacturing Workers ^b	Sharecroppers and Tenants	Sales Workers
Non-Farm Proprietors	Household Service Workers	Craftsmen
Workers Employed Outside County of Residence ^c	Unemployed	Operatives
Professionals ^d		Service Workers, Except Private Household

^a Instead of using the Census of Population classification "Farmers and Farm Managers," which includes tenants and sharecroppers, the number of black farmowners was taken from the *U. S. Census of Agriculture*, 1964. This created some incomparability in the figures, but not enough to make a difference.

Farmowners were considered relatively invulnerable because they could at least not be fired, and because they normally grew some of their own food, thus making them able to resist at least some economic pressures.

^b Instead of trying to cull the number of manufacturing workers from the occupation classifications of "Craftsman," "Operatives," and "Non-Farm Laborers," the *industry* classification "Manufacturing Workers" was used instead of the *occupation* classifications.

The decision to include manufacturing workers among the Least Dependent reflected the assumption that it is difficult to impose sanctions in an industrial enterprise because of the scale of the operation and the problem of training replacements. Manufacturing employers, moreover, were likely to be outsiders who would be less likely to impose economic sanctions on black employees who might vote.

^c The decision to include "Workers Employed Outside their County of Residence" in the Least Vulnerable category reflected the testimony of civil rights activists that whites have greater difficulty coercing blacks who have jobs in other counties.

Nevertheless, the inclusion of this factor caused some unavoidable overlap. Many of those who worked outside their county of residence were probably manufacturing workers, but there is no way to determine how many.

^d Inclusion of the "professionals" in the Least Vulnerable category was perhaps the most difficult decision of all. The largest group of professionals, the public school teachers, are hardly among the "least dependent." But other professionals, including doctors, lawyers, embalmers, federal government employees, and some preachers enjoy relatively great independence. The inclusion of the "Professionals" in the Least Vulnerable group rested on the assumption that the *percentage* of the nonwhite labor force employed as public school teachers would be roughly the same in each county so that any variations in the percentage of professionals in the county's nonwhite labor force could be attributed to the presence of professionals *other* than public school teachers.

those who are renting or occupying plantation shacks at the sufferance of the planter.³²

These measures of economic dependence were then tested against the data on black political mobilization. The results, depicted in Table 3, give ample support to the hypothesis that black voter turnout would be lower in counties where blacks are most vulnerable to economic intimidation. The correlation between dependence and voter turnout was high, statistically significant, and in the expected direction, no matter which index of dependence was used. As Figure 1 illustrates, voter turnout tended to be highest in those counties with the largest percentage of blacks in the least dependent occupations, precisely the pattern hypothesized. This "simple fear model," in fact, accounts for 25–30 per cent of the varia-

tion in black political participation among Mississippi counties.³³ Without a sizable cadre of blacks with relatively secure sources of income, the evidence seems to confirm, a county is unlikely to enjoy a high rate of black support for the Movement, despite the legal safeguards of the Voting Rights Act. The right to vote in the rural South, in other words, still carries a rather substantial price for blacks.

Organization, Economic Dependence, and Black Participation: The "Expanded Fear Model." Critical as economic independence seems to be in explaining black voter turnout, however, it hardly tells the whole story. As Figure 1 illustrates, Jasper and Kemper Counties, with the same proportion of blacks in dependent occupations as Amite and Jefferson

³² Homeownership figures came from the *U.S. Census of Housing*, 1960, Vol. I: States and Small Areas, Part 26, Mississippi (Washington, D.C.: U.S. Government Printing Office, 1963), pp. 38–44. Significantly, these two measures of black economic dependence were closely related. The correlation between the percentage of a county's black electorate in the *Most Vulnerable* occupations, and the percentage of blacks in owner-occupied homes in a county was -0.863 .

³³ The probability that this result might have happened purely by chance was a mere 0.0029, or only 3 in 1000. Using a 5 per cent level of significance, this result is therefore clearly significant.

Strictly speaking, the significance level or probability measure is not really applicable here because these counties are not perceived as a random sample from a larger population. Nevertheless, the significance level is still interesting as a benchmark.

Table 3. Correlations Between Indices of Black Economic Dependence and Black Political Participation, Twenty-Nine Mississippi Counties

Index of Vulnerability	Correlation Coefficient
Percentage of NW labor force in most dependent occupations ^a	-0.538*
Percentage of NW labor force in least dependent occupations	+ .518*
Black homeowners as percentage of black families	+ .414*

^a NW = Nonwhite.

* Significant at the .05 level.

Counties, nevertheless have black participation rates that are not even half as great. Blacks in Issaquena are more vulnerable economically than their brothers in Noxubee and Yazoo, yet support the Movement twice as much. In fact, it is possible to divide most of the twenty-nine counties into two groups, as indicated in Figure 1. Group A counties perform as expected in our hypothesis: participation increases as the percentage of persons in relatively independent

occupations increases. Group B counties, however, fail to conform to this pattern. For them, changes in the degree of dependence seem to make little difference in voter turnout rates. Obviously some other factors are at work. Economic independence may be a *necessary* condition for black participation, but it is *not sufficient*. While variations in the degree of black economic dependence account for 25-30 per cent of the variation in black voter turnout, they still leave 70-75 per cent unaccounted for. How can we account for this remaining variation? And does it discredit the view that "fear" is the cause of black non-participation?

A glance at the recent history of just one of the counties represented in Figure 1, Holmes County, suggests a possible answer to these questions. Blacks in Holmes County were no less economically dependent on local whites in 1967, when they elected the first black man in a century to the Mississippi Legislature, than they were in 1962, when fewer than a handful of blacks were willing to try to register to vote. What transpired between these dates, moreover, was not a change in white attitudes, but the emergence of a grass-roots, black political orga-

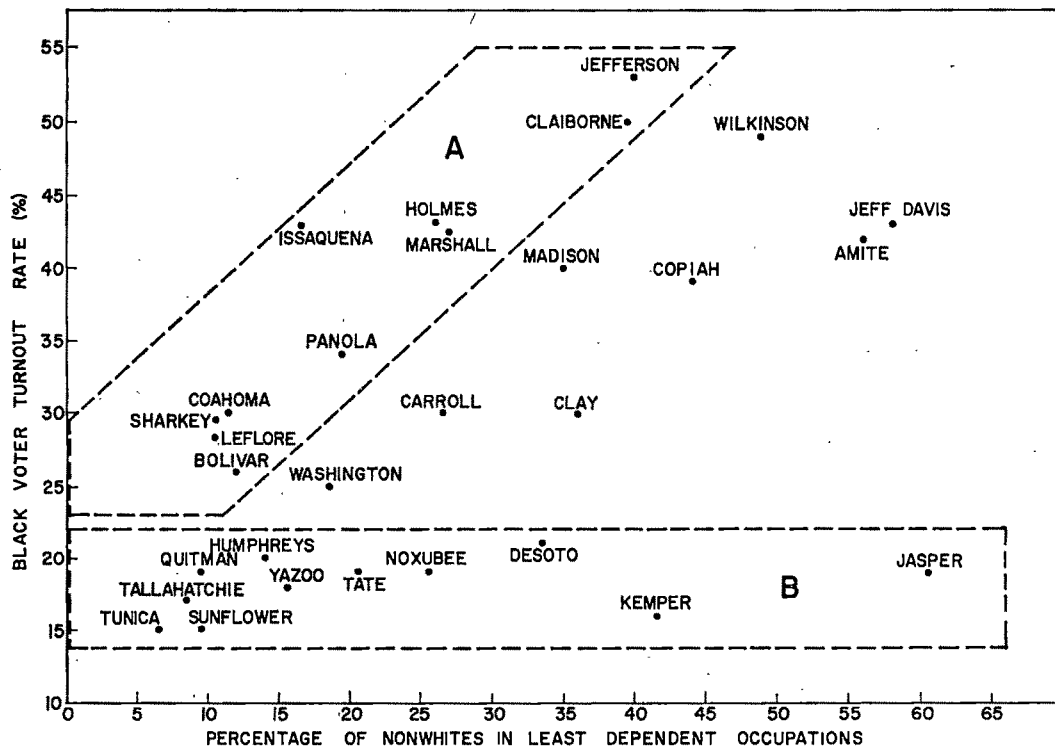


Figure 1. Comparison of Black Voter Turnout and Percentage of Non-White Voting Age Population in the "Least" Dependent Occupations in 29 Mississippi Counties.

nization. According to the two civil rights organizers who worked in the county during this period, the major consequence of this organizational development was to enable individuals to "manage their fear" through group solidarity.³⁴ What this suggests is that fear, at least fear of violence, can be mitigated by *organization*, by the creation of explicit bonds of trust that enable sizeable groups of individuals to coordinate their action and thus spread whatever risks flow from political involvement.

Given the conventional explanation that links organization to political participation by reference to the effect political organization has on reducing apathy and stimulating interest,³⁵ it is important to inquire why this might be so, why organization should be linked to participation through the medium of fear as well. The answer, we hypothesize, lies in the cost-benefit logic that confronts any individual contemplating political action in a context that has traditionally rewarded such action with violent reprisal. In the absence of organization, there is no way to share risks by orchestrating coordinated action by many. As a consequence, no one wishes to act, since the individual risks are too great. Furthermore, the very process of building an organization which would permit mass action will probably never develop spontaneously, because to begin it would require a very risky initiative on someone's part. So everyone is paralyzed—no one dares to move. But if organizations can be forged, some of this fear-induced paralysis can be relieved. If a thousand people march through the county seat one morning, the risk run by each marcher is only one in 1,000, while it would be closer to one in one for the first solitary picket or voter registrant in an unorganized context. As the number of people taking action grows, the risk being run by each individual falls until a point

is reached where the risk is acceptable, people will run it, and the paralysis is broken.

This hypothesized inverse relationship between the deterrent effect of reprisals and the amount of organization in the victim community has limits, of course. It assumes, first, that reprisals are in fact accurately directed at individual political activists or at least believed to be in the victim community. But it also assumes that there is some relatively low ceiling on the scope of these reprisals such that acts of violence cannot in practice be extended to all members of an organization as it grows. Both of these conditions seem to have been met in Mississippi during the 1960s, where violence was normally aimed at the blacks who first ventured into political action, and where the potential presence of the national news media and the FBI kept the absolute level of violence in check. Even so, there is no suggestion here that organization can be as effective an antidote to *fear of economic coercion* as it can be to *fear of physical violence*. Unlike physical violence, economic coercion is more readily hidden from national attention and hence has no practical ceiling on its use. Organization cannot cure all fear, therefore, particularly in the absence of economic independence. Finally, by suggesting that organization affects participation by lessening fear, we do not intend to suggest that it does not also have its other, more traditional kinds of impacts as well. However, the experience of Holmes County and two other counties studied intensively by one of the authors (LMS), as well as numerous interviews with civil rights activists in the South suggest quite strongly that the major consequence of organization was to relieve fear, particularly fear of violence. Our hypothesis, then, is that the level of fear in a county, and hence the level of black political participation, should vary with the degree of black organization, other things being equal.

To test this hypothesized relationship between organization, fear, and political participation, it was necessary to develop a measure of black organizational development for each county. Since intensive organizational analysis of all 29 counties was practically impossible, however, we measured organizational development indirectly, by determining the extent to which certain crucial noneconomic *facilitators* of black organizational development were present in each county. (This approach had the added advantage of making it possible to avoid tautologies by making it unnecessary to measure the extent of organizational development in terms of the presumed *effects* of the organizations.) A detailed examination of Movement

³⁴Sue and Henry Lorenzi, "Managing Fear by a Community—Holmes County, Mississippi," mimeo. The Lorenzi paper suggests that blacks in the rural South never *lose* their fear. Rather, they learn to *manage* it by participating in group activity that threatens the two-caste system. For a fuller analysis of the dynamics of black organizational development in three Mississippi counties in the 1960s, see Lester M. Salamon, *Backwardness and Change in the American South: Mississippi as a Developing Society* (Bloomington: Indiana University Press, forthcoming 1974), chapter 5.

³⁵William J. Crotty, "Party Effort and Its Impact on the Vote," *American Political Science Review*, 65 (June 1971), 439–50; John C. Blydenburgh, "A Controlled Experiment to Measure the Effects of Personal Contact Campaigning," *Midwest Journal of Political Science*, 15 (May 1971), 365–81; David E. Price and Michael Lupter, "Volunteers for Gore: The Impact of a Precinct-Level Canvass in Three Tennessee Cities," *The Journal of Politics*, 35 (May 1973), 410–38.

development in several Mississippi counties by one of the present authors (LMS) and discussions with numerous Movement leaders identified three such noneconomic facilitators of black organizational development: (1) the absolute size of the county's black population; (2) the percentage of the country's voting age population that is black; and (3), the extent of assistance from "outside organizers" to help communities take the first steps in managing their fear.

The first two of these facilitators of fear management and organization are relatively straightforward. Because organization is essentially a matter of trust and communication, it is naturally easier to organize a county if there are fewer people to reach. Similarly, it is easier to inspire confidence and induce people to take risks when you have a 4:1 voting majority than when you have a slim 3:2 majority. Moreover, both of these factors are easy to measure from readily available Census materials.

The role of outside organizers, however, is much more complex and much more controversial. Between 1961 and 1965, hundreds of young blacks and whites trooped to Mississippi under the sponsorship of SNCC and the Council of Federated Organizations (COFO),³⁶ and fanned out into the interior of the state to help local people organize themselves for political action. In addition, throughout this period the NAACP maintained in Mississippi a "field director" who attempted to organize black communities throughout the state.³⁷

In the wake of the "black power" movement of the mid-1960s, the contribution these "outside organizers" made to the development of black political organizations in Mississippi has been seriously questioned. Yet these "outsiders" had certain unique strengths that made it possible for them to play an important role in getting organizational development started. For example, the outsiders had connections with the national press that insulated them in part from the more violent forms of repression. They did not have to worry about economic reprisals

since they had no families in the area and were not dependent on local whites for their livelihood. They could therefore take greater risks than could local blacks. They were able to act as catalysts, taking the first critical steps to get organization started and absorbing the blame for the earliest, and most dangerous, Movement activity. What is more, they possessed a certain amount of "technical information" about how to conduct a meeting, about what other groups were doing, about what the laws said. Finally, they frequently provided a "shortcut" for individuals who wavered between commitment to the Movement and fearful inactivity. The mere presence of white students in particular created a dilemma for local blacks because of the sharp conflict between the rural norm of hospitality and the caste proscriptions on social contact with whites. When a cherubic white student "dropped by" to talk about voter registration, few black families could deny their heritage enough to treat the student inhospitably. The result was to induce many people to "cross the line," to defy the caste code, but to do so relatively effortlessly.

Significantly, outside organizer assistance was not distributed evenly among Mississippi counties. Some counties received heavy doses, particularly during the famous "Mississippi Summer" of 1964, while others were virtually ignored. To determine what level of organizer assistance each county received, eight persons with long experience in the Movement in Mississippi were asked to assign a score from 1-5 to each of the twenty-nine black majority counties in the state according to the amount of "outside organizer" assistance the county had received during the years 1960-68. The scores from each interviewer were then added to give an overall "organizer score" for each county.³⁸ Together with the measures of population size and the proportion of blacks in a county, this "outside organizer" score provides a way to assess the likelihood that a county has succeeded in managing its fear through organization. Together with the measure of *economic dependence* examined earlier, these three measures of *organ-*

³⁶ COFO was the coalition of civil rights groups formed in 1963 to sponsor voter education projects throughout Mississippi and to coordinate the black political battle in the state. At the height of COFO activity, in the historic summer of 1964, some 500 outside workers, mostly white students from the North, were in the state. "COFO files," Delta Ministry Office, Mt. Beulah, Edwards, Mississippi. See Watters and Cleghorn, *Climbing Jacob's Ladder*, pp. 63-68; Bel-
frage, *Freedom Summer*.

³⁷ Until his murder in 1963, the NAACP Field Director in Mississippi was Medgar Evers. Soon after this, Medgar Evers's brother, Charles Evers, took over this job and served until 1969, when he was elected Mayor of Fayette, Mississippi.

³⁸ The eight interviewees were: the Reverend Harry Bowie, originally a SNCC volunteer, who works now for the Delta Ministry in McComb, Mississippi; Lawrence Guyot, former state chairman of the Freedom Democratic Party; Aaron Henry, President of the Mississippi NAACP and co-director of COFO; Jan Hillegas, Editor of the Freedom Information Service Newsletter, a Movement publication; Charles Horowitz, Delta Ministry staff member; the Reverend Edwin King, former chaplain at Tougaloo College and Movement candidate for Lieutenant Governor in 1963; William F. Minor, Jackson correspondent for the New Orleans *Times-Picayune*.

Table 4. Test of the "Expanded Fear Model:" Impact of Four Fear-Related Factors on Black Political Participation in Twenty-Nine Mississippi Counties

Factor	% of Variation in Black Turnout Accounted for by Addition of this Factor (Partial r^2)	% of Variation in Black Turnout Accounted for by this Factor and All Above it (Multiple R^2)
Black Economic Dependence	28.9%**	28.9%**
NW Voting Age Population	0.6**	29.5**
Percentage of VAP Nonwhite ^a	17.0**	46.5**
Outside Assistance	22.1**	68.6**

^a VAP = Voting Age Population.

** Significant at the .01 level.

izational potential provide an overall index of vulnerability and fear for each county. The combination of these four, fear-related factors constitutes what we call the "expanded fear model" of black participation.

When this model is tested against the data on black political participation using multiple regression analysis, the results are striking. As Table 4 above indicates, variations in the absolute size of the nonwhite population in a county seem to add little to the explanation of variations in black participation already achieved by the "simple fear model" (partial $r^2 = 0.6$ per cent), but the other two measures of organizational potential—and hence of fear mitigation—contribute substantially indeed. In fact, taken together, these four, fear-related factors that comprise the "expanded fear model" account for almost 70 per cent of the variation in black participation rates. The probability that this result could have happened purely by chance, moreover, was so slight that it could not be computed. (See Table 4.)

Here, then, is powerful support for the argument that fear plays a vital role in black politics in Mississippi. This finding directly challenges the euphoric hope that the Voting Rights Act of 1965 eliminated, in President Johnson's words, the "last major shackles" impeding black political liberty in the South. The evidence suggests quite powerfully that the heritage of exploitation and repression, sustained by persistent economic dependence and coercion, may be doing to the Voting Rights Act of 1965 what it did to the Emancipation Proclamation a century before: transforming it into a legal form lacking social content.

Before we rest content with this conclusion, however, it is necessary to consider one further matter. To attribute variations in black political participation to variations in the degree of vul-

nerability of black communities is to assume that counties do not vary greatly in their treatment of blacks, or at least that these variations do not significantly affect turnout. The measures of fear used so far, after all, related only to the black communities themselves. Is it not possible that variations in the way whites treat blacks might affect turnout rates also?

To answer this question, it was necessary to develop an index of white hostility. The measures selected relied chiefly on voting statistics, since these statistics are most reliable and are available uniformly for all counties. In particular, white hostility to black power in a county was measured by the extent to which the county preferred Goldwater over Johnson in the 1964 presidential election, and the extent to which its voters turned out to support Wallace in the 1968 presidential election. Although indirect, these two measures do tap the degree of active white opposition to change in each of the 29 counties.³⁹

When these measures of hostility were tested against the data on black participation, the results suggested that variations in the degree of white hostility from county to county do not significantly affect black political participation. The correlations between support for Goldwater and for Wallace, the two indices of white hostility, and black voter turnout were too small to be significant. Indeed, what little relationship there was between white hostility and black political participation was in the wrong direction: instead of decreasing, black participation tended to *increase* with increases in white hostility to black rights. (See Table 5.) Moreover, adding these hostility factors to the "expanded fear model" presented above con-

³⁹ The Goldwater vote was computed as a percentage of the two-party vote, instead of as a percentage of the potential white electorate, because, in 1964, no real threat to white political dominance existed and whites therefore felt little need to turn out for the election. A measure of white turnout, therefore, would have been meaningless, whereas a measure of county preference for Goldwater over Johnson had social relevance in view of Johnson's support for social welfare and civil rights legislation.

By 1968, the presence of a real black challenge at the polls put a premium on white turnout. Not surprisingly, therefore, white turnout increased sharply. In 1964, both presidential candidates together received 409,146 votes; in 1968, Nixon and Wallace alone received 502,146 while Humphrey, drawing mainly on black voters, received an additional 149,419. In the 1968 election, therefore, the best measure of white hostility to black rights was the turnout of voters for Wallace as a percentage of the potential white electorate, for this measure permits us to determine the extent to which the white community in a particular county mobilized itself to express its conservative opinion.

tributes less than one-tenth of one per cent to the amount of variation accounted for.

These findings are consistent with the Matthews and Prothro findings that white violence—another measure of hostility—does not significantly influence black participation.⁴⁰ As already noted, however, the conclusion to be drawn from these findings is not that fear is not a key determinant of black participation, but rather that fear has a long half-life, that it persists even in the absence of specific incidents of violence or expressions of hostility. The variations in white hostility from county to county in the Mississippi black belt are simply not salient enough to give local blacks confidence that they are safer in some areas than others. Not the intensity of local white hostility, but the strength of the local black community is thus the key to dissipating fear and hence encouraging political participation.⁴¹

Altogether, then, these findings establish a *prima facie* case that fear is the cause of low turnout among black voters in Mississippi. They do not yet demonstrate, however, that the alternative models developed above, based on "relative deprivation" and "apathy," are incorrect. Indeed, it is even possible that our presumed measures of vulnerability and fear are really disguised measures of relative depriva-

⁴⁰ Matthews and Prothro, *Negroes and the New Southern Politics*, p. 308.

⁴¹ The fact that we discover a positive, if slight, correlation between white turnout and black mobilization suggests that what we are really witnessing here is a white response to the threat of black mobilization. The more blacks become mobilized, the more whites turn out to vote. This pattern is consistent with findings dating back to V. O. Key's *Southern Politics in State and Nation*, particularly pp. 513-516. It also conforms to the finding of Matthews and Prothro that there is a slight positive correlation between the incidence of violence and the level of black participation (Matthews and Prothro, *Negroes and the New Southern Politics*, p. 308). In both cases, the expression of white hostility is a response to black mobilization, not a cause of it.

Table 5. Correlations Between Two Measures of White Hostility and Black Participation Rates in Twenty-Nine Mississippi Counties

Measure of White Hostility	Correlation with Black Participation
Goldwater Vote as % of Total Presidential Vote, 1964	+ .202 (n.s.) ^a
Wallace Vote as % of White Voting Age Population	+ .240 (n.s.) ^a

^a n.s. = not significant at the .05 level.

Table 6. Correlations Between Two Measures of Discrimination Against Blacks and Black Political Participation in Twenty-Nine Mississippi Counties

Measures of Discrimination	Correlation with Black Participation
Nonwhite Median Family Income as a Percentage of White	-.102 (n.s.)
Nonwhite Median Education as a Percentage of White	+ .362 (n.s.)

tion and apathy. Before we can have real confidence in the "expanded fear model" of non-participation, therefore, it is necessary to see how well the other two "models" explain the observed variations in black voter turnout among Mississippi counties.

Discrimination and Black Political Participation

The "discrimination" model contends that variations in black political participation are the result of variations in the degree of discrimination blacks experience locally. The greater the discrimination, the model suggests, the greater the political participation.

Two measures of inequality were developed to test this model, one focusing on income and the other on educational opportunity. In particular, for each county, the black median family income and the black median education level were computed as percentages of the corresponding white figures for 1960. These indices provided a direct measure of the degree of deprivation a county's black citizens experience vis-à-vis the white citizens of the same county.

According to the "discrimination model," black participation should be highest in those counties where discrimination is greatest (i.e., where income and education levels of blacks are lowest *relative to those of local whites*). In fact, however, discrimination accounts for very little of the variation in black participation rates, whether discrimination is measured in terms of relative income levels or relative educational levels. Although the correlation coefficient in the case of educational deprivation was sizable, it was not in the expected direction. (See Table 6) Participation was higher in counties where relative educational deprivation was lowest. Finally, neither of these discrimination factors added significantly to the explanation of black political participation already achieved by the four fear-related factors

included in the "expanded fear model."⁴² Clearly, discrimination, at least as measured by these two factors, adds little to our understanding of the reasons for variations in black political participation among Mississippi counties.

Because this finding seems to fly in the face of so much social science research into the causes of lower-class protest, however, it may give rise to the suspicion that the measures of discrimination used here are faulty. Some reflection on the dynamics of southern rural society may dissipate these suspicions and make sense of these findings. Most students of protest movements acknowledge that deprivation has to be perceived to be politically relevant. In the case of rural blacks in Mississippi, however, the variations in degree of discrimination among counties are simply too small to notice, and they pale in comparison to the absolute differences between white and black income and education levels *within* each county. Moreover, the relevant reference groups for Mississippi blacks since the advent of radio and television include northern whites and blacks in addition to blacks in other Mississippi counties. Compared to these northern reference groups, Mississippi blacks must feel so deprived that they can hardly perceive any difference between their own condition and that of blacks in other Mississippi counties. For all of these reasons, it is not very surprising to discover that differences among Mississippi counties in the amount of black relative deprivation have no noticeable effects on black political behavior. Like the variations in hostility, the variations in discrimination against blacks among Mississippi counties are too insignificant to have a political impact.

Apathy and Black Political Participation

That variations in black vulnerability, and not variations in white hostility or discrimination, seem to account for variations in local black political participation still does not resolve all the doubts surrounding our explanation of black participation in terms of *fear*, however. The possibility still exists that what we have identified as measures of *vulnerability* may really be measures of something else entirely. Common sense, moreover, lends credence to such a possibility: it seems reasonable, after all, to expect that the degree of independence of a

black community would correspond rather closely to its level of education, income, and social status—the three factors normally identified with the dominant "apathy model" of political participation. Such a finding would not necessarily discredit the view that fear, and not these other variables, was really the *causal* factor. After all, a correlation or regression coefficient is only a measure of statistical relationship and cannot establish which of two interrelated factors is the cause. Nevertheless, the case for the contention that fear of economic harassment and intimidation is the real cause of low black political participation in Mississippi would clearly be stronger if it were possible to demonstrate that income, education, and social status—the three factors that comprise the "apathy model" of participation—were *not* also significantly related to black participation.

To test these notions, five different measures of income, four different measures of education, and one measure of social status were employed. The alternative measures of income and education, all derived from the 1960 *Census of Population*, are listed below:

Income

- NW* Median Family Income
- NW Median Individual Income
- Percentage of NW families with Incomes Under \$1000
- Percentage of NW families with Incomes Under \$2000
- Percentage of NW families with Incomes Over \$4000

Education

- Percentage of NW Population with No School
- Percentage of NW Population with 0-4 Years School
- Percentage of NW Population with Some College

*NW = nonwhite

Each of these measures permits us to focus on a slightly different dimension of the phenomenon. For example, the "percentage of nonwhite families with incomes less than \$2,000" measures the poverty population, while the "percentage of nonwhite families with incomes greater than \$4,000" measures the size of the black middle class.

Social status was measured by means of the Socio-Economic Status Index, a standard sociological measure combining measures of income, education, and occupation into a single index by means of a simple weighing formula devel-

⁴² Addition of the income discrimination variable to the "expanded fear model" boosts the explained variation by only 1.5 per cent, and the likelihood that any chance factor could have produced the same result was an unacceptable 30 per cent. Addition of the education discrimination factor contributes only 2.3 per cent to the explained variation, but with a probability of 20 per cent that the same result could have been produced by chance.

oped by the Population Division of the U. S. Census Bureau. This index thus has the virtue of providing a standardized measure of status, and therefore permits us to determine how relevant standard notions of status are to the particular situation of blacks in Mississippi.⁴³

Income and Participation. Of all the "apathy-related" correlates of political participation, income is probably the most important. In his comprehensive study of political participation in *Political Life*, for example, Robert Lane concluded that income affects voting turnout even more than does education.⁴⁴ In addition, the impact of income variations on black turnout is particularly important because of the implications it may have for the real meaning behind the "fear model." If economic independence, a prime component of the "fear model," turns out to be only a surrogate measure of income level, and if the level of income of a county's black population turns out to be a good predictor of its participation rate, then the whole interpretation of the dynamics at work presented above may be thrown into question.

In fact, however, neither of these relationships holds. In the first place, there is no significant correlation between a black community's income level and its degree of economic *dependence* as measured by occupation ($r = -.200$). Not surprisingly, therefore, there is no significant correlation between the income level of

⁴³ The nonwhite SES Index for Mississippi counties was taken from: Mississippi State College, "Socio-Economic Status Indexes for Mississippi Counties," by Ellen S. Bryant, *Bulletin* 724 (State College, Mississippi: Mississippi State University Agricultural Experiment Station, April 1966).

⁴⁴ As Lane points out: "Perhaps, for a simple conventional act such as voting, income is more important, while more complex forms of participation are more dependent upon qualities associated with education." Robert Lane, *Political Life*, p. 222.

Table 7. Correlations Between Five Measures of Income and Black Political Participation in Twenty-Nine Mississippi Counties

Measure of Income	Correlation Coefficient
Nonwhite (NW) Median Family Income	+ .011 (n.s.)
NW Median Individual Income	+ .274 (n.s.)
Per cent of NW Families with Income Less than \$1000	+ .055 (n.s.)
Per cent of NW Families with Income Less than \$2000	- .191 (n.s.)
Per cent of NW Families with Income Greater than \$4000	+ .152 (n.s.)

Table 8. Correlations Between Four Measures of Black Education and Black Political Participation in Twenty-Nine Mississippi Counties

Measures of Education	Correlation Coefficient
Per cent of NW Population with No School	- .331
Per cent of NW Population with 0-4 Years School	- .414*
Per cent of NW Population with Some College	+ .332
NW Median Education Level	+ .401*

* Significant at the .05 level.

a county's black population and its rate of political participation—no matter which measure of income we use. (See Table 7.) Evidently, poverty is *not* the real cause of black nonparticipation in politics: fear is, and fear born of economic peonage. The *source* of income, not simply the *amount*, is the crucial determinant of political liberty, as we suspected.

Education and Participation. Unlike variations in black income levels, variations in black educational levels among Mississippi counties do seem to have a significant impact on black participation rates. In particular, as Table 8 above suggests, black voter turnout tends to be highest in counties with relatively high educational levels, and lowest in counties with low educational levels. What is more, because a county's black educational level turns out to be closely related to the degree of black economic vulnerability,⁴⁵ this finding raises the possibility that the effect we have ascribed so far to economic invulnerability should really be ascribed to educational attainment.

Whether this finding discredits the view that fear, and not apathy, really accounts for variations in black political participation rates among Mississippi's 29 black majority counties, however, depends on the interpretation attached to these educational variations. A correlation, after all, is simply a measure of relationship and cannot by itself "explain" the relationship. Rather, explanation can come only from careful analysis of the dynamics of the particular social situation.

In most of the literature on political participation, to be sure, the link between education and participation is explained in terms of inter-

⁴⁵ The correlation between nonwhite median education and the percentage of a county's labor force in Vulnerable Occupations is $-.773$, an extremely high figure.

est and understanding, both of them aspects of apathy. Campbell, Converse, et al., for example, argue that educated people participate in politics more than the uneducated people do because the educated command more "facts about politics" and have more sophisticated concepts with which to "maintain a sense of order and meaning amid the flood of information. . . ."⁴⁶

In the context of Mississippi society, however, this interpretation of the social meaning of the education figures is open to serious question. In the first place, it is questionable whether the number of years that blacks spend in local public schools has any relation at all to the amount of politically relevant knowledge they have acquired. Mississippi black public schools are notoriously bad places to learn about politics, for discussion of politics has long been *verboten* in these citadels of conservatism. In the study of the reasons for the failure of black schoolteachers to register to vote in Mississippi cited earlier, for example, Prothro discovered that even social science teachers neglected to discuss politics in class for fear of losing their jobs.⁴⁷

In the second place, while the public schools failed as providers of politically relevant information, other sources filled the gap; but these sources were available to the educated and the uneducated alike. Television, which is now widely available in rural Mississippi, permitted even the poorly educated to learn their constitutional rights from the lips of Dr. Martin Luther King at Montgomery, and Selma, and Washington, D.C. The evening news on radio and television provided nightly lessons in political participation that were unavailable in the classroom. To assume, in short, that formal education signifies greater political comprehension may be to misread the facts. But how, then, are we to explain the clear statistical link between the level of nonwhite formal education and the rate of black political participation?

One answer, it seems, lies in an alternative explanation of the meaning of education in Mississippi's "closed society."⁴⁸ Rather than measuring the amount of understanding of pol-

itics, formal education for blacks in Mississippi may in reality be another measure of "sense of security." The relevant social fact buried in the education statistics may really be the level of literacy in a county, not the level of political sophistication and understanding. And literacy is more properly an index of vulnerability than of comprehension. A person who can read may know little more about politics than one who cannot in rural Mississippi, but he probably has some confidence that his ability to read will save him from at least some dangers. The illiterate lacks even this meager sense of security. Feeling more vulnerable as a result, he naturally experiences greater fear. What is more, the illiterate voter does not even enjoy the "secret ballot" in elections. To vote, he must enlist the assistance of a polling official (who is virtually always a local white) and must vote aloud. In an already frightening situation, the prospect of voting out loud for the Movement's candidates must deter many illiterate blacks from voting, or else induce them to vote against the black candidates.⁴⁹ The observed correlation between education and participation, therefore, may really reinforce, not contradict, the argument that fear is the cause of nonparticipation.

This conclusion finds support, moreover, in the data reported in Table 8 above. The measure of educational attainment that was most powerfully related to black participation was none other than the one measuring *the extent of illiteracy* in each county (namely, the percentage of blacks more than 25 years old with 4 years or less of education). Perhaps then, education in these counties affects participation through the mechanism of fear, not through the mechanism of political comprehension and apathy. Further support for this conclusion can be found in the fact that adding the education factor to the factors already measuring vulnerability in the "expanded fear model" produces only a marginal addition to the amount of variation in black participation rates accounted for.

Social Status and Participation. The only other major apathy-related factor that could be hiding in the data on black dependence and vulnerability is "social status." Numerous scholars have discovered that higher-status individuals participate in politics more regularly than do those of lower status, and have explained this phenomenon in the now familiar terms of apathy: higher-status individuals have greater self-esteem, a greater sense of personal efficacy, and therefore a greater interest in poli-

⁴⁶ Angus Campbell, Philip E. Converse, Warren E. Miller, and Donald E. Stokes, *The American Voter: An Abridgment* (New York: John Wiley and Sons, Inc., 1964), p. 251.

⁴⁷ U. S. Commission on Civil Rights, *Hearings*, Jackson, Mississippi, 1965, I, 211-12.

⁴⁸ The term "closed society" was used by historian James Silver to characterize the lack of political liberty in Mississippi in the late 1950s and early 1960s. See: James W. Silver, *Mississippi: The Closed Society* (New York: Harcourt, Brace, and World, Inc., 1963), especially p. xvi.

⁴⁹ For an example of how these pressures worked, see: Salamon, "Mississippi Post-Mortem," pp. 45-7.

tics. If what we have earlier taken to be measures of dependence and vulnerability turn out to be surrogates for measures of social status, the whole argument developed above would again be thrown into question.

At first glance, the data seem to confirm this impression. The correlation between nonwhite socioeconomic status indexes and black voter turnout was a significant $+.440$, suggesting that the social status of a county's black population is a fairly good predictor of its political participation rate. The problem, however, is that much of this result reflects the influence of the education component of the SES index, not the income or occupational components.

To separate out these influences, the three components of the "apathy model" were tested together using multiple regression analysis to assess the explanatory power of the model as a whole. The results, reported in Table 9 below, indicate that even if we interpret income, education, and social status the way exponents of the "apathy" theory of political participation suggest, "apathy" can still account for only 23 per cent of the variation in black participation rates, compared to the 69 per cent accounted for by "fear."

Conclusions and Implications

Of the three possible explanations of variations in black political fortunes in Mississippi's 29 black majority counties formulated earlier, therefore, the explanation based on "fear" is clearly the one that finds most support in the evidence, as Table 10 indicates. Taken together, the four measures of vulnerability embraced in the "expanded fear model" account for 69 per cent of the variation in black political participation rates, with a probability approaching zero that this result could have occurred by chance.

Table 9. Test of the "Apathy Model": Impact of Three Apathy-Related Factors on Black Political Participation in Twenty-Nine Mississippi Counties

Factor	Percentage of Variation in Black Participation Accounted for by this Factor and All Above It (Multiple R^2)
Median NW Education	16.0%*
Median Family Income	16.1
Socioeconomic Status Index	23.4

* Significant at the .05 level.

Table 10. Comparison of Three Models of Black Political Participation in Twenty-Nine Mississippi Counties

Model	Percentage of Variation in Black Participation Rates Accounted For (Multiple R^2)
"Expanded Fear Model"	68.6%**
"Discrimination Model"	10.3
"Apathy Model"	23.4

** Significant at the .01 level.

The nearest competitor is the "apathy model," but it explains only 23 per cent of the observed variation, and this result could have happened purely by chance more than 5 per cent of the time, rendering this model statistically insignificant. While these findings do not formally "prove" that fear is the real explanation of black voting patterns, they certainly create a strong presumptive case for this conclusion.

The evidence presented here thus squarely challenges the hopeful assumption embodied in the 1965 Voting Rights Act, the assumption that the elimination of the formal, legal barriers to black voting in the Deep South would, by itself, unleash latent "forces of democracy" presumably lying dormant in the southern hinterlands. This assumption has been sustained over the past half-decade only by seizing on every black political victory as proof of its validity while ignoring the bleak record of defeat or attributing it to black apathy or contentment. By contrast, the data analyzed here call for a more thorough rethinking of the preconditions for democracy in the Deep South. If economic dependence and fear of intimidation frustrate political participation for blacks in the Deep South, as the evidence here suggests, then democracy can not be achieved simply by formal legal manipulations of the sort embodied in the Voting Rights Act, important as those manipulations are as a first step. Nor, significantly, will it be achieved simply by raising black living standards. Income, we found, was a singularly poor predictor of black political mobilization. So long as increases in income do not liberate blacks from direct economic dependence on local powers-that-be, and thus leave blacks vulnerable to intimidation or fear of intimidation (as has been the case, for example, with the teachers), improvements in income per se will have little impact on political participation. In other words, not the *amount* of income, but the

source is the crucial factor in determining participation.

The same is true, moreover, with regard to education. Education, we suggested, affects participation mainly to the extent that it relieves feelings of insecurity and vulnerability. But its ability to do so is severely circumscribed by the social and economic relationships in the society at large, and particularly by the persistent fear of coercion carried over from the past, and reinforced frequently enough in the present to survive into the foreseeable future.

The pursuit of democracy in the Deep South, therefore, must acknowledge the continued impact of economic vulnerability and fear on political action. Democracy will emerge only to the extent that these conditions—and not just the formal, legal barriers—are eliminated.

This conclusion not only has analytical implications, however; it has policy implications as well. In particular, in our view, it throws new light on the potential importance of a truly nationally administered welfare program such as the Nixon Administration's original Family Assistance Plan hinted at. Whatever its specifically economic impact, the analysis presented here suggests that such a scheme could have an even more important *political* impact. By taking control of welfare payments away from local powers-that-be, it could liberate a sizable army of "welfare serfs" from the fear of losing their sole source of income if they support black candidates for political office. No single program under serious consideration today, we believe, could protect so large a segment of southern blacks from the fear of economic intimidation and thus open the way for expanded black political participation.⁵⁰

⁵⁰ For further discussion of this point, see: Lester M. Salamon, "Family Assistance: The Stakes in the Rural South," *The New Republic* (February 20, 1971), pp. 17-18; and Stephen Van Evera, "Welfare Reform: Bonanza for Dixie's Blacks," *Progressive*, June 1972, p. 27. Recent versions of this idea, by eliminating the

The findings presented here may have broader implications as well, however, for the Voting Rights Act of 1965 falls squarely into the same tradition as the one that has guided American foreign policy in the "developing nations" for decades. With its stress on the politico-legal barriers to democracy in the Deep South, the Voting Rights Act reflects what Robert Tucker has termed America's "instrumentalist bias," its preoccupation with the outward trappings of democratic government in the "developing societies" and its neglect of the substantive underpinnings of democracy.⁵¹ Americans, political theorist Louis Hartz has noted, characteristically ignore the "deeper social struggles" under way in the developing nations and concentrate instead on the more pragmatic question of procedure. "We fail," Hartz notes, "to appreciate *non-political definitions of freedom* and hence are baffled by their use."⁵²

By demonstrating the limitations of this politico-legal approach to democracy even in the Deep South, where it receives constant sustenance from dominant national norms and plentiful national economic resources, the findings presented here thus raise even more serious questions about its prospects abroad. These findings suggest that the accepted wisdom among political scientists about the causes of nonparticipation in politics may be seriously in error, at least as it applies to disadvantaged populations in politically underdeveloped areas. Apathy, it seems, has carried too large a part of the explanatory burden for too long. It is time to acknowledge as well the role of fear.

provision for national administration, threaten to destroy the potential liberating impact of the plan.

⁵¹ Robert C. Tucker, "Russia, the West, and World Order," in Robert Tucker, *The Soviet Political Mind* (New York: Praeger Publishers, Inc., 1963), pp. 181-82.

⁵² Louis Hartz, *The Liberal Tradition in America*, A Harvest Book (New York: Harcourt, Brace and World, Inc., 1955), p. 306.

Comment: A Re-evaluation of Black Voting in Mississippi*

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Lester Salamon and Steve Van Evera have undertaken a difficult task. To choose among competing explanations of individual political behavior which for the most part make similar predictions—as do their “apathy” and “fear” models—should be difficult enough, but to do so with aggregate, county level data would seem nearly impossible. Yet the authors make the effort and are rewarded with some unexpected and interesting findings. Most surprising and central to their “fear model” of black voting in Mississippi is the discovery that indices of economic vulnerability (or dependence), and not poverty or education per se, most closely relate to black voter turnout. On the basis of this finding they recommend a federally administered welfare program such as President Nixon’s Family Assistance Plan. Not only would it increase the number of beneficiaries and raise the level of income for many impoverished blacks, but more importantly it would liberate them from the coercion of “welfare serfdom,”—as the authors put it—at the hands of local white welfare administrators. A second unexpected finding, and it seems to me underappreciated by the authors, is the strong positive relationship between the percentage of the voting age population that is black and black voter turnout. This discovery is especially curious and noteworthy since earlier research (to be described below) agrees in reporting a *negative* relationship between the percentage of blacks in a community and the level of black voting registration.¹

* I would like to express appreciation to Professor John Quincy Adams for generous assistance in promptly locating answers to a multitude of questions. I would also like to thank Dianne Kernell, Byron Shafer, and Harry Williams for their helpful comments on an earlier draft and Judy Sampson for assistance in preparation of the data and manuscript.

¹ Donald R. Matthews and James W. Prothro, *Negroes and New Southern Politics* (New York: Harcourt, Brace and World, 1966), pp. 115–120. Donald S. Strong, “The Future of the Negro Voter in the South,” *Journal of Negro Education*, 26 (1957), 400–407. Johnnie Daniel, “Negro Political Behavior and Community Political and Socioeconomic Structural Factors,” *Social Forces*, 47 (March, 1969), 274–280. A weaker relationship between these two variables is reported by John H. Fenton and Kenneth N. Vines in “Negro Registration in Louisiana,” *American Political Science Review*, 51 (1957), 704–713.

Before embracing the authors’ policy recommendations or revising conventional wisdom on the subject, several questions should be considered about their analysis and conclusions:

(1) Is the fear model adequately discrete? In its “simple” form, the fear model posits that economic vulnerability and not socioeconomic status best explains nonvoting. At this stage the model presents a test which may either confirm or deny its hypothesis, and may, therefore, be useful. When it is “expanded,” however, to include standard organizational explanations of participation, serious problems surface. For one, the model no longer provides a test to demonstrate that fear is operating. Although the authors provide a fascinating and plausible account of how organization may increase black participation by displacing fear, organization may also operate to “displace” apathy resulting from generations of poverty, poor schooling, and a heritage of nonparticipation enforced by law. Even without systematic economic intimidation extensive political organization would be necessary to stimulate mass political participation among Mississippi’s black citizenry. American history is replete with instances of political organizations successfully enfranchising and mobilizing large and otherwise nonparticipant segments of the population for whom fear as economic coercion was not a primary restraint from participation.² The point is that “fear” is not necessary to our understanding why organized counties record higher levels of black voting than unorganized counties.

A second problem with the “expanded fear model” is that it is too versatile. For example, with respect to the important variable, percentage of the county’s voting age population that is black, fear can account for either a positive or negative relationship with black participation. The authors find a positive correlation and argue that percentage black is an important organizational resource and as such helps to displace fear. The earlier studies mentioned above, however, found a negative correlation and proffered an explanation equally congenial to the authors’ fear model. Whites threatened by

² The most notable example is the growth and operation of urban political machines during the 19th and early 20th centuries.

black influence and control in local elections more thoroughly exercise social and economic sanctions against participation in communities where blacks constitute a large portion of the population.³ Thus, as the percentage of the voting age population that is black increases, voting becomes a more dramatic and forboding act and vulnerability becomes a powerful inducement to stay home. Stated and tested simply then, either finding can be rather easily incorporated into their model. Later in the paper, I shall suggest a framework for reconciling the earlier research with the authors' present finding. But in its current condition the "expanded fear model" subsumes relationships which may have little to do with fear and permits *post hoc* confirmation by opposite findings.

(2) Are the findings accurate? The dependent variable, black voter turnout, is measured during the 1968 presidential election, while the data for independent variables are obtained from the 1960 census, some nine years earlier. During the 1960s many Mississippi counties experienced considerable change in the relative size and demographic features of their black populations. For example, by the 1970 census five of the twenty-nine counties with black population majorities in the 1960 census no longer had black majorities. Moreover, there were substantial changes in their collective social and economic characteristics. During these ten years the percentage of the black population with a high school education doubled—despite the fact that outward migration appears to be very high among young, educated adults.⁴ The percentage of the black population with a yearly income under 2,000 dollars declined from two-thirds in 1960 to less than one-third in 1970, and the percentage with less than a 4,000 dollar yearly income dropped from ninety-one to sixty per cent. Perhaps the most dramatic changes occurred in agriculture. With widespread mechanization, tenant farming and sharecropping became an uneconomical system of agricultural production, and, as Table 1 shows, tenancy experienced a precipitous decline during the 1960s. What was at one time the primary means of livelihood for blacks, today has virtually disappeared in many counties. There is also a slight but steady decline in the number of black farmowners.

Although for many counties throughout the nation 1960 census data may adequately approximate the characteristics of its 1968 population, in Mississippi when looking at the black

Table 1. Drop in the Number of Black Mississippi Tenant Farmers during the 1960s

	1959	1964	1969
Number of Black Farmers	54,927	37,715	17,184
Number of Tenants	32,243	18,580	2,657
Percentage of Farmers Who Are Tenants	58.7	49.3	15.4

Source: 1969 U.S. Census of Agriculture, Mississippi (Part 33, Section 1, Volume 1) Table 3, p. 3.

population which is undergoing rapid social change this may not be the case. Thus, any judgment of the relative effects of income levels, education, racial composition, and farm tenancy (a primary component of Salamon and Van Evera's economic vulnerability index) on black political participation must be tentative until supported by the more accurate data available in the 1970 census.

(3) Are the findings generalizable? Salamon and Van Evera begin by asking why black candidates have failed to win many elections in counties where they appear to constitute a voting majority. This naturally led to an analysis of twenty-nine counties which had black population majorities (although not all of these, even by 1960 figures, actually had voting age majorities). Their findings support certain hypothesized causes of low participation in the important black belt counties of Mississippi, yet they may be wholly inappropriate for explaining participation in other counties with smaller proportions of black population. Because the proportion black in a community (the dimension on which the twenty-nine counties were selected for examination) may itself be related to the independent and dependent variables, we cannot simply assume that the reported relationships are pervasive throughout the state. Given the provocative nature of the findings, the authors are naturally prompted to state general conclusions and make policy recommendations. Before this becomes acceptable, however, the general applicability of their findings must be ascertained.

With these questions in mind, the analysis will be replicated with some important modifications. First, 1970 census data will be used to measure the independent variables. As indicated above, there is reason to suspect that the figures for the various social and economic indices are significantly different from those reported in the 1960 census. (For a description of index construction and intercorrelations among selected independent variables see Appendix A.) Second, the investigation will be expanded to include all eighty-two of Mississippi's counties, providing an opportunity to test the

³ Matthews and Prothro, 115–117.

⁴ John P. Thames, "Population Migration Effects on Mississippi Poverty," *Mississippi Law Journal*, 39 (May, 1968), 423–450.

reported findings in counties with varying racial composition. In order to do this the dependent variable, black voter turnout, must be operationally redefined. The previous study averaged the Humphrey vote and the vote for local black candidates (when available) to arrive at a "mobilization index." In this analysis only the Humphrey vote will be employed, since in 1968 only a relatively small number of counties had black candidates running for office.⁵ Salamon and Van Evera report that the Humphrey vote correlates with the vote for the local black candidate at .92; therefore, the difference in the two indices should have little effect on the results. (For a discussion of the validity of using the Humphrey vote as a measure of turnout for the other Mississippi counties, see Appendix B.) Third, the stability of the findings over time will be checked by examining black turnout during the 1971 election in which Charles Evers, the black Mayor of Fayette, Mississippi, ran an unsuccessful statewide campaign for governor against the regular Democratic nominee, William Waller.⁶ By using more accurate census figures, expanding the scope of the study, and extending it over time we obtain a richer and more reliable set of data for examining the effects of fear, apathy, and politics on black political participation.

Replicating the Analysis

Three principal explanations of variations in black turnout have been described in the previ-

⁵ The county level voting returns for the 1968 presidential election are taken from F. Glenn Abney, compiler, *Mississippi Election Statistics* (University, Miss.: Bureau of Governmental Research, 1968), supplement, no pages given.

⁶ The general election was held in November, 1971. Charles Evers, running as an Independent, as did many other black candidates for local office, received 23 per cent of the total vote. County level returns are taken from . . . *In the Public Interest* (Millsaps College: Institute of Politics in Mississippi) Vol. 2 (June, 1972), 3.

ous paper. They are apathy, presented as the conventional social science model and measured by the standard socioeconomic measures; fear as measured by occupational vulnerability; and political organization which in this analysis can only be indirectly indexed by percentage black of the voting age population. The last two were combined to form the "expanded fear model" of participation. Because of the objections to this merger raised earlier, however, each explanation will be evaluated as separate and distinct. This does not, of course, preclude the possibility that two or indeed all three explanations may be operating simultaneously and in a complementary manner.

In Table 2 the multiple correlations for the variables central to the several explanations are displayed.⁷ In some respects the results are similar to those discovered by Salamon and Van Evera. The indices of occupational vulnerability consistently correlate with black voting participation at about the same levels as the joint correlation of median education and family income. The important point here is that the coefficients for the vulnerability variables reported in Table 2 are not nearly so large as those reported by Salamon and Van Evera in their Table 3. It is curious that 1960 data would produce stronger correlations with voter

⁷ The reader is cautioned to remember that the relationships are for ecological variables and that the danger of unjustifiably inferring individual-level associations from correlation coefficients is great. Because individual cases have not been grouped (into counties) according to their scores on the dependent variables (black voter turnout in 1968 and 1971) unstandardized regression coefficients should be more revealing about individual level "effects." Where opportune, regression slopes based on b scores will be used. For a lucid discussion on this point see W. Phillips Shively, "Ecological Inference: The Use of Aggregate Data to Study Individuals," *American Political Science Review*, 63 (December, 1969), 1183-1196. See also Hubert M. Blalock, Jr., *Causal Inferences in Nonexperimental Research* (Chapel Hill: Univ. of North Carolina Press, 1964), chapter 4.

Table 2. Correlations of Selected Socioeconomic Variables with Black Voting Participation in Mississippi

	1968		1971	
	All Counties	29 Selected Counties	All Counties	29 Selected Counties
(Multiple Correlation Coefficients)				
<i>Vulnerability Indices</i>				
% Vulnerable				
% Invulnerable, Revised	.12	.25	.37	.42
<i>Socioeconomic Indices</i>				
Med. Education				
Med. Family Income	.26	.27	.44	.59
% of Voting Age Population, Black	-.04	.41	.19	.40

turnout in 1968 than the more accurate, updated figures. Some of the difference could conceivably lie in the slightly modified indices (refer to Appendix A and Table 2 of the preceding paper). A more probable reason for such a pronounced discrepancy, however, is that vulnerability as measured in 1960 compared with 1970 represents essentially a different variable. The size of the tenant population given in Table 1 heavily contributes to the total percentage of the labor force in dependent occupations in 1960, especially in the black belt counties of the Delta, the last stronghold of the southern-style plantation system. (Almost all of the Delta counties are included among the authors' twenty-nine predominantly black counties.) Reflecting the massive decline of farm tenancy recorded by the 1969 agricultural census, this occupational category no longer contributes so heavily to the percentage of dependent workers. By late 1968 the 1960 census figures of the percentage in vulnerable occupations in a county may reveal less about the individual features of the county's black labor force than it does about a general rural profile of the county. Perhaps it is this contextual element which adds to the explanatory power of Salamon and Van Evera's measure.⁸

As with Salamon and Van Evera's finding, the percentage of the voting age population that is black (percentage V.A.P., Black) closely correlates with participation in the twenty-nine counties with a large black population for both the 1968 and 1971 elections. If the same causal relationship exists throughout the state, then the statewide correlations should be even larger, given the increased variation. Table 2, however, reveals no relationship at the statewide level in 1968 and only a weak one for

the 1971 election. This finding suggests that the percentage V.A.P., Black should be more closely scrutinized within ranges of black-white population ratios; in areas where there is a small black proportion this variable may operate differently.

All that the multiple correlations for the vulnerability and socioeconomic indices in Table 2 can do is to suggest that the two sets of variables are equally powerful in explaining variation among counties in black voter turnout. They tell us nothing about the relative merits of the two explanations. For this, partial relationships are needed. In Table 3 all five variables covering the three explanations are included in a multiple regression analysis. The coefficients are beta's, standardized regression coefficients similar in this case to fifth order partial correlation coefficients.⁹ Assuming that these variables are causally independent, we can compare the relative contribution of each to black turnout. Given the small N upon which the computations are based, the reader is advised not to pay too much attention to the absolute value of the coefficients or to small differences among them. Instead the table is intended to display which variables are "operating" under controls and which are not. Clearly the vulnerability indices, with only one exception (per cent vulnerable in 1971, for all counties), fail to display sizable relationships under controls.¹⁰ Thus, fear as

⁹ Hubert M. Blalock, Jr. describes this as the "quantitative" criterion for evaluating the relative importance of independent variables in "Evaluating the Relative Importance of Variables," *The American Sociological Review*, 26 (December, 1961), 866-874.

¹⁰ Salamon and Van Evera use percentage of black families who own their homes as a check on the occupational dependence variables but subsequently drop this index from their analysis. Including percentage of black homeownership with the other two vulnerability indices slightly increases the total multiple correlation coefficient. Upon close inspection the per cent of black homeownership was found largely to duplicate the effects of the occupational measures, and therefore it will not be employed in this analysis. Housing data were taken from the *1970 Census of Housing* (part 26: Mississippi [Washington: G.P.O., 1971]).

⁸ For a brief review of the literature and discussion of the employment of ecological data to measure contextual or structural variables see Tapani Valkonen, "Individual and Structural Effects in Ecological Research" in *Quantitative Analysis in the Social Sciences*, ed. Mattei Dogan and Stein Rokkan (Cambridge, Mass.: The M.I.T. Press, 1969), pp. 53-68.

Table 3. Partial Regression Coefficients (Beta's) for All Five Independent Variables

Election Year	Data Base (counties)	% Vulnerable	% Invulnerable, Revised	Median Fam. Inc.	Median Education	% VAP, Black	Total Variance Explained (r^2)
1968	All	-.04	-.07	-.24	.51	.12	8.3%
	29	-.05	.13	.28	.13	.60	36.5%
1971	All	-.33	.16	-.44	.51	.45	38.2%
	29	.02	.04	-.17	.68	.48	54.6%

economic intimidation is seriously weakened as the primary model of black voting. Median family income does retain some correlation, but in three of four instances the direction switches from positive to negative. Although this does not necessarily mean that *among individuals* increasing income will produce nonvoting it does suggest what Salamon and Van Evera concluded from their findings, that the impoverished condition of the black community does not have any independent depressant effects on county-to-county variations in black turnout.¹¹

Education emerges from the regression analysis as the single most important independent variable. With the exception of the twenty-nine heavily black counties in the 1968 election, median educational level of black adults has a pronounced independent effect on turnout. This finding is certainly consonant with a massive amount of research which shows that for individuals education is closely tied with various forms of political participation. Salamon and Van Evera also found that median education is a powerful predictor of voting, and after a cursory analysis of the correlations of various levels of education with turnout (see their Table 8) concluded that education reflects fear more than apathy. They argue that since the percentage of the black population with little or no schooling correlates more closely with turnout ($r = -.41$) than does the percentage with some college ($r = .33$), then illiteracy must be the main reason for nonparticipation. Because illiterates often require assistance in voting, usually from white election officials and poll watchers, they are more susceptible to intimidation.¹² Undoubtedly such incidents of intimidation have occurred and do occur, but with the data available it is impossible to determine whether illiteracy primarily reflects fear or apathy. We can only speculate on the cause. Even with fear absent we should expect to find illiteracy highly correlated with nonparticipation. Yet the authors conclude, "Education, we found, affects participation mainly to the extent that it relieves feelings of insecurity and vulnerability." The manner by which education affects voting among blacks in Mississippi is more problematic than Salamon and Van Evera acknowledge.

In Table 5 of Appendix A, the percentage of

¹¹ Median family income is closely related to community poverty levels, correlating $-.93$ with percentage of black families listed in poverty by the 1970 census, and $.88$ with the percentage having less than a 2,000 dollar yearly income.

¹² Recent cases of white election officials either misdirecting or not assisting black illiterate voters is documented in *The Shameful Blight* prepared by the Washington Research Project (Washington, October 1972), pp. 82-87.

the voting age population that is black is shown to correlate strongly with the main independent variables representing the fear and apathy models. Thus, the possibility exists that the effects of percentage V.A.P. Black are spurious and will be eliminated with the introduction of appropriate controls. The regression coefficients in Table 3 refute this hypothesis completely. In each instance the percentage V.A.P. Black increases in its relative importance over its zero-order relationship, and for three of the four regressions, it is among the most powerful variables present. *To conclude, median education and the percentage of the voting age population that is black emerge under controls as the most influential variables in producing county-to-county variations in black turnout.*

A Closer Look at the Racial Composition of the Electorate

Black voting is rather curiously related to the proportion black of a county's voting age population. At the statewide level weak zero-order correlations between these two variables are shown in Table 2, yet for the twenty-nine heavily black counties, there is a strong positive relationship between percentage V.A.P. Black and turnout during both elections, even under multiple controls. Apparently the effects of this variable on turnout are shaped by the racial composition of the sampled counties. In Figure 1 this suspicion is confirmed. For both elections the relationship is similarly curvilinear and nonmonotonic.

The means of percentage voting in Figure 1 plot the strong positive relationships first identified in Table 2 between percentage V.A.P. Black and turnout in the heavily black counties. A closer examination of the means across the range of percentage black reveals that this association occurs only after the black proportion of the voting age population reaches the sixty per cent level. This finding fits neatly into Salamon and Van Evera's discussion of this variable as an organizational resource. *Only in settings where black candidates have a good chance of winning do increases in proportion black have a positive effect on black turnout.* Apparently when the potential black majority is between 50 and 60 per cent the practical chances are too slim to stimulate much activity, and below 50 per cent the likelihood of winning is remote indeed.

In counties where the black citizenry constitutes only a small proportion of the total electorate an inverse relationship appears; among counties with less than 30 per cent of the eligible voters black, the correlations between percentage V.A.P., Black and turnout are

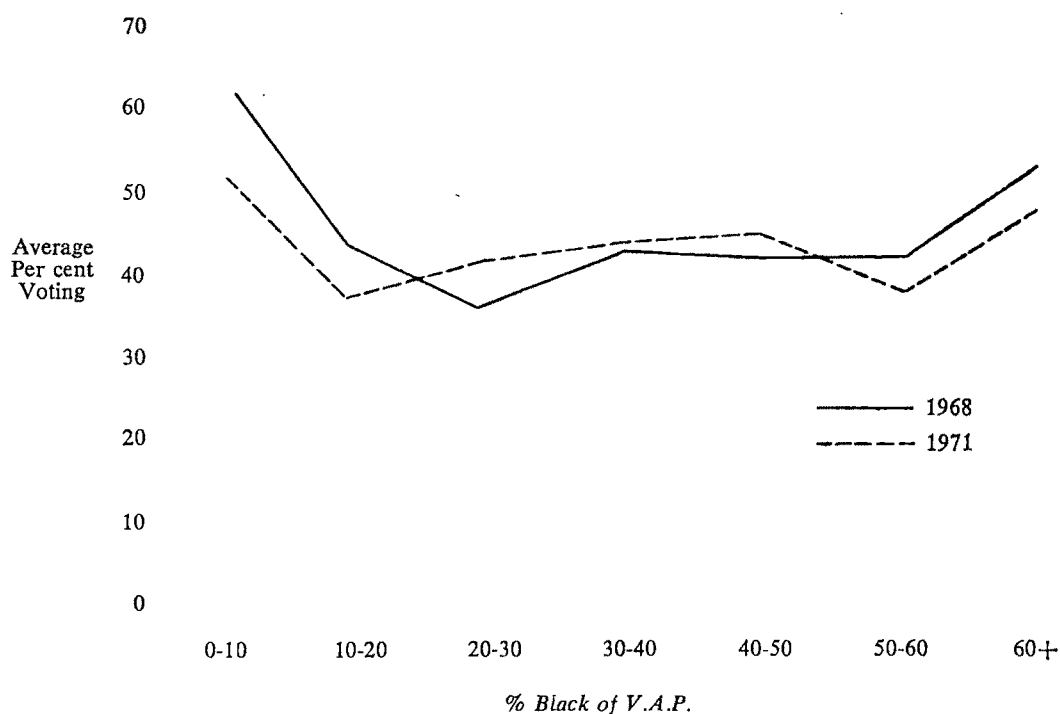


Figure 1. The relative size of the Black electorate is related in a curvilinear and nonmonotonic manner with Black voting turnout.

— .56 for 1968 and with only two cases in the lowest population range in 1971 a positive .09. Among counties in the intermediate range of 30 to 44 per cent black,¹³ increases in the percentage V.A.P. Black appear to produce little change in voting turnout with correlations at .00 and — .06 for the 1968 and 1971 elections, respectively.

Thus far, a reason has been offered for the upturn in participation at the 60 per cent level of Black V.A.P., but none has been suggested for the inverse relationship in 1968 at the lower ranges. In some respects these counties display a pattern similar to that reported by earlier studies in which black registration throughout the Deep South decreased as the black propor-

tion of the county's population increased. Donald Matthews and James Prothro in their definitive study on black participation prior to the civil rights movement find that the rate of decline in percentage registered accelerated as the percentage black of the county's population approached majority status, presumably reflecting stricter social control on the part of the white community as black enfranchisement threatened white dominance in local politics.¹⁴ In contrast, Figure 1 depicts a decelerating rate of decline which levels off in the 20–40 per cent range. Thus, whether the decline in turnout for 1968 reflects decreasing white tolerance or some other phenomenon is questionable. It may be that threat perception among whites begins at a lower threshold in Mississippi than in the other southern states. We know that before the civil rights movement, black registration in Mississippi was by far the lowest in the South, hovering around the three per cent mark.¹⁵ Perhaps, because of the increasing social and legal constraints on some forms of intimidation and the unavailability of various methods of disenfranchisement common before the civil rights

¹³ Forty-five rather than 50 per cent is used as the cut off for several reasons: (1) Because of differential outward migration some of these counties which in 1970 had less than a black voting age majority, may well have had a black majority in 1958. (2) In 1968 with the most recent census nine years old ambiguity probably existed as to the actual size of the potential black electorate and thus the same incentives to organize politically may have operated. (3) Since a 50 per cent cutoff produces only minor and inconsequential changes in the slopes given in Figure 1, and the 45 per cent provides a more desirable distribution of cases, the lower percentage figure is used here and in subsequent figures.

¹⁴ Matthews and Prothro, *Negroes and New Southern Politics*, Figure 5–1, p. 116.

¹⁵ *Ibid.*, p. 151.

movement, black participation on the average cannot be lowered much below the forty per cent figure. Thus, participation drops sharply to this plateau and then flattens out. Also to be considered is the likelihood that as the proportion black increases, black voters will constitute voting majorities for certain subcounty offices. Because of highly segregated residential patterns there are many "beats" which contain black majorities even though the county as a whole has a white electoral majority. Black candidates have since 1968 done fairly well in elections for such subcounty offices as constable and justice of the peace. Perhaps this electoral situation which naturally occurs with greater frequency as the proportion black increases operates to increase voting and offsets to some extent pressures against participation. Admittedly this argument is highly speculative and *ad hoc*, but it does serve to identify additional variables that may be important to participation but cannot be easily tapped in a study of this nature.¹⁶

In sum, what is being suggested is that the relationship between participation and percentage black may be a modified version of the relationship found before the civil rights movement. As a result of federal laws and voting rights activity, the potential black electorate in heavily black counties has been liberated and has become an incentive and a resource for political organization. In the other counties where blacks do not constitute large voting majorities, the effects of the voting rights activities have been less pronounced, and the more traditional pattern of a negative relationship emerges, although even among these counties there has been some impact with mean turnout registering above the 30 per cent figure. By this reasoning, *the effects of the civil rights movement have been greatest in counties where the black citizenry constitutes a potentially large electoral majority*. To this point the argument has been somewhat conjectural, tailored to fit the curves in Figure 1. We can do more, however, than merely debate its merits; there exists other evidence which can be brought to bear on its adequacy.

Some Mississippi counties experienced

¹⁶ There is some chance that this early negative slope may to some degree be an artifact of the composition of the counties' populations. In counties with very small black populations an even small white defection may greatly affect the percentage of black turnout, since the number of whites who defect may represent a relatively large percentage of the potential black electorate. Thus, the mean turnout percentage for the counties at the extreme left end of Figure 1 may be less accurate than for counties with larger black populations.

greater civil rights activity than others. For example, prior to passage of the major voting rights legislation some counties witnessed extensive civil rights campaigns from outside organizations such as, the NAACP, the Voter Education Project, and in 1964 the umbrella organization COFO, while others remained apparently untouched.¹⁷ And subsequently, although all Mississippi counties were required by the Voting Rights Act of 1965 to end *de jure* disenfranchisement, by 1968 less than half received federal registrars and overseers.¹⁸ In comparing counties which experienced greater and lesser degrees of civil rights activity, the explanation presented above makes several important predictions: (1) Counties with potential black majorities which experienced the civil rights movement should display a substantially larger turnout than the overall average. (2) Counties in which this movement was largely *absent* should follow the pre-civil rights pattern of a negative relationship, with the turnout being *lowest* among counties with a large percentage of the V.A.P. Black. (3) Therefore, exposure to voting rights activity should produce its largest differences (effects) among counties with potential electoral majorities.

Without in depth, county-by-county information it is impossible fully to assess the degree to which the civil rights movement "entered" and affected a given county. If we assume, however, that the presence of federal registrars in a county represents an adequate surrogate measure for more comprehensive but unavailable data on civil rights activity we can further test the conclusion made above that the voting rights activity had its greatest effects in heavily black populated counties.¹⁹

¹⁷ Pat Watters and Reese Cleghorn, *Climbing Jacob's Ladder* (New York: Harcourt, Brace and World, 1967).

¹⁸ The counties which had received federal examiners by late 1967 are identified in *Political Participation* compiled by the United States Commission on Civil Rights (Washington: G.P.O., 1968), Table 9, pp. 244-247.

¹⁹ There is good reason to believe that the presence of registrars is an important ingredient in black political mobilization. For example, by 1968, in Mississippi counties with federal examiners present, 71 per cent of the black eligible voters were registered as compared to only 50 per cent registered in non-examiner counties. Harrell R. Rodgers, Jr. and Charles S. Bullock, III, *Law and Social Change* (New York: McGraw-Hill Book Co., 1972), p. 32. Close observers of the civil rights movement in Mississippi have often asserted that the Child Development Group of Mississippi, the umbrella agency sponsoring Head Start projects in twenty-nine counties, created in its citizen advisory councils the core of political leadership in these counties. This variable was found to add little explanatory power to the effects of federal registrars, however.

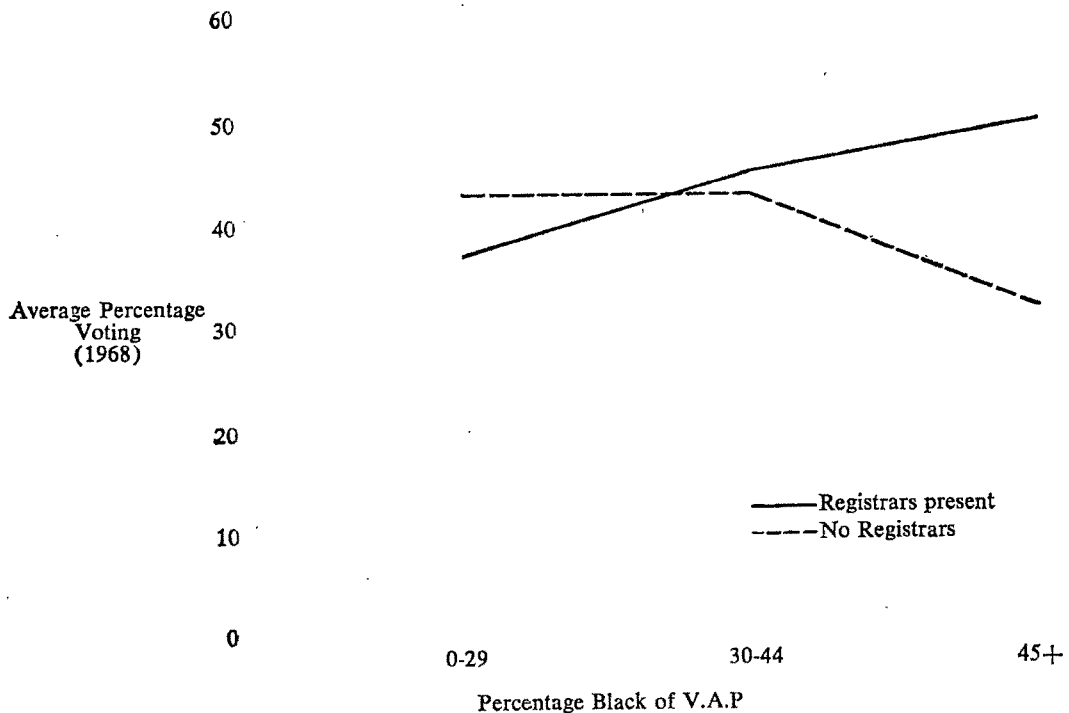


Figure 2. The Direction of the Relationship between Black Voting Participation and Per cent of the V.A.P. that is Black is Conditioned by the Presence or Absence of Federal Registrars.

In Figure 2 we can compare voter turnout in 1968 for counties with and without federal registrars. Just as predicted, voting turnout is highest for counties where the black citizenry constitutes a voting age majority and federal registrars were present. The second prediction is also confirmed; in counties less affected by the civil rights movement (i.e., no federal registrars) the relationship between percentage V.A.P. Black and voting closely corresponds with the pre-civil rights movement pattern reported by Matthews and Prothro among others. In these counties as the percentage V.A.P. Black increases, the per cent voting decreases, especially as the proportion black reaches a potential electoral majority. (It should be noted that although the pattern is similar the range of variation in turnout is much smaller.) Thus the differences between counties with and without federal registrars present are greatest among those counties where the black V.A.P. is in the majority. As speculated above, *one of the most important consequences of the civil rights movement appears to be the mobilization of black electoral majorities.*

From the viewpoint of assessing the civil rights movement in the Deep South, we can conclude that private and governmental efforts during the mid-1960s had their greatest impact

in communities where the return should be greatest whether measured in terms of sheer numbers of new voters or in the acquisition of political power. Of course, if only a small number of predominantly black counties were affected by the movement, then although the impact on them may be great, its overall significance for the extension of political equality would be minor. We can see from Table 4 that this is not the case. In fact, federal registrars went disproportionately into the predominantly black counties and as a result optimally concentrated their energies and resources. Moreover, indications are that during the early stages of the movement when the burden was carried primarily by private groups the focus was similarly

Table 4. Relation Between Presence of Federal Registrars and Percentage V.A.P. Black

Percentage V.A.P. Black in County		Registrars Present in County	
		Yes	No
Under 45		20	42
	45 +	11	9

N=82
r=.40 (based on continuous percentaging)

on the black majority counties.²⁰ *Judged by a cost-benefit perspective, the civil rights movement fares well; resources and attention were focused on areas which yielded the highest return.*

Why Black Candidates Fail to Win Elections

For those of us who value the expansion of voting rights the preceding paragraph reports good news. Yet although the civil rights movement appears to have effectuated greatest change in communities containing a sizable black electorate with some chance of electoral success, expectations of frequent black victories in county elections are not being fulfilled. Even when the black electorate contains a large majority the elections are usually close, and often the black candidate loses. Why is this so?

First, black voting certainly needs to expand further. Realizing that black voting early in the 1960s was virtually nonexistent, one finds it hard to gainsay the successes of the civil rights movement. Both in 1968 and 1971 forty-two per cent of the eligible black voters were voting, and according to the most conservative estimate about three-quarters of those registered voted in 1971.²¹ Mass black political participation is a reality; in 1971 more than 170,000 voted. Given the dire social and economic conditions of this subpopulation, it may be expecting too much to look for a substantially higher turnout in the near future. Many black citizens have yet to enter the system as participants, and until more do so the chances for victory in marginal counties are small.

A second important variable which affects the chances of a black victory is so obvious that it is generally ignored; it is the white vote. Perhaps white turnout is intuitively viewed as a constant and for this reason is overlooked. *White voter turnout, however, largely represents a response to the potential and actual black turnout.* White voting correlates with percentage V.A.P. Black .42 in 1968 and .46 in 1971, and it correlates with the black turnout .10 and .40 for the 1968 and 1971 elections respectively. The slopes of these relationships, which are plotted in Figure 3, represent the effect of the given independent variable on white turnout, the steeper the slope the greater the effect. It is interesting that in 1968 and 1971 the independent contribution of percentage V.A.P. Black is the same. This variable represents in a sense the potential threat of black voting to white control. As the relative size of the black electorate (percentage V.A.P. Black)

increases, so does white turnout. This strongly suggests an important reason why black candidates frequently lose elections they should win. In counties where black candidates have some chance of winning, white voters are going to the polls at a very high rate and often an otherwise decent black showing is insufficient.

The second variable, the actual percentage of blacks voting probably reflects the mobilization of black voters in registration drives and election campaigns and as an index of this mobilization correlates highly with white turnout. Its increased impact on white turnout (compare the regression slopes of Figure 3A with 3B and the partial coefficients in 3C) during the 1971 election may have been due to the presence of a black candidate running for the state's highest office. Charles Evers began his gubernatorial campaign as an attempt to create a coattail to assist black candidates running for county and state legislative offices. The possibility of actually winning was initially acknowledged as unlikely. As election day neared, however, the campaign changed. Evers began soliciting hitherto nonexistent white liberal support as well as the vote of newly enfranchised youth. Purchasing local television time, he began to appeal for support from a larger constituency which may have diverted attention from getting out the black vote for local candidates.²² When the vote was in, his candidacy was rewarded with only 23 per cent of the total votes cast and spot checks of a number of all white precincts show almost total opposition. The extent to which his campaigns simply increased the white turnout against him and other black candidates can only be guessed, but given the evidence that black voter mobilization has the side-effect of also increasing the white turnout, the adverse consequence may have been large. More generally, this finding suggests that black mobilization in Mississippi should proceed as quietly and inconspicuously as possible. Any effort to appeal for white support will probably just make the black candidacy more salient to whites and result in a net loss of votes.²³

Summary and Conclusion

The central thesis of Salomon and Van Evers's ambitious effort is that fear as measured

²² . . . *In the Public Interest*, pp. 4, 6.

²³ Fred Wirt in his important study of integration in Panola County, Mississippi, reports that this consideration is appreciated by candidates of both races. "A candidate active among the other race stood to gain very few votes but more likely would mobilize even more voters against him because of the adverse racial reaction." (*Politics of Southern Equality* [Chicago: Aldine, 1970], p. 161).

²⁰ Watters and Cleghorn.

²¹ . . . *In the Public Interest*, p. 3.

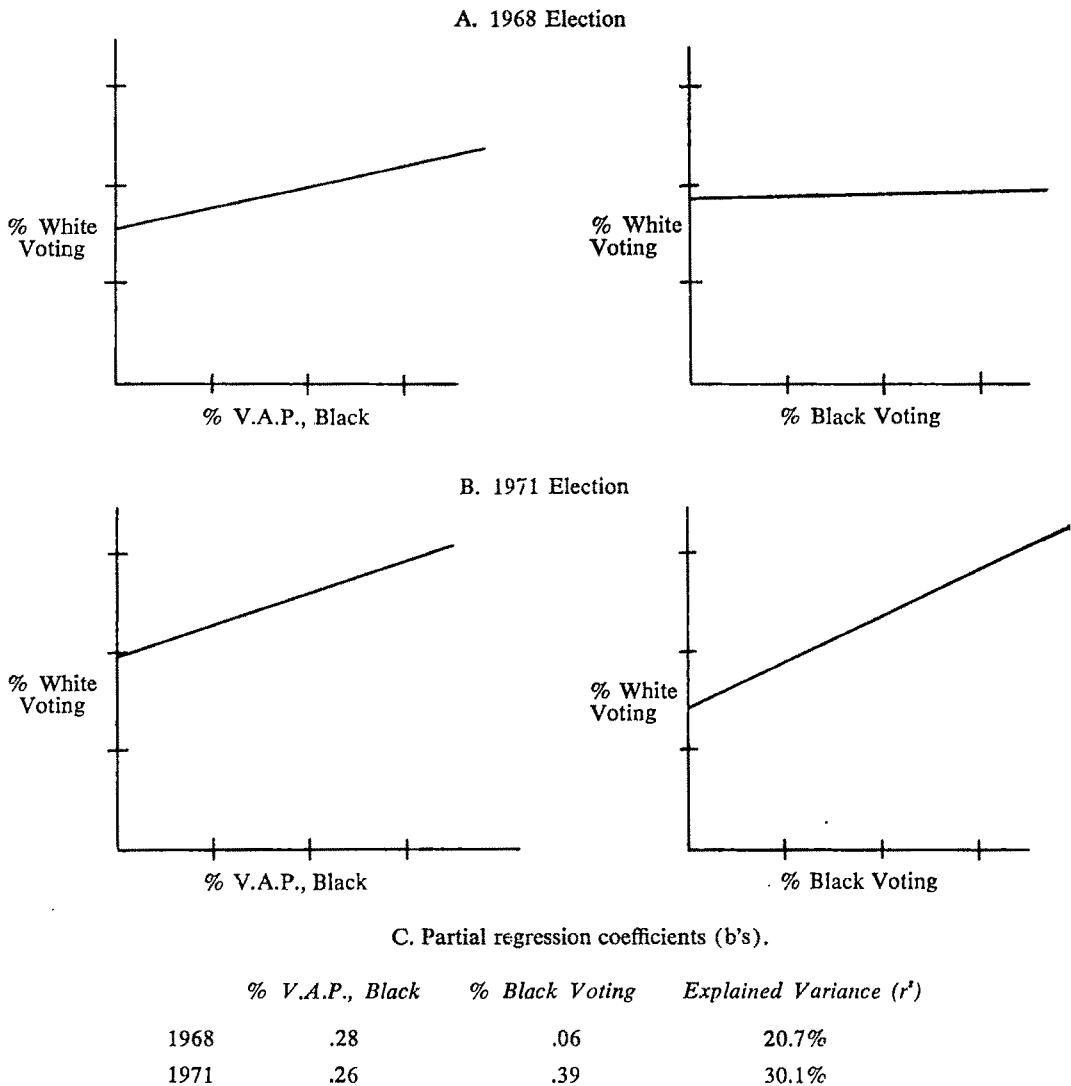


Figure 3. White Voter Turnout Reflects the Level of Black Voter Mobilization.

by occupational vulnerability is the primary explanation of black voting. On re-examination of this explanation using an expanded data base, fear as measured does not appear to have much independent importance in explaining county-to-county variations in black turnout. This is not to say that at the individual level there are no effects, but simply that the ecological data fail to suggest it. Education, on the other hand, is found in both studies to be a powerful and stable factor in black political participation. Whether education primarily reflects the level of vulnerability to intimidation and manipulation or instead represents the standard political interest and awareness concomitants so frequently found for other populations, can with

the evidence presented here only be argued. Surely individual cases of both fear and apathy resulting from poor education could be found. It is persuasive support for the standard "apathy" model, however, that the level of college education among the blacks in a county is almost as important as the county's level of black illiteracy in explaining black voting turnout.

Both studies have discovered that among heavily black populated counties, variations in the percentage black of the voting age population is an important factor in black turnout. This strong positive relationship was present for both the 1968 presidential and the 1971 gubernatorial elections. Salamon and Van Evera and I agree that this variable is probably indic-

ative of the level of organizational activity on the part of aspiring candidates as well as continuing political groups within a county, but whether these organizations operate primarily to displace "fear" or "apathy" remains in doubt. By expanding the analysis to include counties with smaller black populations we find that the percentage black of the electorate changes dramatically in its relationship with turnout. At the intermediate population range (30 to 44 per cent black of V.A.P.) there is virtually no relationship with voting turnout, and among counties with a comparatively small black population the relationship actually changes direction. Additional evidence suggests that the conversion from a strongly negative to a strongly positive relationship among counties with potential black voting majorities may be one of the chief successes of the voting rights campaigns and federal voting laws of the 1960s.

Winning elections and sharing political power is not simply the result of getting out the black vote. First, in counties with a potentially large black electorate, white turnout normally is high. Second, as black organizations mobilize their own constituency with registration drives and election campaigns, they unintentionally mobilize the white vote in opposition. Consequently, as their turnout increases, the threshold vote necessary for winning is also raised.

Salamon and Van Evera and I agree that black voting in Mississippi is in some sense subject to peculiar influences. Despite our apparent disagreement over the relative merits of certain explanations of black voting, we concur in paying close attention to contextual factors not commonly found in voting research, the most important being the size of the potential black electorate. Moreover, the study of black political participation in the Deep South produces a unique confluence of several fields of inquiry in political science. Research and theory in such diverse areas as voting behavior, law and social change, and political development can all inform and be informed by the study of black voting in this region. Thus, research into black political participation represents an opportunity not only to describe and understand the process of mass enfranchisement of the last major population group in the country, but also may provide in its uniqueness a rich field for testing the breadth and adequacy of theories of political participation and political development.

Appendix A. Index Construction

All of the social and economic data have been taken from three sources: *General Popu-*

lation Characteristics: Mississippi (no. 26, 1971); *General Social and Economic Characteristics: Mississippi* (no. 26, 1972), 1969 *Census of Agriculture: Vol. 1 Area Reports* (section 2, County Data, part 33: Mississippi).

Occupational Vulnerability. This index of economic dependency and vulnerability to white economic coercion closely follows Salamon and Van Evera's index of "most vulnerable" occupations given in Table 2 of their paper. This index is composed of the percentage of the county's black labor force (age 16 and over) which is either unemployed or employed as farm tenants, household workers, farm laborers and foremen; although unmentioned in their Table 2 it must be assumed that foremen were included in their count since the 1960 census also lists laborers and foremen under a single heading.²⁴

A revised index was constructed by adding to the above occupations all black salaried agricultural workers and local governmental employees (including school teachers). Comprising a somewhat larger proportion of the black labor force, this new measure correlates with the original index at .91. Since the two measures are so highly correlated, the revised occupational vulnerability will be dropped from the analysis, and only the measure employed by Salamon and Van Evera will be retained.

Occupational Invulnerability, Revised. It was not possible nor desirable to duplicate completely the authors' "least vulnerable" category. The number of workers employed outside their county of residence—included in their index—is not indicated in the 1970 census reports. Also, in the 1960 census, as the authors acknowledge, the "professionals" category included highly vulnerable school teachers. Comprising the "least vulnerable" or "invulnerable" occupations are farm owners, non-local government workers, nonagriculture self-employed, mining, and manufacturing.

Median Education. Unfortunately the census reports median education for the sexes separately. After an inspection of statistical relationships between median education for each sex and the dependent variables confirmed that

²⁴ Salamon and Van Evera note that schoolteachers are highly vulnerable, but because the 1960 census included teachers in the category of "professionals" they could not be included among the highly vulnerable occupations. The 1970 census which lists teachers separately permits their inclusion among the highly vulnerable. The presence of this relatively small group, however, had almost no effect on the relationships.

% Vulnerable	(1)		-.59	-.74	-.41	.16
% Invulnerable	(2)	-.43		.73	.25	-.33
Median School	(3)	-.73	.23		.50	-.25
Median Family Inc.	(4)	-.55	.02	.76		-.37
% VAP(21+) Black	(5)	.51	-.24	-.66	-.61	
		(1)	(2)	(3)	(4)	(5)

Table 5. Intercorrelations Among Selected Independent Variables.

(Correlations to the left of the diagonal are for all counties ($N = 82$); the coefficients to the right of the diagonal are for the 29 counties in the Salamon-Van Evera paper.)

male and female median education have virtually identical "effects" on turnout, they were combined (averaged) to create a single, although slightly imperfect, median education index.

Percentage of the Voting Age Population that is Black (% V.A.P. Black). For the 1968 election the voting age population is based on the percentage aged 21 and over. Several weeks before the registration deadline for the 1971 general election, however, voting rights were extended to 18- through 20 year olds. Although few of the newly eligible voters managed to register in time, they are included in the calculation.²⁵ Using the 21-year-old voting age population index instead produced minor changes in the correlations with the 1971 turnout.

Black Voter Turnout. For 1968 this variable was formed by dividing the total Humphrey vote in a county by the black voting age population. For 1971 the Evers vote is employed, and the eligible electorate included ages 18-20.

Appendix B. Validity of the Humphrey Vote as a Measure of Black Turnout

The Salamon and Van Evera paper reports an extraordinarily high correlation ($r = .92$)

²⁵ . . . *In the Public Interest* (Millsaps College: Institute of Politics in Mississippi), Vol. 1 (August, 1971), 1-14.

between the Humphrey vote in 1968 and the vote for local black candidates. Averaging the two measures is redundant, and as the authors note using either index alone yields the same results (see footnote 21). This indicates that the Humphrey vote alone suffices as a valid index of black voting. Of course, the same caveats issued by the authors hold here. In a few instances, to be sure, black voters voted against Humphrey and against the local black candidate either from mistake, fear, or choice, but as a measure of county-to-county variations in black voting the Humphrey vote remains satisfactory.

Some readers may question the validity of this measure for the other fifty-three counties with a smaller black population. Perhaps in these counties there is less polarization between the races and consequently more crossover voting. Intuitively this seems unlikely. More plausible is the hypothesis that the same dynamics of voting preferences are operating equally in all counties. Moreover, the evidence supports the latter viewpoint. Below are correlated county figures for the Humphrey and Evers vote (in 1971). The Humphrey vote correlates closely with the vote for the black gubernatorial candidate three years later.

Correlations between:

Total for Humphrey and Total of Evers, $r = .98$.

% of Total for Humphrey and % of Total for Evers, $r = .94$.

Fear Revisited: Rejoinder to "Comment" by Sam Kernell

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In his interesting comment on our article, Sam Kernell raises several searching objections to our analysis that throw some doubt on our central conclusion about the role of fear in black participation in the South; but then proceeds to demonstrate the usefulness of our analysis by extending its application over both time and space and confirming several of our hypotheses in the process. While we are somewhat jealous that Mr. Kernell did not have the same opportunity we had to have his results rendered slightly out of date by this *Review's* lengthy publication delays, we are nevertheless grateful to him for the support his research provides to several of our earlier findings and for the opportunity it affords us to revisit our hypotheses in the light of more recent elections and Census data. Despite the thoughtfulness of his criticisms, however, we believe that many of his objections to our analysis are misdirected and that his effort to replicate our analysis is seriously incomplete and hence misleading. To demonstrate this, we propose to consider each of his objections to our original article in turn, specify why his purported replication of our analysis falls short of a true replication, and finally, present the results of a true re-testing of our earlier hypotheses against more recent data. What we discover from this three-pronged effort is that our earlier conclusion—that fear has been a major determinant of black political participation in the Mississippi "black belt"—survives both the scrutiny Mr. Kernell ably gives it and the updating to which we subject it ourselves. In the process, moreover, we are able to demonstrate the apparent utility of the models we developed earlier in measuring an important dimension of political change, a dimension we will refer to as the transition from "fear-based nonparticipation" to "apathy-based nonparticipation," and that we believe could be generally useful in examining the "opening" of political systems to real popular participation in the course of political modernization.

Evaluation of Objections

Kernell raises seven major objections to the analysis presented in our article. While each of

these objections has some merit, each also has several shortcomings and must therefore be scrutinized with care. Kernell's first objection, for example, is that it is difficult to choose among our competing "fear" and "apathy" explanations of political participation because they "make similar predictions" (p. 1307). In fact, however, as Table 10 in our article indicates, the "fear model" accounts for 69 per cent of the variance in our dependent variable, black turnout, while the "apathy model" accounts for only 23 per cent. How these can be construed as making "similar predictions" is difficult to understand.

Kernell's second objection, also offered in his opening paragraph, is a contention that hypothesis-testing using aggregate analysis is "nearly impossible." Not only does this fly in the face of the recent methodological discussions defending the use of ecological techniques cited in our article (footnote 15), but also it raises the question of why Kernell then proceeded to use aggregate analysis himself. We are aware of the shortcomings of ecological research and took pains both to warn our readers and to minimize the dangers. In addition, we underlined the dangers that survey research also embodies for the purpose we had in mind. More recently, we have examined what survey data is available to determine whether the relationships we discovered at the aggregate level can be verified as well at the individual level. As we will illustrate below, the results are supportive.

A third objection to our analysis raised by Kernell deals with the *discreteness* of our fear model. In particular, Kernell questions the inclusion of our organization variables in the fear model on grounds that organization may affect black voter participation through some mechanism other than fear. Which explanation of the dynamics really at work behind a set of numbers can never be determined from the numbers themselves, of course. We therefore agree with Kernell that readers should look beyond the numbers into the dynamics of the social situation to interpret the true meaning of any empirical indicator such as our organization vari-

ables; and we took pains in the body of the article to emphasize this very point and to explain our interpretation of this set of variables. Despite Kernell's hypothetical objections, we still find this explanation convincing. The important point to re-emphasize here is that our decision to include this set of variables in our fear model was not based on mere conjecture or *post-hoc* rationalization. To the contrary, it was based on extensive interviews with numerous civil rights activists in the South, on a review of written accounts of civil rights organizing in the Deep South (see footnotes 5 and 34 in our text), and on a detailed analysis of black organizational development in three Mississippi counties performed by one of the present authors (LMS).¹ To help guarantee we were tapping the fear-related aspects of organization, moreover, we chose empirical indices that promised to focus in on this particular dimension. This was particularly true of our "outside organizer" and "per cent of VAP, black" variables. The whole logic behind interpreting the "per cent VAP, black" as an organizational variable is that the risks associated with voting become bearable only as the prospects of winning become better. We are gratified, therefore, that Mr. Kernell himself ultimately concludes that his own finding "fits neatly into Salamon and Van Evera's discussion of this variable [the black proportion of the voting age population] as an organizational resource" (p. 1311). In addition, we find the alternative argument about the role of organization—an argument that rests on the analogy between black-belt civil rights organizations and big-city political machines—unconvincing, if only because the civil rights organizations have had no access to the social services and patronage that constituted the real basis of the urban machines' mobilizing ability. To attribute the same dynamic as the "machine" to a set of embryonic political organizations that completely lacked the major resources available to the machine—services and patronage—is, we think, inappropriate. For all of these reasons, we believe that our interpretation of the meaning of civil rights organization in the southern black-belt is valid and our decision to include this set of variables in the fear model consequently appropriate.

Kernell's fourth complaint about our "expanded fear model" is that it is "too versatile." In particular, he suggests that "with respect to the important variable, per cent of the county's

voting age population that is black," fear can account not only for the positive relationship with black participation we found but also for negative relationships because, as earlier writers have suggested, white hostility increases as the real potential of black power increases, and hence blacks should participate less as the black proportion of the population rises. However, Kernell's observation here conveniently overlooks two crucial parts of our analysis that were aimed explicitly and precisely at this point, and that we believe kept us well out of the pitfall he claims we succumbed to. First, we examined *only* the black majority counties largely to eliminate the effect on black participation of variations in white hostility (which is really what Key and Matthews and Prothro were measuring with their per cent black variable, as Kernell concedes). By focusing only on counties where the potential of black electoral victory was real and where white hostility was consequently relatively high, we deliberately avoided the charge of "versatility" Kernell nevertheless levels at us by holding constant the white hostility impacts of variations in black population proportions, and measuring only the impact that variations in the black proportion of the population above this minimum had on black organizational potential. No content with this safeguard, however, we employed a second one to determine whether choosing counties in this fashion really produced the control we intended. In particular, we formulated an explicit set of white hostility measures and tested them against the participation data to guarantee that the results we were attributing to variations in black vulnerability were not really attributable to variations in white hostility. The results, reported in Table 5 of our article, supported our assumption. Yet Kernell overlooks this analysis. In view of this, his statement that our model "permits *post hoc* confirmation by opposite findings" seems incorrect to us.

A fifth objection raised to our analysis by Mr. Kernell is that our findings are not accurate because, as Mr. Kernell puts it, "the dependent variable . . . is measured during the 1968 presidential election, while the data for the independent variables are obtained from the 1960 census, some nine years earlier." Although Mr. Kernell is certainly correct in noting that we did not use the 1970 census (because it was not available until two years after we completed our analysis and 18 months after we submitted our article to the *APSR*), we disagree strongly with his conclusion that this renders our results inaccurate—for several

¹ This analysis will be published shortly by Indiana University Press as part of Lester M. Salamon's book-length study of social and political change in Mississippi and the American South.

important reasons. First, contrary to what Mr. Kernell claims, our independent and dependent variables are not all separated by nine years. For example, the basic index computed from local elections held largely in 1967, and the 1968 presidential election was used primarily as a check on the local election figures. As for the independent variables, they were certainly not all "obtained from the 1960 census," as Kernell claims. For example, the "organizer index," one of the four components of our "expanded fear model," was obtained from interview sources in 1969 and covered the entire period of the early and mid-1960s. More importantly, the very independent variables of greatest concern to Kernell, those relating to agriculture, were not taken from the 1960 Census at all, but from the Agricultural Census of 1964. What makes these observations especially important, moreover, is the fact that the dramatic social and economic changes that Kernell claims might invalidate our results did not occur at a uniform rate throughout the 1960s. To the contrary, the really massive agricultural dislocations cited by Kernell were concentrated in the latter part of the decade, after the extension of the federal minimum wage law to agricultural laborers in 1966 and the switch from the commodity distribution relief program to the food stamp program in 1967.² At the time of the 1967 local elections, and even the 1968 presidential elections, therefore, these changes were still very much in progress. Since social and economic changes of the sort that occurred in Mississippi beginning in 1966 do not take place overnight, we believe the Census of Agriculture figures for 1964, the Census of Population figures for 1960 and our organizer index for 1961-1968 come closer to depicting reality at the time of the 1967 local elections and the 1968 presidential election than do the comparable 1970 figures; for by 1970 the changes that were only beginning to be felt massively in 1967 were already far more complete. Interestingly, moreover, Kernell's own findings, reported in his Table 2, lend credence to this view, for almost every one of his 1970 independent variables does a better job of accounting for the 1971 black turnout (as measured by the

Evers vote) than it does in accounting for the 1968 black turnout (as measured by the Humphrey vote).

A final problem with this objection Kernell raises about the accuracy of our analysis is that it overlooks the most important aspect of the dislocations that occurred between 1960 and 1970 and hence implies a far greater change in results from using the 1970 figures instead of the earlier ones than is in fact the case. In particular, Kernell focuses his attention exclusively on *absolute changes within counties*. What is most relevant for correlation and multiple regression analysis, however, are *relative changes among counties*. Only if the changes that occurred affected counties differently will they influence the results. However, Kernell neglected to compute the correlations between the 1960 or 1964 values of the key variables and the 1970 values. Table 1 illustrates the results of such a computation and suggests the inadequacy of Kernell's focus on absolute instead of relative changes. For example, while Kernell, in the absence of systematic examination, argues that "vulnerability as measured in 1960 compared with 1970 represents essentially a different variable" (p. 1310), the correlation between these two for the 29 black majority counties we examined turns out to be a high +.772. In fact, except for the median family income measure (the weakest of the variables), the 1960/64 values of all the key variables are *at least* this closely related to their 1970 values. By focusing on gross, absolute changes and ignoring the persistence of more stable variations among counties, Kernell thus further weakens his contention that our analysis is inaccurate. Despite Kernell's criticism, therefore, we feel justified in believing that our original independent variables are not only valid, but preferable, for examining the 1967 and 1968 elections. Moreover, the generally high correlations between the 1960/64 values of these variables and the 1969/70 values leads us to suspect that the same analytical model that explained the 1967/68 results will continue to

Table 1. Correlations Between 1960/64 Values of Key Variables and 1970 Values

Variable	Correlation Coefficient
% Vulnerable	.772
N. W. Voting Age Population	.992
N. W. Median Education	.905
N. W. Median Income	.488
% Voting Age Population, Black	.880

² The extension of the minimum wage law gave planters in the black-belt counties a powerful incentive to shift to mechanized agriculture and dismiss black field laborers, while the switch to food stamps completed the social squeeze process by requiring cash payments of relief recipients who had formerly been able to stay alive during the workless winter months by securing free commodities. For a discussion of the consequences during 1967 and 1968, see: Tony Dunbar, *Our Land Too* (New York: Random House, 1972).

have substantial, if somewhat diminished, explanatory power when redefined using 1970 data and applied to the 1971 elections. The reanalysis to be presented below generally confirms this suspicion and, in the process, suggests an interesting new way to measure an important aspect of "political development."

Kernell's sixth objection to our analysis is a challenge to the *generalizability* of our findings, particularly their generalizability to white majority counties in the South. We readily concede this point. We made no claim that our results would be appropriate for "explaining participation in other counties with smaller proportions of black population" (Kernell, p. 1308). Indeed, our decision to focus on the black majority counties reflected this very supposition. Our theory was that interpersonal coercion would tend to affect participation most significantly only where the stakes in an election are high, *and* the outcome of the election is in doubt and could be reversed by coercion. While the first of these conditions holds in virtually all Mississippi counties, the second applies only to those with black voting age population majorities or near majorities. By extending our analysis to all Mississippi counties, therefore, Mr. Kernell has importantly complemented our research by demonstrating what we would have predicted: that our findings about the role of fear do *not* seem generalizable to the white-majority counties in Mississippi. However, we do not agree with Mr. Kernell that this means our findings are not generalizable at all. For while our explanation of participation patterns in the Mississippi black-belt may not apply to the white majority areas of Mississippi, we believe it may apply quite well to other parts of the world—to those places where social conflict is intense, where electoral institutions are called upon to handle this conflict, where the outcome of elections is in doubt, and where those in power have the means physically or economically to coerce those who are seeking power. Several portions of the so-called "developing world" probably meet these specifications. Indeed, as we suggested in our original article, the presence of a "fear-based pattern of participation" may be viewed as an important defining characteristic of an "underdeveloped" or "premodern" polity. To the extent this is true, our analysis could provide a fruitful basis for cross-national empirical research that might yield quite interesting generalizations about political participation patterns in developmental settings. Our analysis, in short, may be far more generalizable than Kernell contemplates.

Kernell's final major objection to our analysis deals with our effort to interpret the social reality denoted by the relatively high correlation between black participation rates and black education levels. We have no quarrel with Kernell's major point that "with the data available it is impossible to determine whether illiteracy primarily signifies fear or apathy." Indeed, we took pains in the article to emphasize this very point. What we were eager to suggest, however, was that the conventional explanation of the meaning of education in terms of apathy was not the *only* explanation possible; that an explanation that interpreted education in terms of fear was at least as plausible, and possibly more so in the peculiar circumstances of the southern black-belt. In questioning this suggestion, however, we believe Mr. Kernell inaccurately characterized the chain of reasoning that led us to this alternative explanation. In his view, our interpretation grew directly out of "a cursory analysis of the correlations of various levels of education with turnout," as if a set of numbers were sufficient to generate a theory. In fact, our chain of reasoning was the exact reverse of what Mr. Kernell suggests: we developed our alternative explanation on the basis of what we have learned about the dynamics at work in this society and only then turned to the data to determine what empirical credence it gave to our theory. Finding some, we cited it for what it was worth, warning readers as we went that "a correlation . . . is simply a measure of relationship and cannot by itself 'explain' the relationship." (p. 1303). Readers must choose for themselves which interpretation of the education variable seems most plausible. For our part, we continue to believe that a large portion of the relationship between education and turnout in the southern black-belt can be attributed to education's relationship to fear. Even so, we conceded the point to adherents of the conventional interpretation and took the conservative course of excluding the education variable from our expanded fear model. In retrospect, and despite Kernell's objection, we believe a strong case could be made for reversing this decision and incorporating education into our fear model.

Replication Problems

Against the background of his objections to our analysis—objections we generally find unconvincing for the reasons just cited—Mr. Kernell proceeded to undertake what he terms a replication of our analysis using more recent data for the independent variables and extending the analysis in time and space to embrace

the 1971 elections and all Mississippi counties. In large part, Kernell's findings mirror our own. Yet they also partially challenge the conclusion we reach about the importance of fear in explaining black turnout. In evaluating this challenge, however, it is important to understand certain crucial respects in which Mr. Kernell's analysis differs from ours.

Although we naturally concur that 1970 figures are appropriate for analyzing the 1971 election, we have already explained why we think our own earlier figures are preferable in analyzing the 1967-68 elections. Even more important than this difference in data base, however, are additional disparities that differentiate his approach from ours even when we use the same 1970 data base and focus on the same 1971 election in the same 29 counties. In a word, Mr. Kernell offers only a partial replication of our original analysis, yet neglects to note this partiality. In the first place, he completely ignores one of our three models, the "discrimination model." In its place he substitutes what he calls the "political organization model," which is really a fragment of our "expanded fear model." Even here, however, Mr. Kernell is not true to our original analysis, for two of the three organizational variables included in our "expanded fear model"—the black voting age population and our organizer index—mysteriously disappear from his analysis completely, without so much as a mention. The conclusion Kernell draws from his Table 2 that the fear and apathy models are "equally powerful" in explaining black turnout rates thus seems somewhat misleading, since the "apathy model" as evaluated in this table still contains both its income and education components, whereas two of the original four variables in

the "fear model" have been dropped from the analysis altogether and a third broken off and set up as an autonomous explanation. For these and other reasons, therefore, we feel that Mr. Kernell's purported replication fails to do justice to our original analysis.³

Fear and Apathy Revisited—Measuring Political Change

To correct the shortcomings in Mr. Kernell's purported replication of our original analysis

³In addition to the differences already cited, Mr. Kernell proceeds differently in defining the vulnerability index. While we used the black voting age population as the denominator in computing this index, Kernell uses the black "labor force," which subjects his index to the vagaries inherent in the Census definition of the labor force and different cultural patterns among households. The labor force, after all, includes not all adults, but only those employed or "actively seeking work."

Another departure in Kernell's replication is the use of the eighteen-year-old and over population as the voting age population for computations in connection with the 1971 election, even though the voting age was lowered only a few months prior to the election. Since the younger generation is likely to be generally less responsive to coercion than the older one, the inclusion of this younger element in the computations somewhat prematurely probably understates the influence of the fear-related variables.

Finally, although it is not as central to our own analysis, Kernell's decision to use the Humphrey vote and the Evers vote to measure black turnout in the white majority counties strikes us as somewhat dubious, despite the high correlation between these two votes. In the absence of alternative evidence, the suspicion remains that in counties where the black population is small, local whites might feel at ease enough to vote their economic self-interest and support an economic liberal. Otherwise we would have to believe, for example, that an astounding 76 per cent of the 1,199 voting age blacks in Harrison County trooped to the polls to vote for Hubert Humphrey in 1968. Certainly a substantial share of these votes must have been cast by whites.

Table 2. Regression Equations for "Expanded Fear" and "Apathy" Models of Black Voter Turnout, 1967/8 vs. 1971, Twenty-Nine Mississippi Counties

Model	Variables	Beta (Standardized Regression Coefficients)		% of Variance Explained (R ²) Cumulative	
		1967/8	1971	1967/8	1971
"Expanded Fear"	% Vulnerable	-.59	-.42	28.9	13.9
	N. W. Voting Age Population	-.37	-.51	29.5	25.7
	% VAP, Black	.26	.34	46.5	44.8
	Organizer Index	.46	.28	68.6	49.6
"Apathy"	SES Index	.71	N.A.	7.3	N.A.
	Median Education	-.21	.76	23.3	33.4
	Median Income	-.32	-.35	23.4	42.6

and hence assess more accurately the utility of our original models in accounting for the variations in black turnout during the more recent 1971 elections, we reproduced the original analysis using 1970 values for our independent variables and 1971 values for the dependent variable.⁴ As in our original analysis, we restricted our attention to the same 29 substantially black counties in Mississippi on the grounds, outlined above, that blacks in these counties experience pressures significantly different from those felt by blacks in the substantially white counties. In addition, since we continue to have faith in the validity of the independent variables in our original analysis for examining the 1967 and 1968 elections, focusing on the same counties permitted us to measure empirically any change in participation patterns that might have occurred over time. To the extent that fear declines as an explanation of black turnout between 1967 and 1971, for example, we can speak of an "opening" of the political system, signified by the displacement of "fear-based nonparticipation" by "apathy-based nonparticipation."

The results of the up-dating of our original analysis, reported in Table 2, are thus doubly interesting. In the first place, they indicate that in 1971, as in 1967-68, the "expanded fear model" continues to explain more of the variance in black turnout rates than the "apathy model."⁵ In the second place, however, they suggest that the relative power of the fear model has declined over time: instead of the

3:1 advantage over the "apathy model" in terms of variance explained that it enjoyed in the latter 1960s, the fear model holds a far slimmer 11:9 advantage by the early 1970s. Apparently some important changes have occurred in Mississippi political life since the first half decade following the Voting Rights Act of 1965. Perhaps the most important feature of these findings for analytical purposes, however, is that our indices permit us to measure these changes empirically. In the process, they point the way to further research into an important dimension of political change, not only in the black-belt South but also in other regions of the world where comparable situations exist. Indeed, the transition from what we have termed "fear-based nonparticipation" to "apathy-based nonparticipation" might serve as a fruitful new way to assess the pace of "political development" in so-called developing regions.

The conclusion that emerges from our aggregate analysis of Mississippi black-belt counties to the effect that fear of coercion continues to influence turnout patterns substantially finds additional support, moreover, in individual level data as well. Prodded by Mr. Kernell, and by our own suspicions about aggregate data, we rechecked the relationship between economic coercion and black voting using survey data from the 1968 and 1970 Michigan Survey Research Center opinion surveys. Unfortunately, the Survey Research data is not wholly satisfactory, since SRC polled only 200 Southern blacks whose voting and occupational status could be determined and only 117 of these lived in counties with populations over 30 per cent black. Keeping in mind the inherent limitations of so small a sample and the special limitations of survey research in tapping the political attitudes and behavior of southern blacks, some interesting results nevertheless appear when we divide the SRC sample into vulnerable and invulnerable groups as we did for our Mississippi study. In particular, the turnout rate of the invulnerable group in the 1968 and 1970 elections was significantly higher than that of the vulnerable group, as our "fear model" would predict. (See Table 3.) What is more, the spread between the two groups is considerably greater in counties with substantial (over 30 per cent) black concentrations than in counties without such concentration, also as our fear model would predict. Finally, and perhaps most interestingly, when we applied the same occupational classifications to deep-southern whites in the SRC survey to determine whether we might actually be observing the effects of occupational status or some other effect of occupation not linked to coercion and therefore applicable

⁴ The data for the independent variables came from *U.S. Census of Population, 1970, General Social and Economic Characteristics: Mississippi, No. 26*; and the *U.S. Census of Agriculture, 1969, Vol. 1, Area Reports, Part 33, Mississippi*. In addition, we used the "organizer scores" collected earlier in connection with the original research for our article.

Unfortunately, the Socio-Economic Status (SES) index used to measure occupational status in our original "apathy" model has not yet been computed for 1970 and had to be dropped from the analysis. Given the high correlation between the SES index and education, however, the effect of this is probably slight.

We were also unable to include in our vulnerability index the workers employed outside their county of residence, since figures on this variable were not reported in the 1970 Census.

Except for these alterations, the analysis presented here mirrors that reported in our original article.

⁵ When these models are computed using the twenty-one-year-old and over—instead of the eighteen-year-old and over—population as the base, the "spread" between these two models increases, with the fear model accounting for 55 per cent of the variance and the apathy model for 45 per cent. Since the voting age was lowered to 18 only a few months before the 1971 election, there is reason to believe the twenty-one and over population is the more accurate one to use. We have nevertheless reported the results using the eighteen and over population in our Table 2 in order to conform to Mr. Kernell's methodology.

Table 3. Voter Turnout as a Function of Economic Vulnerability Among a Sample of Southern Black Interviewees

	% Voting, 1968 and 1970		
	Entire South N=200	Heavily Black Counties N=117	Heavily White Counties N=72
Invulnerable	68% (N=44)	69% (N=26)	67% (N=18)
Vulnerable	51% (N=156)	48% (N=91)	57% (N=54)

alike to whites and blacks, the results are far different. Deep-southern whites, as predicted, do *not* respond to occupational classifications as southern blacks do. Indeed, to the extent that there is any occupation-related difference in the turnout rates of southern whites apparent in the data, it is the "vulnerable" whites who participated more, reversing the relationship found among blacks. (See Table 4.)

Given the limitations of this SRC sample, we cannot claim these results as conclusive evidence of the importance of economic coercion among southern blacks. We offer them, therefore, only as a partial independent confirmation at the individual level of a set of relationships our article and this comment established at the aggregate level. As such, they are still most encouraging.

One final comment is in order concerning the interpretation of the data reported in our Table 2 above. Like Mr. Kernell, we discover a larger Beta weight coefficient for the median education variable than for any other, even under controls. Not only does this raise again the problem we discussed earlier of how to interpret the education variable, but also it raises centrally the whole question of how to treat Beta weights in evaluating policy alternatives. Mr. Kernell questions the policy implications we draw from our analysis on grounds that the Beta weight associated with the education variable is greater than that associated with the economic vulnerability variable, and that a greater payoff in terms of increased black voter turnout would therefore accrue from concentrating resources on altering education levels rather than on altering economic vulnerability levels. Beta weights, however, indicate only

the proportion of a standard deviation change in a dependent variable produced by one standard deviation change in the independent variable. Because different independent variables have different standard deviations, however, before embracing or rejecting any particular policy recommendation it is imperative to examine two facts in addition to the Beta weight: first, the actual sizes of the standard deviations of the various variables; and second, the "cost" of achieving a one standard deviation change in these variables. The first of these is readily available, but the second is far more elusive. No comprehensive and conclusive statement can be made here, therefore, about the relative advantages and disadvantages of the two policy alternatives arising from both Kernell's and our own research. Nevertheless, some preliminary scouting of the terrain is possible. In particular, the data indicate that to achieve a one standard deviation change in black median education in a county would require raising the median education level of the county's entire adult black population by an average of 8/10 of a year, or almost a full grade level (using 1970 figures). By contrast, a one standard deviation change in the proportion of a county's black population most vulnerable to economic coercion would require shifting only 6 per cent of the county's adult black population out of this vulnerable condition. Since, as we argue, this latter result could be at least partially achieved through basic administrative changes in the welfare system with relatively little additional expenditure compared to the costs of additional education, we continue to believe there is a strong argument on "cost effective" grounds alone for pursuing policies aimed at relieving vulnerability, despite the greater Beta weight associated with the education variable. Clearly, however, this conclusion must remain tentative pending further detailed analysis.

Federal Registrars and White Turnout

In addition to his direct comments on our article, Mr. Kernell presents two further pieces of analysis that bear on the general subject of black political effectiveness in the South and that require brief comment here. The first is a more detailed examination of the impact on black turnout of the proportion black in a county. The second is an examination of the impact of black voter turnout on white voter turnout. In both cases, Mr. Kernell develops interesting findings that are quite consistent with our own earlier hypotheses. In both cases, however, he also encounters some serious difficulties.

Table 4. Voter Turnout as a Function of Economic Vulnerability Among a Sample of Deep-Southern White Interviewees

	% Voting, 1968 and 1970 (N=141)
Invulnerable	59% (N=34)
Vulnerable	64% (N=107)

So far as the former is concerned, Kernell confirms and strengthens our earlier finding that high black population majorities are associated with high black turnout rates, largely because of the effect substantial black majorities have as organizational resources. But when he attempts to measure the impact of political organization directly in mobilizing the electorate, Kernell comes perilously close to a tautology. This is so because the indicator of movement activity and organization that he chooses—the presence of federal registrars—is by Kernell's own admission "an important ingredient in black political mobilization" (footnote 17).⁶ What Kernell fails to do, however, is specify why this should be so. His use of the registrar figures as a surrogate for local organization implies an assumption that the presence of a federal registrar signifies the existence of a strong, local black political organization. The triggering mechanism of the Voting Rights Act, however, was an administrative finding of *low* black registration, which would suggest that registrars were sent where black organization was weakest. Nor did the registrars create organizations or seek to dissipate apathy by stimulating voter registration drives. Rather, their role was completely passive: to appear at the courthouse and register eligible voters who presented themselves. Finally, Kernell's interpretation of the registrar figures overlooks the numerous counties (like Sunflower County, Mississippi) in which Movement activity was heavy but to which registrars were never dispatched until after 1968.

An alternative, and we think more plausible, explanation of the positive relationship Mr. Kernell finds between the presence of federal registrars and black turnout might be that the registrars' presence dissipated *fear* by demonstrating, at long last, the federal government's apparent intention to protect black voters, and to do so at the local level where the protection was really needed. Federal registrars provided tangible evidence of federal support for the expansion of black political participation and thus probably constituted an important source of confidence to fearful potential black voters. In short, Mr. Kernell's findings here seem to confirm our own conclusions about the role of fear in black participation.

Kernell's demonstration that white voting turnout tends to increase with increases in black turnout is equally intriguing, but also somewhat troubling. The problem here is that it

is difficult to determine how much confidence to put in the results since Kernell neglects to carry over into his own analysis of white turnout the same empirical precision he brought to bear in evaluating our analysis of black turnout. In particular, no controls are introduced to determine whether the sole independent variable, black turnout, that is used to explain variations in white turnout is really a surrogate for some other factor altogether that is related to *both* black turnout and white turnout. White education levels, for example, tend to be highest in the black-belt counties, which also have the highest black turnout rates, as Kernell has shown. In the absence of careful analysis employing controls, it is difficult to interpret the results with any confidence.

Conclusion

Mr. Kernell has done a service in subjecting our analysis to scrutiny and extending its reach over time. For the most part, we find his detailed objections to our analysis to be thoughtful but not convincing, for the reasons elaborated at length above. We therefore believe that our mode of analysis survives his scrutiny intact. In fact, in many respects, we believe Mr. Kernell's findings complement ours, particularly in verifying—as we would have predicted—that our findings about the role of fear do *not* seem to apply to white-majority counties; in confirming the importance of the organizational resource represented by high black proportions in a county; and in demonstrating a positive relationship between black turnout and the presence of voter registrars, which we believe can be explained in terms of the registrar's role in dissipating fear. Where we differ from Mr. Kernell is in our evaluation of the accuracy of 1960–64 data for analyzing the 1967 and 1968 elections and in our conclusions about the role of "fear" in influencing black participation both in 1967/68 and in 1971. So far as the latter is concerned, the application of our earlier mode of analysis to the more recent 1971 elections, and a brief examination of available survey data, confirm, we believe, our earlier conclusion about the importance of fear in explaining black turnout in the black-belt counties of Mississippi, even though that importance has diminished somewhat over time. Finally, and perhaps most interestingly, this updating of the analysis suggests the potential utility of the indices and theory we developed in our original article to students of political development interested in studying empirically the whole process of political change associated with the "opening" of political systems to mass popular participation in situations involving extensive social and economic conflict.

⁶ Our own index of civil rights movement activity was explicitly designed to avoid this problem by measuring movement activity directly instead of in terms of its hypothesized results. Yet Mr. Kernell neglects any mention of our organizer index.

COMMUNICATIONS

TO THE EDITOR:

Recently John Ruggie extended some of the tools of economic analysis into the field of international organization "to demonstrate and illustrate one possible theoretical system," specifying "when, where and how states seek to organize internationally,"¹ I believe that the full theoretical implications of his analysis were not drawn upon and that the article could have been substantially enhanced by a simple extension of his model. Let's imagine his same world consisting of two identical states, *A* and *B*, with identical preferences, resources, and capabilities. Resources are of course, limited and may be used in the pursuit of given tasks either domestically or through international cooperation. We will, however, change two of his assumptions in the direction of reality. First, we change his assumption that "decisions made in one context remain independent of decisions made in another," (p. 878) to admit their dependence. It seems highly unrealistic to imagine that country *A* does not consider what is being done for it internationally in deciding what to do for itself on the domestic level. Secondly, that there is "no direct interaction between *A* and *B*" (p. 879) seems very naive in a theory relating to international organization.

Let us now go to Figure 1, which is a slightly modified version of Ruggie's Figure 2. On the vertical axis we measure for only one country, *A*, "Task Performance through International Organizations" (hereinafter referred to as "*i*") and on the horizontal axis "Task Performance through National Organizations" (referred to as "*n*"). A preference map, composed of a set of indifference curves completely describes *A*'s preferences regarding *i* and *n*. The slope of an indifference curve at any point shows the rate at which *A* is willing to give up *i* to obtain *n*. This is referred to as the marginal rate of substitution of *i* for *n* (MRS_{in}). In the diagram we have drawn only two of the infinite number of indifference curves in *A*'s preference map, I_1 and I_2 .

A's ability to perform tasks through *i* or *n* depends upon the resources available to *A*. The feasible and efficient combinations of *i* and *n* possible for a given amount of resources are represented by a transformation curve, the slope of which at any point indicates how much more *n* can be obtained by transferring re-

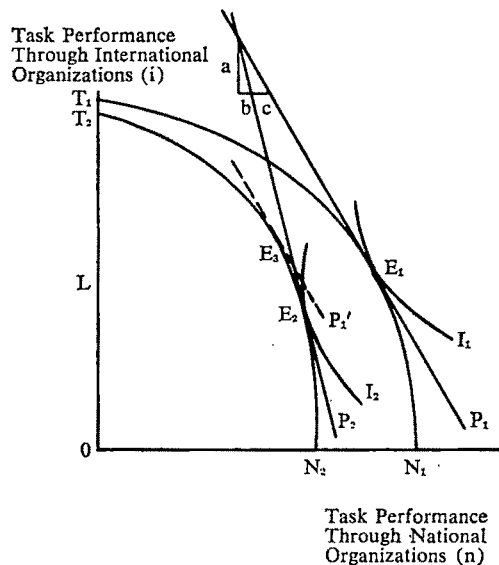


Figure 1. Task Performance through National and International Organizations.

sources from *i* and vice versa. This is referred to as the Marginal Rate of Transformation of *i* to *n* (MRT_{in}). Assume for the moment that *A*'s resources permit the transformation curve $T_1 N_1$. Given $T_1 N_1$, *A* will be in equilibrium at E_1 where $MRS_{in} = MRT_{in}$, either of which is represented by the slope of line P_1 . The slope of P_1 can be thought of as the implicit "price" of *n* in terms of *i* since it represents the rate at which one is "traded off" for the other. We have assumed that the picture for *B* is exactly the same as described for *A*.

I am in agreement with Ruggie that if we now shrink the resources of *A* (leaving *B* the same), the reduction in *A*'s ability to produce *n* will contract at a greater rate than its ability to produce *i*. This is demonstrated by a drop in the transformation curve to let us say $T_2 N_2$. The reason behind this is that *B* continues to contribute to international organizations and that many of the things supplied by the international organization are, at least in part, public goods. *A* receives the benefit of *B*'s contribution whether or not *A* contributes. This may be best illustrated in the extreme case where if *A*'s resources fall to zero, its ability to produce *n* is reduced to zero. But some *i* will still be forthcoming from the international public goods supplied exclusively by *B*. The transformation curve for *A* would become a single point on the vertical axis, such as L .

¹ John Gerard Ruggie, "Collective Goods and Future International Collaboration" *American Political Science Review*, 66 (September, 1972), 874-93.

Should there be no interaction between A and B , A will be at a new equilibrium at some point, like E_2 , with a new implicit price for i and n represented by the slope of P_2 . This is the point at which we are left by Ruggie. Let us take this a step further and open up an interaction between A and B and observe the implicit prices of i and n in both places for they now differ. I have extended P_1 and P_2 to their intersection for comparative purposes. In A , i trades off for n at the rate a/b . In B , the same trade off is $a/b+c$. In terms of "opportunity cost," n is relatively costly in A and i is relatively costly in B .

If i and n were normal commodities, any text in international economics would show that A would begin to specialize in i , B in n , and that through trade both would be better off. For example, if A could "trade" with B at the implicit price ratio existing in B , A would move to "production" at point E_3 , where A 's MRT_{in} = slope P_1' = slope P_1 , thereby taking on greater international responsibilities and acquiring n through international trade. (Indeed the implicit price will not remain equal to the slope of P_1' since as B moves to a point to the right of one like E_1 the "price" of i in terms of n decreases. This modification would add clutter without insight.) Trade would take place along line P_1' , raising A to some indifference curve above I_2 , until the highest possible indifference curve is reached at which point MRS_{in} = slope P_1' = MRT_{in} , thus establishing a new equilibrium. But i and n are not normal commodities and n cannot be provided to A by B since n is explicitly a domestic task. How can n be acquired through trade? Simply by transferring some of the tasks which have been performed domestically into the international sphere. A therefore has the incentive to make some of the tasks which have been performed domestically into tasks which are now performed by international organizations. (If this change is made then the dimensions of the diagram change to reflect that with the same level of resources more i and less n is forthcoming simply because there are now more i tasks and fewer n tasks.)

Performance of domestic tasks as compared to internationally performed tasks becomes *relatively* more difficult for the smaller country, since they are performed entirely out of domestic resources. International tasks on the other hand do receive some support from outside the smaller country. Those tasks aided by the international community become easier to perform *relative* to domestic ones. Therefore if the smaller country can shift some domestic tasks

into the international sphere, a greater number of tasks will be performed in total. The smaller country may maintain a preference for domestic task performance but nevertheless as it becomes relatively smaller (in terms of capacity) it feels an even greater incentive to shift tasks onto the international community.

While A has the incentive to shift tasks into the international sphere, doing so is not possible without the cooperation of B . First of all, A is able to present B with its greater willingness to assume international tasks and have some of its tasks assumed by international cooperation as is demonstrated by the willingness of A to move toward point E_3 . Secondly, B has had an experience exactly opposite to that of A . B finds that if i and n were normal commodities, it would be to B 's advantage to try to specialize in n and obtain i through trade. In our example however, this is equivalent to a willingness on B 's part to assume some tasks which were i tasks and make them n tasks. At a time when A is placing greater demands upon the international system the implicit price of internationally performed tasks rises. B 's response is to shift toward greater performance of n tasks. Whether or not B will accept international tasks as domestic tasks would depend upon the system operating in B . Neither i nor n are homogeneous items (as is always the case in similar economic analysis). In increasing n , B may perform tasks which may be public goods within B but not between A and B (e.g. public works in B), public goods in A but not in B (aid to A) or public goods in both places (e.g., pollution abatement). The latter two would represent the adoption of international tasks as domestic. Which of these B would choose is the meat of international cooperation and cannot be answered by this analysis.

Whether or not there is a net increase in the participation of international organizations in task performance will depend upon how many new tasks are shifted to the international organizations by the small countries (countries like A) and how many formerly international tasks are adopted as domestic by the larger countries (here, B countries). This is a problem in dynamics and depends largely upon the gap between the "small" countries (size in terms of capabilities) and the "large." If the gap is widening over time we should be observing the "large" countries assuming as domestic some tasks which were international, and the "small" countries simultaneously transferring domestic tasks to the international sphere.

Ruggie's data support the conclusion that the large countries will adopt some international tasks as domestic. His examination of

1960s' data on development assistance from the Development Assistance Committee (DAC) countries of OECD shows a direct relationship between GNP (representing national resources) and the amount of resources being devoted to development assistance. This is an indication that one task (development) which formerly was handled almost exclusively by domestic organizations in the developing country is being shifted at least in part out of the developing country and into the international arena. Second, his data show an inverse relationship between GNP and the ratio of multilateral to bilateral aid contribution. Since these data pertain only to DAC countries—all of which may be classified as "large" (or *B* countries in our above analysis), another hypothesis is given credibility. That is, as a country becomes increasingly "large" it has a tendency to adopt formerly international tasks as part of its own domestic tasks. In this case as the *A* countries shift their development task into the international sphere, the *B* countries are willing to adopt development as one of their domestic objectives and extend increasing amounts of bilateral aid.

My model has indicated an increasing incentive for small countries to shift domestic tasks into the international sphere as the capability gap widens between them and large countries. We would expect negative correlations between the difference: (Developed Country GNP) minus (Developing Country GNP) i.e., the development gap, and the ratio:

$$\frac{\text{Developing Country Contribution to Multilateral Development Efforts}}{\text{Developing Country Contribution to Domestic Development Budget}}$$

To substantiate this theory such results would only be valid in a world where there is this "capability" gap which is widening between the *A* and *B* countries, that is where the production possibilities curves such as those in Figure 1 are growing farther apart. This is of course the case.² Generating the data for a test on this relationship would be a considerable task since domestic development budgets would be difficult to disaggregate from overall budgets. Nevertheless, we have rough approximations to verification of this in the fact that only recently have developing countries begun to look to international cooperation as a means to economic development. Only within the past 25 years or so have they begun to contribute in increasing amounts to such organizations

as regional development banks, economic integration movements and so on. The numerator in the above ratio has only recently become positive, at a time when the development gap is widening.

What I have attempted to do here is to draw out some of the possible extensions of the Ruggie model so as to create a more dynamic exposition of the framework within which nations interact to decide upon international as opposed to domestic task performance. Indeed the facts that the task categories are not homogeneous and that domestic tasks can be shifted to the international arena complicate the issue considerably.

WILLIAM LOEHR

University of Denver

TO THE EDITOR:

I am pleased that Professor Loehr thought my article to be sufficiently important to warrant a comment on his part; in turn, his comment is sufficiently thoughtful to warrant a response on mine.

Loehr's "extension" of the model presented in "Collective Goods and Future International Collaboration" (*American Political Science Review*, 66 [September, 1972], 874-893) stems from his desire to change what he takes to be two of my assumptions "in the direction of reality." I must point out, however, that, perhaps because of lack of clarity in the original formulation, he misconstrues both assumptions

he wishes to change. First, he interprets the assumption that "decisions made in one context remain independent of decisions made in another" to mean that what country *A* does domestically in any given sector will remain independent of what it will seek to do internationally in that sector. It was not my intention to convey such an impression. In fact, the entire first half of the article, including Figures 1 and 2, Propositions 2 and 3, as well as Tables 1 and 2, was designed to show precisely how the domestic and international arenas are related to one another as a country decides upon an appropriate level of international collaboration. What the assumption means, as is stated at several points in the discussion, is that decisions in different functional contexts, such as public health, the regulation of nuclear proliferation, and monetary affairs, will be made on the basis of issue-specific criteria, so that these contexts remain independent of one another.

² Everett E. Hagen and Oli Hawrylyshyn "Analysis of World Income and Growth, 1955-1965," *Economic Development and Cultural Change*, 18 (October, 1969), 42-63.

Second, Loehr wishes to drop the assumption that there is "no direct interaction between *A* and *B*" (where *A* and *B* are two countries), thinking it to be naive when theorizing about international organization. I did too. The assumption is explicitly dropped on p. 886, and the entire second half of the article is devoted to conceptualizing and illustrating the consequences of different types of interdependencies in the relationships between *A* and *B*. For some inexplicable reason, Professor Loehr ignores this section altogether.

Loehr's comment, in other words, is not so much an extension of my model based on different assumptions, as it is an alternative formulation based on the identical assumptions. As such, it is an interesting contribution with which I would have but minor quarrels, which need not be pursued in this forum.

JOHN GERARD RUGGIE

University of California, Berkeley

TO THE EDITOR:

The review of my book, *The Politics of Reform in Peru*, in the March 1972 issue of the *American Political Science Review*, overlooked an essential point in criticizing certain areas of omission. Contrary to the reviewer's assumption that this was primarily a book about APRA and Peru, I was trying to discover the role of the mass reform party as an instrument for initiating reform. Regardless of emotional attitudes toward the Apristas of the present, they did provide some of the best historical evidence available concerning this connection. The reviewer failed to discern how "development" was relevant, because he regarded the party as the point of the book. My intention was to use APRA (and, in less depth, AD in Venezuela and MNR in Bolivia) as the source of independent variables, in order to trace the apparent effects on developmental reforms as dependent variables. How well I succeeded is, of course, open to debate, but let the book be judged on the basis of its design, and let other books be written by those who prefer different approaches.

GRANT HILLIKER

Ohio State University

TO THE EDITOR:

While I much appreciate Professor Rudolf Heberle's extensive review of *From Karl Mannheim* in the September, 1972 *American Political Science Review*, I should like to point out that his statement that "The essays have been available in English before, except one which is given in Wolff's translation (Problems of Sociology in

Germany)" is not quite correct. Several other items have never before appeared in translation. There is another essay "On the Diagnosis of Our Time," which was printed in the first issue of the shortlived *Mass und Wert*, coedited by Thomas Mann (1937). And there are, in the Introduction, the translation of the original analytical table of contents, three closely printed pages of *Ideologie und Utopie*, neither translated nor reprinted in German in later editions (Sec. XVI); an excerpt from an untranslated small book on the "tasks of sociology called for by the present—a teaching program" of 1932 (Sec. XVI, 1); and the translation of Mannheim's answer to a newspaper poll of 1937 (Sec. XIX, 2). It should also be pointed out that the book is available not only in the hardcover edition for \$12.50 but also as a Galaxy paperback for \$3.95.

KURT H. WOLFF

Brandeis University

TO THE EDITOR:

I wonder if I might venture a comment on Edward N. Muller's impressive "A Test of a Partial Theory of Potential for Political Violence" (*American Political Science Review*, 66 [September, 1972], 928-959). Though ably conceived and rigorously executed, the article seems to claim a bit too much.

The central thrust of the article is to debunk Ted Robert Gurr's theory of relative deprivation (RD) in favor of an "alternative approach" resting on two variables: (1) the perceived legitimacy of the regime, (2) the perceived viability of political violence in attaining objectives. However, as Professor Muller notes, legitimacy is one of four intervening variables in Gurr's earlier formulation of RD theory (the other three being facilitation, institutionalization, and the coercive potential of the regime), with RD itself as the independent variable and violence as the dependent variable (Gurr, "A Causal Model of Civil Strife: A Comparative Analysis Using New Indices," *American Political Science Review*, December 1968). Although there are several references to Gurr's *Why Men Rebel* (1970), Professor Muller does not refer to Gurr's further refinement in that work of his conceptualization of the intervening variables. In Gurr's revised formulation, whether RD explodes into actual violence becomes the function of two sets of intervening variables: (1) the scope and intensity of normative (historical, cultural, ideological) justifications for violence as well as *men's perception of the effectiveness and utility of violence in solving social problems*; (2) the relative physical strength of the contending par-

ties as well as their ability to provide institutionalized, peaceful alternatives to violent expression of discontent.

Moreover, Professor Muller seems to overlook the point that RD, though fundamentally a psychological concept, may have a variety of bases. The most common is the economic, in which RD refers to men's perception of discrepancy between expectation and achievement. Equally important is the political base, in which RD refers to men's perception of discrepancy between their socioeconomic status and political influence. In fact, John T. McAlister, Jr., has applied to the Vietminh Revolution Gurr's model of relative deprivation as a basic condition of political violence. He identifies a group of relatively well-educated Vietnamese whose socioeconomic advance was thwarted by inadequate employment opportunities. He focuses on a second group of native intelligentsia who had achieved a degree of socioeconomic advance but no corresponding political power. McAlister concludes that the denial of political power to the intelligentsia—and the resultant discrepancy between socioeconomic status and political influence—was a most important source of discontent leading to revolution. He believes that much of the impetus for the Vietminh Revolution came from the relatively small segment of the Vietnamese population who had experienced some social mobility in the colonial contest, but no commensurate political power (McAlister, *Vietnam: The Origins of Revolution*, 1970, esp. pp. 325ff.).

In short, it is difficult to see how Professor Muller's treatment is an "alternate approach" to Gurr's. Would it not be more accurate to say that Muller has selected for extended treatment two of Gurr's intervening variables? (Could it not be, for example, that the erosion of the "legitimacy sentiment," as Professor Muller puts it, is due to relative deprivation?) In that case, are not the Gurr and Muller approaches complementary rather than mutually exclusive?

MOSTAFA REJAI

Miami University

TO THE EDITOR:

Professor Rejai's claim that my partial theory is not an alternative to Gurr's theory is correct—if one interprets the words "alternate ap-

proach" (p. 930) in my article to mean *mutually exclusive*. Probably, I should have specified what I meant by "alternate approach" more precisely; but, I assumed it was clear enough from the context of the article as a whole. I did not intend to equate "alternative" with "mutually exclusive;" rather, I meant to call attention to the difference in emphasis between Gurr's theory, which proposes that relative deprivation is the root cause of potential for political violence (*Why Men Rebel*, 1970, pp. 24, 36–37, 159, 328), and my partial theory, which proposes that relative deprivation is not one of the major causes of potential for political violence, and is considerably less important than the diffuse support/legitimacy/political trust concept. My partial theory is definitely complementary to Gurr's in the following ways: (1) Legitimacy is an important concept in both formulations; (2) my partial theory includes the hypothesis, suggested by Gurr (p. 193) that shared discontents (presumably relative deprivation) are only related to potential for political violence via legitimacy, i.e., have no direct effect on potential for political violence independent of legitimacy (which, by the way, seems to be inconsistent with the relative deprivation → potential for collective violence → potential for political violence sequence conjectured at pages 24, and 159 and depicted in the causal model at page 328); and (3) belief in the utility of past violence is an important concept in both formulations.

Actually, in a presentation of a full theory of potential for political violence ("A Nonalienation Interaction Theory of Political Protest," paper prepared for the European Consortium for Political Research Joint Sessions of Workshops, "Political Behaviour, Dissatisfaction and Protest," Mannheim, Germany, April 12–18, 1973), I hypothesize that specifically *politicized* relative deprivation—relative deprivation that is blamed on political and social structures—is an important direct cause of potential for political violence, when it interacts with other variables. On the other hand, I still do not conceive of relative deprivation, even in a "politicized" form, as the root cause of potential for political violence, with Legitimacy & Co, as intervening variables.

EDWARD N. MULLER

SUNY at Stony Brook

On Understanding Fascism: A Review of Some Contemporary Literature*

A. JAMES GREGOR

University of California, Berkeley

The above collection of recently published books provides a convenient occasion for undertaking a review of our efforts to "understand" generic fascism—and particularly Mussolini's Fascism. For almost fifty years now, biographers, historians, political scientists, and social scientists in general, have been attempting to "understand," in some substantial sense, the "meaning" of the tangled skein of events we now identify with fascism. Whatever "understanding" is taken to mean, it implies, minimally, that we can *define* our objects of inquiry with some precision—that we entertain some confirmed empirical *generalizations* about them—and that these generalizations be *logically related* to produce a comprehensible narrative account. Such an account is generally spoken of as an "empirical theory" of our range of concerns. Preliminary to, implicit in, and consequent upon, the possession of such a theory, are *normative* or *evaluative considerations* or both. In general, it can be said that to "under-

stand" a complex political sequence, one is expected to have, at his disposal, sets of such formulations available for public display and intersubjective scrutiny.¹

Social scientists have employed a variety of intellectual strategies for coming to understand complex historical and political phenomena in this way. These strategies have included interpretive biographies, special accounts of one or another aspect of a complex sequence of events, broad-gauged narratives devoted to the general history of the phenomena in question, as well as general theoretical approaches and special and specific theories calculated to explain them. Our collection includes instances of all these strategies: a major biographical work (by Richard Collier); special accounts of American public opinion concerning Fascism (by John Diggins) and the relationship between Italian business leadership and Fascism (by Roland Sarti); a standard historical treatment of fascist movements (by F. L. Carsten); two books by historians that attempt to put the entire sequence in theoretical perspective (the works of Gilbert Allardyce and H. Roderick Kedward); as well as three books devoted to "classic" and special theories of fascism (the neopsychoanalytic interpretation of Wilhelm Reich, the moral crisis thesis of Peter Drucker, and the neo-Marxist assessment of Reinhard Kuehn). Each of them aspires to provide something like adequate understanding of generic fascism in general, and Mussolini's Fascism in particular.

Biography and the Understanding of Fascism

Richard Collier's *Duce!* is an effort to provide understanding of Italian Fascism by revealing something of the personal character of its leader. Collier tells us interesting and insightful things about Mussolini which may be entertaining historical gossip, but which provide precious little leverage on understanding Mussolini or Fascism in any significant theoretical sense. What seems to hold Collier's entire connected narrative together is his penchant for

**The Place of Fascism in European History*. Edited and with an Introduction by Gilbert Allardyce. (Englewood Cliffs, N.J.: Prentice-Hall, 1971. Pp. 178. \$2.45.)

The Rise of Fascism. By F. L. Carsten. (Berkeley: University of California Press, 1967. Pp. 256. \$6.50.)

Duce! A Biography of Benito Mussolini. By Richard Collier. (New York: Viking, 1971. Pp. 447. \$12.50.)

Mussolini and Fascism: The View from America. By John P. Diggins. (Princeton: Princeton University, 1972. Pp. 524. \$16.50.)

The End of Economic Man: The Origins of Totalitarianism. By Peter F. Drucker. (First Colophon Edition. New York: Harper & Row, 1969. Pp. 271. \$1.95.)

Fascism in Western Europe, 1900-45. By H. Roderick Kedward. (New York: New York University Press, 1971. Pp. 260. No price given.)

Formen Buergerlicher Herrschaft: Liberalismus-Faschismus. By Reinhard Kuehn. (Hamburg: Rowolt, 1971. Pp. 190. DM 3.80.)

The Mass Psychology of Fascism. By Wilhelm Reich. (Newly translated by Vincent R. Carfagno. New York: Farrar, Straus & Giroux, 1970. Pp. 400. \$3.25.)

Fascism and the Industrial Leadership in Italy, 1919-1940. By Roland Sarti. (Berkeley: University of California Press, 1971. Pp. 154. \$8.00.)

¹For a more protracted account of what "understanding" might be taken to mean cf. A. J. Gregor, *An Introduction to Metapolitics* (New York: Free Press, 1971), chap. 7.

explaining the behavior of his protagonists in terms of general dispositions to behave as they do. Mussolini behaved as he did largely because he was afflicted with an "unwavering" aim: "to command attention at all costs" (p. 37). Furthermore, Mussolini had developed, according to Collier, an "obsessional life-long hatred of the rich" (p. 41). Between his longing for "personal recognition and prestige" (p. 42), his "gnawing ambition" (p. 45), his "built-in instinct for violence" (p. 45) and his "temperamental" activist disposition (p. 48), Mussolini acted out an elaborate psychodrama using, as a background, the props that history identifies as Fascism.

Whatever understanding such accounts deliver, it is parasitic on ill-contrived psychological generalizations and some curious notions about social dynamics. We do not know, for instance, if we are to take the talk of "instincts" and "obsessions" seriously. But even if we consider such talk nothing more than thoughtless turns of speech, explanations in terms of noninstinctual and nonobsessional psychological dispositions are extremely difficult to warrant. Finally, even if we could produce an empirical warrant for such explanations of behavior, we would still face the problem of explaining why individual psychological dispositions can be acted out on a national and world scale.

Collier, himself, makes it particularly difficult to establish the truth of his own interpretation. We are never quite sure what evidence Collier is prepared to accept in identifying the dispositional traits used in his accounts. As a case in point, he goes to some lengths to record significant instances of Mussolini's personal courage, beginning with his youthful defiance of a farmer's threatening shotgun, through his quietly suffering twenty-seven surgical operations (all but two without anesthetics) to remove bits of shell casing from his body, to Mussolini's calm reaction to various assassination attempts and his near brush with death during his rescue in 1943 (pp. 36, 50-51, 57, 90-91, 268). With such evidence at our disposal we might have imagined that "courage" might be a dispositional property that could be attributed to the *Duce*. And on page 36 Collier seems to grant as much. Mussolini, he tells us, "was no coward." And yet we are mystified to find (on page 361) that when Mussolini faced his assassin, calmly opening his jacket to say "Shoot me in the chest," Collier can only suggest that that display represented "the first time in his life [in which Mussolini] had risen above fear"! Mussolini was, according to Collier, dispositionally "fearful"—and only on the occa-

sion of his assassination did he succeed in rising above it. Curious.

The issue of Mussolini's cowardice is not the only occasion for such a curious treatment of evidence. Collier goes on to tell us, for instance, that the young Mussolini would "often" spend "hours astride an old farm horse in the midst of the swift-flowing River Rabbi, so riveted to a book that only twilight reminded him to head for home" (p. 35), and further, that as a young man, Mussolini would remain "immersed for hours in . . . books" (p. 39). One might imagine that as a consequence, and on the basis of such evidence, the young Mussolini was something of an intellectual. Not at all. Collier informs us that for all his immersion in, and riveting to, books, Mussolini was "always a superficial reader." Mussolini, in fact seems to have read only nine pages of any book—"the first three, the last three, and the three in the middle." Not only that, but his reading was, again according to Collier, very selective. Mussolini, we are told, "read only those writers who spoke for violence, like Georges Sorel and Friedrich Nietzsche" (p. 46).

If we are expected to give credence to such accounts it would seem that one might provide more substantial evidence in their support. Since at least 1964 Mussolini's youthful literary work, published between 1901 and 1914, has been available for scholarly scrutiny. During the historical period under consideration, Mussolini reviewed, for a variety of publications, a large number of books ranging from Russian novels to Robert Michels's socioeconomic treatise on *Cooperation*. Beside these reviews, Mussolini wrote three long monographs and a novel. He translated at least eighteen pieces from the French and the German, including pamphlets by Karl Kautsky, Wilhelm Liebknecht, and Peter Kropotkin. In his various articles Mussolini cited every work then available by Karl Marx and Friedrich Engels—and provided quotations from Marx's *Contribution to the Critique of Political Economy*, the volumes of Marx-Engels correspondence published in 1913, Marx's articles published in the *New York Tribune*, Engels's volume on the *Conditions of the Working Class in England in 1845* and the *Anti-Duehring* as well as Engels's famous introduction to Marx's *Class Struggles in France*.² In his monograph on the Trentino (*Il*

² Cf. A. J. Gregor, *The Ideology of Fascism* (New York: Free Press, 1969), pp. 95f. Any reasonably adequate assessment of Mussolini's intellectual capabilities would require an acquaintance with his collected works now available as the *Opera omnia*, 36 volumes (Florence: La fenice, 1951-1963). In the

Trentino veduta da un socialista), Mussolini provided extensive quotations from H. S. Chamberlain's *Grundlagen des neunzehnten Jahrhunderts*. In his monograph on John Huss (*Giovanni Huss il Veridico*) Mussolini quoted extensively from the work of Friedrich von Bezhold on the history of the Reformation in Germany—and from the works of L. Leger, C. Cantù—as well as those of Huss himself. In his polemics against Alfredo Tagliacatela, *L'uomo e la Divinità*, published when Mussolini was twenty-one, one can find verbatim quotations from a variety of ethical and philosophic works.

Whether all this is simply the product of "superficial reading" requires more in terms of evidence and analysis than Collier's insistent pronouncement. That it was the product of just having read nine pages of each book is more than doubtful. Appropriate quotations are not usually culled from the first three, the last three and the middle three pages of any book. That Mussolini read *only* authors who "spoke for violence" seems to be simple nonsense.

If we are to come to "understand" Fascism by coming to understand its *Duce*—and understanding him implies that we entertain defensible generalizations about his dispositional traits—it would seem that Collier gives us little intellectual leverage. Collier's book is, in fact, representative of an entire genre of biographical literature devoted to the interpretation of Fascism by coming to "understand" its leaders.³

bibliography appended to his book, Collier refers (p. 410) to Mussolini's *Opera omnia* as composed of "23 volumes," which suggests that he is not well acquainted with the work. Furthermore, on the same page Collier cites the pre-war collection of Mussolini's speeches and essays, the *Scritti e discorsi*, as involving only 12 volumes when they, in fact, involve 13—and he gives "La fenice" as the publisher when the publisher was, in fact, Hoepli.

³It would seem that Alan Cassels (*Fascist Italy* [New York: Crowell, 1968] pp. 1f.) is correct in suggesting that because we have little serious understanding of the entire fascist phenomenon, we attempt to search out the "meaning" of it all in biographies devoted to fascist leaders. There is an incredible number of biographies devoted to Mussolini—and most of them, largely because they serve as surrogates to serious understanding, are hopelessly incompetent or transparently biased or both. The best biography of Mussolini is, of course, the as yet incomplete work of Renzo De Felice (*Mussolini il rivoluzionario* [Turin: Einaudi, 1965], *Mussolini il fascista: la conquista del potere, 1921-1925* [Turin: Einaudi, 1966], *Mussolini il fascista: l'organizzazione dello stato fascista, 1925-1929* [Turin: Einaudi, 1968]). The philofascist biographies of Giorgio Pini and Duilio Susmel (*Mussolini: l'uomo e l'opera*, 4 volumes [Florence: La fenice, 1953-55]) and Ivon de Begnac (*Vita di*

What is generally missing from such idiographic accounts is theoretical substance. There are no defensible generalizations that bind the instantial descriptions together. Collier, as the case in point, offers us little more than a collection of interesting vignettes shaped out of the memories of the hundreds of participants in the Fascist sequence whom Collier has interviewed twenty-five years after the events they are asked to recall. With a research staff that at its peak totaled forty-four, involving an enterprise that covered seven countries, consumed three-and-a-half years of time, and elicited the recollections of 454 eyewitnesses, one might have legitimately expected a great deal more.

Biographies seem to be afflicted with special kinds of disabilities. This may be because the authors of biographies believe that since they are dealing with an individual life they need not search out defensible generalizations that might successfully lace their account together. Irrespective of that seeming conviction, it is obvious that biographies, in their pursuit of understanding, generally exploit unrestricted but poorly formulated, generalizations about their subjects' psychic or emotional dispositional traits. In this respect Collier's volume is simply one instance in a large class of similar volumes. Its shortcomings are those of the entire class.

All this is not to say that such disabilities uniquely afflict biographies. John Diggins's recent volume concerning American attitudes toward Fascism seems to suffer from similar deficiencies.

Benito Mussolini, 3 volumes [Milan: Mondadori, 1936-40]) are valuable for scholarly purposes: they undertake a careful review of the available historic evidence in their efforts to expose pervasive bias and scholarly irresponsibility in the academic treatment of Fascism. In this respect Bruno Spampanato's *Contromemorale*, 3 volumes (Rome: Illustrato, 1952) is not as useful, beset as it is by selective reporting of evidence and some significant historical inaccuracies. Most of the biographies available in English are all but totally inadequate. Most are written by non-academics and are animated by an irrepressible bias. Those of Paolo Monelli, *Mussolini: The Intimate Life of a Demagogue* (New York: Vanguard, 1954) and Laura Fermi, *Mussolini* (Chicago: University of Chicago, 1961), are among the worst. It is interesting, in this regard, to compare the "official" biography written by Giorgio Pini while he served the Fascist Regime, *The Official Life of Benito Mussolini* (London: Hutchinson, 1939), with the postwar biography he wrote with Duilio Susmel. Of the biographies available in English, Christopher Hibbert's, *Benito Mussolini: The Rise and Fall of Il Duce* (Baltimore: Penguin, 1962) is among the best. We do not have, as yet, a fully objective and competent biography of Mussolini in English.

Special Interpretive Histories and the Understanding of Fascism

For our purposes, John P. Diggins's *Mussolini and Fascism*, is an instructive book. While its principal object is to reveal *something* about the prewar American views concerning Mussolini and Fascism (which it does quite well), it attempts a great deal more. Buried in Diggins's account is an implicit judgment that Americans had somehow "misunderstood the essence of the Fascist movement" (p. xv). Diggins admits that Fascism was "an amorphous phenomenon" and that "few American thinkers could define adequately the nature of Fascism" (pp. xvi, 445)—and he grants that until the end of the Second World War there was "no consensus on the nature of Fascism" (p. 365). But for all that, he alludes to "the reality of Fascism" and "the real nature of Fascism" (pp. 483, 504) as though behind the amorphousness and the fugitive definitions there was an "essence" that Americans *should* have perceived. Presumably we have, today, espied that "essence." Unhappily, a characterization of that "essence" is scarcely to be found in the five hundred pages of Diggins's closely written prose.

It is clear, of course, that for Diggins, Fascism was a "Bad Thing." This judgment seems to satisfy his moral sensibilities. But if we attempt to discover how Diggins would characterize Fascism, we find little more than an identification of Fascism with "totalitarianism" (pp. 25, 190, 197, 251, 334, 435, 456, 464, 466, 467, 487). Beyond that, Fascism was a "right-wing," conservative and reactionary movement that had some "progressive features" and seemed to enjoy a "vast popularity" (pp. 16, 24, 59, 87, 219). It was "right-wing," in Diggins's judgment, at least in part because it exploited "national humiliation—the vital mental germ of almost every right-wing movement" (p. 87). It seems to have been "conservative" and/or "reactionary" because it was "the first anti-democratic political experiment in the twentieth century" (p. 207). Furthermore, Fascism was "the first modern political movement to blare the noise of ideology into concert halls, art galleries, and literary salons" (pp. 240f.).

One can only suggest that none of this is very helpful in trying to characterize the "essence" of Fascism. Not only does "totalitarianism" remain undefined in Diggins's book, but if "national humiliation" serves to spark "right-wing movements," then surely Castro's and Mao's communism seem to qualify as "right-wing." Further, if Diggins suggests that Fas-

cism was the "first anti-democratic political experiment of the twentieth century," he seems to do an historic disservice to Bolshevism. Whatever else it was, Bolshevism doesn't seem to qualify as a "democratic" political experiment. Similarly, to insist that Fascism was the first movement to make art and culture political is to neglect the Bolshevik insistence that all culture serve the Revolution. In any event, it is reasonably clear that Diggins has little to suggest in terms of characterizing the "nature," the "reality" or the "essence" of Fascism. At the end we are left with the commonplace that Fascism is "a state of mind," and that we must "analyze the fascism within ourselves" (p. 495).

Other than these general disabilities, the critical shortcoming that characterizes Diggins's effort turns on his apparent readiness to generalize on the basis of a vague and suspect methodology. Diggins insists upon generalizing about Fascism's "amazing popularity in America" (p. 41; cf. p. xv)—about the "favorable press" Fascism received in America. And yet, on the other hand, he admits that the "American mind" harbored "many-sided and contradictory images" of Fascism and Mussolini. More than that, Diggins is prepared to admit that Fascists felt that a "dreadful misunderstanding" about Fascism was "prevalent" in the United States (cf. pp. 49, 52).

The fact is that there is no public way of choosing between these apparently incompatible generalizations. Diggins admits that he has attempted to generalize on the basis of uncontrolled data, using a methodology that is, at best, obscure. He informs his readers, for example, that he has little confidence in the results of social science survey data and systematic content analysis. Polls, he tells us, provide only "crude opinion surveys" that lack "depth" (cf. pp. xvii, 339). They tend to be "incomplete and unimaginatively formulated . . ." (p. 340). Moreover, "the hard fact . . . is the fetish of the statistician, not the historian" (p. 155). The historian, in his effort to coin generalizations, apparently invests his confidence in something Diggins calls "rough content analysis" (p. xvi).

If all that is true (and it probably is), one can only wonder how, in fact, the historian proceeds to formulate and support generalizations. Diggins recognizes that he has "methodological" problems. He is dealing with that "slippery beast known as public opinion" and the problem of attributing modal values to complex populations and subpopulations. Diggins wishes to transport his readers "to the

heart of American political values and assumptions." He intends to talk significantly about "the American mind," the "public mind," the "popular mind," as well as the "nation's political psyche." He will inform us concerning "the values of America's business culture," the "businessman's creed" and the "liberal mind." He will discuss the collective properties of the "American press," characterize a whole nation's opinions as "America's apologia" for Fascism, and allude to the whole nation's response to Fascism as "America's receptivity" (pp. xvii, 6, 11, 19, 21, 36, 43, 60, 68, 73, 158, 167, 221, 291, 338, 361). Most social scientists would be fascinated to know how all this might be accomplished. If systematic content analysis will not do—can "rough content analysis" succeed? If probability sampling and polling won't do, how does one proceed? If the controlled sampling of "hard data" cannot support inverse projection to total populations, what will?

Diggins has surveyed an uncontrolled sample of an indeterminate body of surviving literature, using vague and unspecified criteria of selection and reporting, and has delivered his personal judgments not only about complex constructs like the "national political psyche" and the "public mind," but has also ventured on omnibus explanation. He can catalogue, for example, the "psychic [and 'desperate'] needs of the American people." He informs us that Americans are "fascinated by unsavory creatures," that they "have respect for power," that Americans have "nativist instincts" and that the admiration for the "hero of iron" is "rooted deep . . . in the American value system" (pp. 64, 65, 69, 71). We are told, furthermore, that Americans were "titillated" by Mussolini's amorous exploits because an entire generation had been "weaned, as it was, on Freud and gin" (p. 63). Diggins's book is simply fat with such "information."

In effect, there is little in Diggins's book that could pass as warranted social science generalizations or cognitively supported explanation. At its best (in chapter 17), the author reviews with insight and considerable critical skill, the major efforts at interpreting Fascism—and finds them wanting. Which suggests that Diggins might have made a better case if he had insisted that Americans have been, and are, much confused about what to make of Fascism.

The best that could be said, on the basis of what Diggins has told us, is that "American opinion was hopelessly complex and painfully divided" (p. 322; cf. pp. 184–185, 199, 201–202, 205–207, 228, 233)—which seems plau-

sible if unexciting. Diggins provides a convenient repository of vaguely characterized, but suggestive, descriptive claims about various sorts of published opinions attributable to various ill-defined populations. He has reviewed a wide range of literature and given his judgments about its content. Beyond that the book cannot pretend to tell us anything about the "American political psyche" or the "heart of American political values" that reasonably sophisticated folk-wisdom could not tell us with as much warrant. Still less can Diggins's book tell us anything about the "essence," the "reality," or the "nature" of Fascism.

Among the volumes devoted to treatments of special aspects of Fascism,⁴ Roland Sarti's *Fascism and the Industrial Leadership in Italy* is quite another matter. Sarti's book is an effort to explore, descriptively and without pretension, the relationship between the Fascist political elite and the industrial leadership of Italy (defined in terms of the relatively small group of leading personalities who held office or were active in Italy's two most influential business pressure groups, the *Confederazione Generale dell'Industria Italiana* and the *Associazione fra le Società Italiane per Azioni*). By comparing the convictions of the leaders of *Confindustria* and the *Associazione*, as represented in their published writings and private correspondence,

⁴There are any number of books devoted to special aspects of the Fascist Regime. Among the best are F. W. Deakin's *The Brutal Friendship* (New York: Harper & Row, 1962), which deals with the final years of the Fascist Regime, and Alberto Aquarone's *L'organizzazione dello stato totalitario* (Turin: Einaudi, 1965) which deals with the organizational infrastructure of the "totalitarian" state. Several reasonably good books on Fascist diplomacy have been written from a variety of points of view. That of Giorgio Rumi, *Alle origini della politica estera fascista* (Bari: Laterza, 1968) deals with the period from 1918 through 1923 and exploits archival material hitherto inaccessible. Luigi Villari's *Italian Foreign Policy under Mussolini* (New York: Devin-Adair, 1956) should be read, if only because it is philofascistic. Giorgio Perich's *Mussolini nei Balcani* (Milan: Longanesi, 1966) and Francesco Jacomoni di San Savino's *La politica dell'Italia in Albania* (Rocca San Casciano: Cappelli, 1965) are specialized histories of Fascist relations with the Balkan countries. Carlo Silvestri's *Mussolini, Graziani e l'antifascismo* (Milan: Longanesi, 1949) is a specialized history of various aspects of Mussolini's Social Republic. It generated a storm of controversy at its publication and merits reading. There are a number of histories and documentaries devoted to the relationship between Fascism and the Italian press. Among the more interesting is the volume by Alfredo Signoretti, *"La Stampa" in camicia nera* (Rome: Volpe, 1968) and the anthology edited by Oreste Del Buono, *Eia, Eia, Eia, Alalà! La stampa italiana sotto il fascismo* (Milan: Feltrinella, 1971).

Sarti provides considerable insight into their political and economic aspirations. By comparing their overt and parochial intentions with manifest Fascist practice we can begin to make some substantive judgment about the relationship between a reasonably well-characterized body of men that constituted a significant part of the industrial leadership of Italy and the political behavior of Fascist Italy. The book provides a body of reasonably hard data against which the merit of generalizations entertained by any number of social scientists can be effectively gauged.

The range of Sarti's interests permits him to confine his claims to manageable dimensions. Some of his broader judgments, however, are sufficiently important or arresting to have merited more elaborate development and more systematic support. He ascribes, for example, Italy's postwar "economic miracle," at least in considerable part, to policies undertaken by the Fascist Regime (pp. 122-3, something suggested by Ludovico Garruccio⁵ as well). Sarti also insists that through the agencies of the Fascist *Istituto per la Ricostruzione Industriale* "the government [exercised] a control over the economy that was unequalled outside the Soviet Union" (p. 124)—a judgment that merits more extended discussion. Moreover, some indication of the effects of overall Fascist economic policy might have been surveyed, since there is very little in English that assists even specialists in making an appraisal. But granting that it might have done more, Sarti's book is a model instance of competent contemporary historiography that recommends itself to anyone seriously interested in the Fascist phenomenon.

General Histories and the Understanding of Fascism

Accounts that are less specific and more broadly historical in character tend to be more self-consciously "theoretical" and more specifically "causal." F. L. Carsten's *The Rise of Fascism*, as a case in point and at its very beginning, provides what Carsten takes to be a partial catalogue of empirical factors that created the "preconditions for the development of ['fascist'] movements . . ." (pp. 9-10). One such factor, for example, was the prevalence of insecure lower middle classes, threatened by both the process of concentration in industry and trade on the one hand, and the growth, organization and restiveness of the working classes on the other. All fascist movements, in some sense,

were to profit from such circumstances. Their membership was to be obtained by mobilizing the "uprooted," those whose "traditional ways of life" had been "eroded" (pp. 56, 10). The postwar crisis and economic dislocation, the insistent fear of communist revolution, and the weakness and ineffectiveness of parliamentary government all conjoined to produce fascist victory (p. 78). Aided and abetted by the connivance or the apathy of the government, the constabulary and the military, the fascists acceded to power.

Such generalizations constitute the explanatory core of Carsten's account of the rise of generic fascism. They are all stenographic and synoptic renderings of generalizations produced in the social science literature by practitioners like Robert Michels, Emil Lederer, and Heinrich Bennecke and were commonplace by the time Talcott Parsons wrote his brief account of the "sociological aspects" of fascist movements in the 'forties.⁶ They have regularly appeared in discursive and historical treatments ever since.⁷

The difficulty with such generalizations, even when they issue from social science tracts, is that one is really at a loss to specify how they might be confirmed or disconfirmed. To say, for example, the "lower middle classes" have been "threatened" and rendered "insecure" by threats of revolution, or as a consequence of industrial developments, is intuitively plausible. Empirical evidence of felt insecurity could probably be collected without too much difficulty. But even granting that, one hasn't the faintest notion *how much* threat and *how much* insecurity is necessary to produce even a low-order probability concerning fascist recruitment successes. In order to be successful, "fascism" apparently requires a host of vaguely specified preconditions: a threatened lower middle class

⁶ Cf. Robert Michels, *Sozialismus und Faschismus in Italien*, 2 volumes (Munich: Meyer & Jensen, 1925); Emil Lederer, *The State of the Masses* (New York: Norton, 1940); Heinrich Bennecke, *Wirtschaftliche Depression und politischer Radikalismus* (Vienna: Olzog, 1968); Talcott Parsons, "Some Sociological Aspects of Fascist Movements," *Essays in Sociological Theory*, revised edition (New York: Free Press, 1959).

⁷ These kinds of formulations are found sprinkled through works of the following kind: Jose Ortega y Gasset, *The Revolt of the Masses* (New York: Norton, 1932); Hans Kohn, *Political Ideologies of the Twentieth Century* (New York: Harper, 1966); Angelo Tasca, *Nascita e avvento del fascismo* (Rome: La nuova Italia, 1950); Hannah Arendt, *The Origins of Totalitarianism* (New York: Harcourt, Brace, 1951); George D. H. Cole, *Socialism and Fascism, 1931-1939* (New York: St. Martin's, 1960) and even Eric Hoffer, *The True Believer* (New York: Harper & Row, 1951).

⁵ Ludovico Garruccio, *L'industrializzazione tra nazionalismo e rivoluzione* (Bologna: Mulino, 1969), pp. 139-45.

(or classes), displaced masses, destabilized youth, protracted economic dislocation, a military catastrophe or a national humiliation of some significant magnitude, and the availability of leaders gifted with some special personality traits.

One doesn't have to be particularly clever to realize that such a catalogue of preconditions seems to identify the circumstances antecedent to almost *any* modern revolution. Moreover, neither threshold levels, indices, nor specific quantitative measures are suggested that might characterize any of these necessary or contingent conditions. Furthermore, we aren't given much of a clue about how inclusive or restrictive the category "lower middle class" is to be.

Historical accounts like Carsten's are best conceived as repositories of selectively collected and reasonably well attested factual data which might suggest possible empirical research. Each historian will collect, store and recount such data on the basis of his own often vaguely characterized sorting criteria. Fortunately, each historian has not only a task differing in range and focus from those chosen by other historians, but brings different sorting criteria to his enterprise. Social scientists can sometimes use the data provided as tests of reasonably specific empirical generalizations or as the basis of possible verificational studies. But to serve in such capacity an historical work must be reasonably specialized (like that of Sarti). Nonspecialized books like Carsten's can only serve such ends moderately well. Carsten's discussion of Italian Fascism, for example, is far too general to be mustered to such tasks. He is much less general, and consequently at his best, when he discusses National Socialism. Here, as distinct from his discussion of Italian Fascism, his touch is sure. He is dealing largely with primary source material.

Carsten's book can best be conceived as a pedagogical tool, a synoptic and interesting discussion of a broad and relatively shapeless topic. That Carsten imagined that such a book might have "explained how it was possible that [fascist] movements not only developed, but were able to become mass movements and to seize power" (p. 8) can be most charitably attributed to an excess of enthusiasm. The book is an interesting discussion of a wide variety of "fascist" movements, but it affords little in the way of serious *explanation*. Still less, unfortunately, does it provide a handle on why such movements should all be characterized as "fascist."

In his last chapter Carsten provides what he takes to be a criterial definition of "fascism." A

"fascist" movement, for Carsten, is one which is (1) strongly nationalist, (2) anticommunist and anti-Marxist, (3) antiliberal and antidemocratic, (4) characterized by a hierarchically organized one-party state that is "strongly elitist in character," (5) possessed of an ideology that includes a "powerful myth," a "myth" that glorifies the past, and/or the "Leader" who is "venerated like a Saint by the faithful who could do no wrong and must not be criticized, who was God-given, and to whom superhuman qualities [are] attributed." The "Leader" is conceived to be possessed of a "personal dynamism and magnetism which could arouse vast crowds to frenzy and ecstasy" (p. 231). "Fascist" movements are further characterized, according to Carsten, by (6) a "cult of violence and 'action.'" They also (7) "appealed to all social groups" except those identified as "the profiteers, the parasites, the financial gangsters, the ruling cliques, the rapacious capitalists," and "the reactionary landowners" (p. 232).

One need not be particularly astute to recognize that such a criterial characterization describes not only "fascist" movements but also most "African Socialist" movements and most "revolutionary movements" in underdeveloped countries, as well as some self-characterized "Communist" regimes. Now that the Soviet Union has decided that Maoism is a "petty bourgeois" form of "anti-Marxist adventurism," it is difficult to know, for instance, how Carsten's characterization of "fascism" would exclude Maoism.

Theoretical Approaches to the Understanding of Fascism

Our more contemporary historians have become increasingly circumspect in claiming to "understand or explain" fascism. They have become, moreover, increasingly aware of the methodological obligations they assume in pursuing their craft. The books by Gilbert Allardyce and H. Roderick Kedward constitute evidence of that circumspection and that insistent sense of obligation. Allardyce, for example, is quick to admit that "few scholars are convinced that they fully understand [fascism]" (p. 1). Kedward similarly insists that "the history of fascism is still in its infancy" (p. v). Allardyce admits that historians use the generic term "fascism"—but he also recognizes that they have not agreed on how the term might be defined (p. 6). He then proceeds to arrest our attention with theoretical snippets from the interpretive works of R. Palme Dutt, Erich Fromm, Peter Drucker, Carl J. Friedrich, Hannah Arendt, Eugen Weber, Seymour Martin Lipset, John

Weiss, Barrington Moore, Ernst Nolte and Wolfgang Sauer. Not only has a reasonably precise definition of "fascism" eluded us—but we are provided with *eleven* explanatory theories, each promising purchase on "understanding." We have an embarrassment of riches. Kedward chooses to reduce the abundance of eleven candidate "theories" to three or four "approaches." Fortunately, there is a convenient overlap between the eleven "theories" of Allardyce and the three "approaches" of Kedward.

Allardyce reviews very briefly and competently the range of "classical" interpretations, despite his seemingly irrepressible disposition to find political motives behind every candidate interpretation. He does seem to emphasize unnecessarily the "moral dimensions" of "understanding" (as do Diggins and Carsten)—as though fascism were somehow more subject to moral assessment than, for example, Stalinism might be. It would be difficult to make a case for Mussolini's Fascism being more barbarous and more murderous than Stalinism—or Maoism or Castroism, for that matter. Furthermore, if as Allardyce suggests, National Socialism was an "aberrant" form of fascism (p. 5), then there is no reason to believe that the proper understanding of *even* generic fascism requires more "moral sensitivity" than the proper understanding of Stalinism, Maoism or Castroism. It would seem that we could proceed with our business with no more, nor no less, "moral sensitivity" in the case of Italian Fascism or generic Fascism than would be the case in our treatment of Stalinism, Maoism, Castroism, Nkrumahism or Nasserism. We can inspect the suggested interpretation of fascism armed with the same measure of detachment or involvement, the same intrinsic norms of our discipline, as we bring to the study of any complex political phenomenon.

Kedward's book, like Carsten's is a handy pedagogical aid that provides a brief account of a number of fascist movements. Contemporary students are not likely to know anything about fascism and Kedward's book is perhaps less freighted with errors than most. Which is not to say that it is afflicted with none. There are a few silly errors: on page 52 Kedward soberly tells his readers, for instance, that Hitler had brown eyes! Some of Kedward's judgments suggest that his treatment of Fascism is more than a little tendentious. We are told, for example, that "Mussolini's regime" failed "to bring industrial greatness to Italy" (p. 111). If we recognize the very narrow industrial base with which Italy entered the twentieth century, and

the peninsula's almost total lack of the most basic essentials for industrial growth—fossil fuels and iron ore—Italy *did*, in fact, make significant strides toward industrial maturity during the Fascist period.⁸ Every index of economic growth we have devised—total volume of output, industrial output, output per man, output per man-hour, and overall rates of capital for-

⁸ In this regard, cf. William Welk, *Fascist Economic Policy* (Cambridge: Harvard University, 1938); Antonio Benni, "Lo sviluppo industriale dell'Italia fascista," in *Lo Stato Mussoliniano*, ed. Tomaso Sillani (Rome: La Rassegna Italiana, 1930), updated as "The Industrial Development of Fascist Italy," in *What is Fascism and Why?*, ed. Tomaso Sillani (New York: Macmillan, 1931). The most readily available comparable aggregate statistics on Fascist economic development are to be found in Angus Maddison, *Economic Growth in the West* (New York: Twentieth Century Fund, 1964). There is no single comprehensive and competent account of Fascist economic policy and achievement. Of the many discursive treatments of Fascist economic policies, almost all are totally inadequate for anyone attempting to assess overall economic performance. Even the treatment by an internationally recognized economist, Paul Einzig (*The Economic Foundations of Fascism* [New York: Macmillan, 1933]), is loosely argued and almost totally innocent of any statistical analysis, and contemporary books like that of A. F. K. Organisky, *The Stages of Political Development* (New York: Knopf, 1965), devoted to the analysis of Fascism as an economic system, fail to cite any substantive data on industrial growth, diversification, or sectoral change. Henry Ashby Turner's recent essay on "Fascism and Modernization" (*World Politics*, 24 [July, 1972], 547-564), in which he attempts to make a case for an interpretation of "fascism" as an "antimodern utopianism," fails to include any data at all on rates of industrialization in Fascist Italy. Fascist publications provided an abundance of raw and presumably standardized data on the economic aspects of the Fascist Regime in publications like *Critica Fascista* and *Era Fascista*. An English edition of *Era Fascista* was published annually by the Fascist Confederation of Industrialists, but literally no effort has been made to sort out of that material a body of defensible statistical data that might provide insight into Fascist economic policy, its accomplishment or failure of accomplishment. To date we must remain content with general and fragmentary accounts like that to be found in Rosario Romeo, *Breve storia dell'industria italiana* (Rocca San Casciano: Cappelli, 1963) or tendentious and incomplete analyses of aspects of Fascist economic policy such as that found in Carl T. Schmidt's *Plough and the Sword* (New York: Columbia University, 1938). Brief accounts like that of S. Lombardini, "Italian Fascism and the Economy," in *The Nature of Fascism*, ed. S. J. Woolf (New York: Random House, 1969) and Shepard B. Clough, *The Economic History of Modern Italy* (New York: Columbia University, 1964), pp. 222-287, are suggestive, but hardly adequate. The most significant gap in the literature devoted to Mussolini's Fascism is to be found in the general area of economic analysis. Whatever evidence we *do* have seems clearly to indicate that Italy proceeded to economic maturity during the Fascist period. (In this regard cf. Garruccio, pp. 50-53, 140-144).

mation—indicates that Fascist Italy went through sustained and substantial growth. Whether Fascism or any other system could ever bring “industrial greatness” to as implausible a place as Italy is a moot question. But it does seem to have produced sustained and significant rates of industrial and overall economic growth in Italy.

Whatever remaining flaws mar Kedward's account, they are by and large compensated for by his book's merits and by his concluding effort to provide a preliminary definition of “fascism” by rejecting the commonly conceived unilinear relationship between “fascism” on the “right” and “communism” on the “left”—and suggesting instead that the two “extremist” movements share a curvilinear relationship which makes them both “converge at many points” and renders them “indistinguishable” in some respects (p. 241).⁹ Kedward does insist that “fascism” and “communism” are in some other senses “quite distinct,” but he does not pursue the distinctions with any vigor.

Kedward's book, like that of Allardyce, is a good introduction to the undergraduate study of fascism. Both books—Kedward in interpretive summary and Allardyce with representative selections provide a brief introduction to the candidate “theories” of fascism that are now standard fare for aspiring social scientists.

Special Theories of Fascism: Neofreudianism

Fortunately, editors seem prepared to publish more substantial theoretical works for advanced students and professionals, and within the last few years we have seen the reissuance of two “classic” attempts at the interpretation of fascism as well as a new book which provides a contemporary version of a “Marxist” assessment of the phenomenon. The books by

Wilhelm Reich and Peter Drucker originally appeared about thirty years ago.

The original German edition of Reich's *The Mass Psychology of Fascism* was incredibly bad when it first appeared, and irrespective of its continued popularity, it has not aged well. Today it reads like a caricature of speculative theorizing. Someone has even succeeded in making an unimaginably bad book even worse by preceding the text with a glossary of terms that provides the reader with stenographic definitions of such significant conceptual terms as “orgone energy,” “vegetotherapy,” and “biopathy.” “Orgone energy,” we are told (p. xxx), is the “Primordial Cosmic Energy . . . discovered by Wilhelm Reich between 1936 and 1940.” “Vegetotherapy,” in turn, is concerned with the “discovery of the muscular armor” which “binds up” “vegetative energies.” “Vegetotherapy” concerns itself with the “concentration of atmospheric orgone energy” through the agency of an “orgone energy accumulator” (federal agents of the Food and Drug Administration please make note). “Biopathy,” in turn, is a “disorder resulting from the disturbance of biological pulsation in the total organism” (p. xxix). Armed with this kind of conceptual apparatus we are presumably prepared to understand fascism.

“Fascism” we are surprised to discover, is nothing more than “the organized political expression of the structure of the average man's character” (p. xiii). “It is the mechanistic-mystical character of modern man,” we are told, “that produces fascist parties . . .” (p. xiii). Since the *average* man's character produces fascism, it would seem that fascism should be a *universal phenomenon*. And that seems to be what Reich suggests. “[Fascism] could have developed in any country or nation. It is not a character trait that is confined specifically to the Germans or Italians. It is manifest in *every single individual in the world*” (p. 320, emphasis supplied).

The reason “every single individual in the world” is so seriously flawed is explained by “orgone biophysics.” It seems that the “average man” and “every single individual in the world” has become “plasmatically rigid” by having “killed” his “genital function” (p. 341). “Man's biopathic character structure” is the product of “four to six thousand years of authoritarian mechanistic civilization” (p. 322). Man has become “biologically rigid,” his “genital musculature” characteristically in a “spastic condition” (p. 151), because of the “ossification of the human plasma” (p. 344) brought about by preventing the individual from achiev-

⁹ The literature that identifies the sustained similarities between “fascist” and “Marxist” political systems, particularly in terms of “totalitarianism,” is abundant; cf. Carl Friedrich and Zbigniew Brzezinski, *Totalitarian Dictatorship and Autocracy* (New York: Praeger, 1956); Dante Germino, *The Italian Fascist Party in Power* (Minneapolis: University of Minnesota Press, 1959); Elie Halevy, *The Era of Tyrannies* (New York: Doubleday, 1965); David Ingersoll, *Communism, Fascism and Democracy* (Columbus, Ohio: Merrill, 1971); N. Kogan, “Discussion—Fascism and the Polity,” in S. J. Woolf, *The Nature of Fascism*; Mihail Manoilescu, *Die einzige Partei* (Berlin: Stellberg, 1941); Mary Matossian, “Ideologies of Delayed Industrialization,” in *Political Change in Underdeveloped Countries*, ed. John H. Kautsky (New York: Wiley, 1962); Ludwig von Mises, *Planned Chaos* (New York: Foundation for Economic Education, 1947); A. J. Gregor, “Classical Marxism and the Totalitarian Ethic,” *Journal of Value Inquiry*, 2 (Spring, 1968) 58–72, and *The Fascist Persuasion in Radical Politics* (Princeton: Princeton University Press, forthcoming).

ing "natural [sexual] gratification" (p. 31). The inhibition of the child's natural sexuality succeeds in the "damming up of sexual energy" (p. 89). These inhibitions began with the decline of the "matriarchal work democracy" which existed (by virtue of Reich's ethnological speculations) four or six thousand years ago—and with the advent of the "authoritarian patriarchal family" (pp. 138, 320, 322, 341). As a consequence, "all forms of fascistic, imperialistic, and dictatorial mysticism can be traced back to the mystical distortion of the vegetative sensations of life, a distortion that results from a patriarchal and authoritarian organization of the family and the state" (p. 136).

In order to "understand" fascism we must pay the exorbitant price of accepting Reich's bizarre speculations about the "damming up" of "cosmic orgone energy" that has afflicted mankind for "thousands of years" (cf. p. 357). Our inquiry began with an effort to understand the fascism that came to Germany and Italy—and now we discover that fascism is the product of disabilities suffered universally by mankind for "thousands of years." As though all that were not enough, Reich insists that "twentieth-century fascism"—for all its universality and millenarian roots—is "something completely new" (p. xxi). All of which cannot help but leave us somewhat confused.

Reich's book is only one of many written by psychoanalytic enthusiasts. It happens to be the worst. Peter Nathan's *Psychology of Fascism*¹⁰ is a close runner-up. Hard behind that is R. Osborn's *The Psychology of Reaction*.¹¹ Erich Fromm's *Escape from Freedom*¹² (a fragment of which is contained in the Allardyce collection) is considerably better than most, but it is flawed by the same disabilities that seem to afflict this entire effort at interpretation.

Only if we grant that Reich or Fromm and their confreres have divined some special research method that permits them to generalize over whole populations on the basis of clinical studies undertaken with self-selected neurotics can we begin to generate propositions about the "human basis" of fascism. But even granting all that, we are left with serious puzzlements. According to Reich, "Marxist" revolutionaries in the Soviet Union can produce the perfect analogue of fascism (pp. 226, 236, 281, chap. 10). So it would seem that the sexual repression that originates in the "authoritarian family" consti-

tutes the *necessary* and *sufficient* conditions for the advent of fascism. Fascism is a movement, then, that presumably springs from the conditions of the "authoritarian family"—a family which is, in the judgment of both Reich and Fromm, characteristically "middle class." Unhappily, all the serious research conducted after the Second World War failed to produce any compelling evidence of an unambiguous relationship between the middle class, the "authoritarian" family, and "fascist" or "protofascist" disposition.¹³

The speculative theorizing of psychoanalytically disposed authors cannot serve as even a partial interpretation of fascism. What such accounts do document, interestingly enough, are the shared similarities that overtly characterize "fascist" and "communist" political systems. Reich, Fromm, and Nathan all allude to the common property traits displayed by such authoritarian, hierarchically organized, one-party systems under the personalist dominance of a "charismatic" leader.

Special Theories of Fascism:

Fascism as the Consequence of Moral Crisis

Peter Drucker, in his *End of Economic Man*, came to essentially the same insight. He concludes that "Soviet Russia is as fascist a state as Germany . . ." (p. 244). He arrives at that conclusion however, via a different theoretical itinerary.

Drucker's book is, of course, a far more substantial work than any in the psychoanalytic tradition. His chapter on fascist "noneconomic policy" still remains one of the best brief expositions of "fascist" and/or "totalitarian" economics: a policy of controlled consumption in the service of massive capital accumulation—an accumulation which provides the fixed capital assets of an expanding industrial system. Calling this perfectly comprehensible policy of rapid capital accumulation a "non-economic" strategy seems to be the consequence of Drucker's commitment to some special convictions

¹³ After the Second World War a group of social scientists attempted to provide empirical evidence of the existence of, and the factors that produced, the "authoritarian personality"; cf. T. W. Adorno et al., *The Authoritarian Personality* (New York: Harper, 1950). The entire work was subjected to sustained and telling criticism in *Studies in the Scope and Method of "The Authoritarian Personality,"* ed. R. Christie and Marie Jahoda (New York: Free Press, 1954). Roger Brown provides a convenient review and criticism in his *Social Psychology* (New York: Free Press, 1965), particularly page 523. John Kirscht and Ronald C. Dillehay provide a sustained criticism in their *Dimensions of Authoritarianism: A Review of Research and Theory* (Lexington: University of Kentucky, 1967) particularly pp. 134f.

¹⁰ Peter Nathan, *The Psychology of Fascism* (London: Faber, 1943).

¹¹ R. Osborn, *The Psychology of Reaction* (London: Gollancz, 1938).

¹² Erich Fromm, *Escape from Freedom* (New York: Holt, Rinehart, and Winston, 1941).

about economic systems. For Drucker, an economic system should be geared to the *maximization of consumption*, the generation of *increased and immediate individual welfare benefits*. Anything else, it would seem, would be "non-economic."

Because of this and a number of similar convictions, Drucker insists that "fascist" (and by implication, "socialist") systems do not "justify" themselves as "benefiting their subjects" (p. 14). Any political mobilization based on noneconomic satisfactions—"moral" rather than "material" rewards—would thus be *ipso facto*, "unjustified" and "unjustifiable."

Armed apparently with such a notion, Drucker proceeds to argue that "fascists" refused to "justify" their right to rule and that they appealed instead to the simple principle that "power is its own justification." Now, a veritable library of books is easily available that provides the fascist rationale for the exercise of specifically political power.¹⁴ Drucker would be hard pressed to find the insistence that "power is its own justification" in any of them. Drucker may dismiss fascist (or for that matter "socialist") arguments as rationalizations, but he cannot pretend that fascists never attempted nor advanced an argument calculated to justify their political tenure (pp. 11–12). Worse than the insistence that fascists made no argument to license their right to exercise political power is Drucker's insistence that the "masses," because afflicted with "black despair," did not even demand that an argument be made.

Drucker insists that his book is calculated to deliver "an explanation and interpretation of totalitarianism which is valid and adequate" (p. xxvii). He also contends that he will accomplish that end by discussing political, social and economic matters. But what he conceives to be the *principal* cause of fascism becomes immediately evident. When we talk of "fundamental changes within the basis of social organization,"

he informs us, we "must" address ourselves to "a revolution of man's concept of his own nature, of the nature of his society, and of his own function and place in this society" (p. xxviii). Drucker is very specific: "... the true cause, the only possible cause, of a revolution is a fundamental and radical change in the order of values, especially in the most important sphere, man's conception of his own nature and his place in the universe and in society" (p. 10).

Drucker seems convinced that the ideas men entertain explain, in a direct and forthright manner, their collective political behavior. Thus, he imagines that "it is the very essence of Europe that it conceives man as free and equal" (p. 50). Because of the traumatic insecurities that characterize the twentieth century, the "masses" have lost "faith" in the "two cornerstones" of the traditional order: "freedom" and "equality" (p. 58). As a consequence of that loss of faith, the "masses" abandoned themselves to fascist (and socialist) demagogues. Thus, in the last analysis, according to Drucker, fascism is the product of a general loss of faith in "freedom" and "equality."

At this point one can only suggest that not only has there not been an "essential European idea" of "freedom" and "equality"—there have been as many "European" notions of "freedom" and "equality" as there have been "European" political thinkers—but the notion that social and political change is the consequence of the taking up or abandoning of "ideas" by the "masses" is simplistic and implausible on its face.¹⁵

Drucker is astute enough to recognize that appeals to "national character" to explain social change constitute the "last resort of baffled historians unwilling to admit their inability to explain puzzling events . . . The national character of every modern people is so complex, seemingly so contradictory and so largely determined by intangibles, that almost anything can

¹⁴ Fascist arguments are found in a wide variety of places. The most compelling are those of Giovanni Gentile, *The Genesis and Structure of Society* (Urbana: University of Illinois, 1960) and "Origini e dottrina del fascismo," in *Il fascismo*, ed. Costanzo Casucci (Bologna: Mulino, 1961); Sergio Panunzio, *Teoria generale dello stato fascista* (Rome: Cedam, 1939); Carlo Costamagna, *Dottrina del fascismo* (Turin: UTET, 1940); Antonio Canepa, *Sistema di dottrina del fascismo*, 3 volumes (Rome: Formiggin, 1937). There were efforts to vindicate Fascism's claim to political power as early as the organization of the Fascist movement; cf. Sergio Panunzio, *Stato nazionale e sindacati* (Milan: Imperia, 1924), *Lo stato fascista* (Rocca San Casciano: Cappelli, 1925); Balbino Giuliano, *L'esperienza politica dell'Italia* (Florence: Vallecchi, 1924); Dino Grandi, *Giovani* (Bologna: Zanichelli, 1941. [A collection of Grandi's essays from the period 1913 through 1920]).

¹⁵ Drucker's notions are shared by Benedetto Croce; cf. "Chi è 'fascista'?" in Croce, *Pagine politiche* (Bari: Laterza, 1945), and "Il fascismo come pericolo mondiale," and "La libertà italiana nella libertà del mondo," in Croce, *Per la nuova vita dell'Italia* (Naples: Ricciardi, 1924). The same ideas resurface in a number of places where an explanation of Fascism is sought in terms of the "corruption of the Idea of Liberty"; cf. Luigi Eiraudi, *Il buongoverno* (Bari: Laterza, 1954) and von Mises, *Planned Chaos*. The entire notion that "masses" act because their "ideas" or "conceptions" are "corrupted" is not only tendentious but scientifically primitive. In this regard see Reinhard Kuehn's discussion of Ernst Nolte's *The Three Faces of Fascism* (New York: Holt, Rinehart & Winston, 1966) in "Problem einer Theorie ueber den Internationalen Faschismus," *Politische Vierteljahresschrift* 11 (November, 1970) 318–341.

be read into it" (pp. 113f.). I would suggest that the attempt to attribute the possession of one or another collection of "motivating ideas" to the European "masses" is beset by the same disabilities. The psychological traits, the "value systems" and "ideas" of the "masses," are so complex, seemingly so contradictory, and so largely determined by intangibles, that almost anything can be read into them.

Interestingly enough, Drucker admits that fascists insisted that they had "succeeded in discovering the secret of true freedom" (p. 80). Fascism also seems, according to Drucker, to have succeeded in going a "long way toward creating a genuine feeling of social equality among the lower classes" (p. 139). He also grants that fascism reduced unemployment, kept and expanded productive plant, and provided security where security had been wanting. All of which suggests that fascists, however much given to hyperbole they might have been, *did* advance arguments about "freedom" and *did* attempt to produce some kind of "equality"—and also succeeded in resolving *some* practical problems. Which suggests, in turn, that in order to explain the successes of fascist recruitment, one doesn't have to postulate a universal "mass despair" or an abandonment of the "European" ideas of "freedom" and "equality." People *may* have been attracted to fascism because its conception of "freedom" and "equality" made more sense, under the circumstances that obtained in Italy in 1922 and Germany in 1933, than the "standard European liberal" conceptions.¹⁶ Now that a host of political systems in the world have opted for notions of "freedom" and "equality" manifestly different from those which Drucker (and others like Croce and von Mises) conceive to be the only ones admissible—it seems clear that one cannot confidently explain fascism in terms of the "abandonment" or "corruption" of the "idea" of "freedom" or "liberty." And if one does not have to argue that men choose fascism because they abandoned "freedom," then one is not driven to explain such "abandonment" by postulating a universal "black despair" that would drive the "masses" to seek "sorcerers," "magicians," and "miracles."

Whatever evidence we have from the fascist experience in Italy indicates that Fascist success was not the product exclusively or primarily of "mass despair" or the "corruption" of the "idea

of liberty," but was at least in part a function of a conflict of very specific, proximate, and material group interests. The newly propertied small landholders of the Po valley, the large landholders, the technical and humanistic intelligentsia aspiring for employment in a restricted job market, threatened and insecure businessmen, war veterans, the constabulary and the military harrassed by anti-establishmentarian socialists, all found something in Fascism that appealed to their real, immediate, and material group interests.¹⁷

Fascism's fairly explicit political program—the maximization of national production through class collaboration of juridically recognized "productive categories"—was apparently no less rational than the political programs of its competitors. Finally, the maximization of national production would provide Italy and Italians with "freedom" against the "plutocratic imperialisms" of foreign powers—and the juridical recognition of productive categories would provide Italians with "equality."¹⁸

Special Theories of Fascism: Neo-Marxism

All of which sounds as though the interpretation offered by "Marxists" might be considerably closer to the mark.¹⁹ Such interpretations of

¹⁶ The literature tracing the appeals of Fascism for various classes and segments of the population includes a wide variety of sources. For a discussion of Fascist appeals to large and small landholders, particularly in the Po valley, cf. Mario Missiroli, *Il fascismo e il colpo di stato dell'Ottobre, 1922* (Rocca San Casciano: Cappelli, 1966) and Giuseppe De Falco, "Il fascismo milizia di classe," in *Il fascismo e i partiti politici italiani*, ed. Renzo De Felice (Rocca San Casciano: Cappelli, 1966). For an account of Fascist appeals to declassed intellectuals and the technical and humanistic petty bourgeoisie, cf. Michels, *Sozialismus und faschismus*, pp. 253–323; Giovanni Ansaldo, "Il fascismo e la piccola borghesia tecnica," in Casucci's *Il fascismo*; and Luigi Salvatorelli, *Nazionalfascismo* (Turin: Gobetti, 1923). Renzo De Felice provides an extended account of Fascist appeals to business interests, veterans, the military and the constabulary; cf. *Mussolini il rivoluzionario*, particularly chaps. 12 and 14, and *Mussolini il fascista: la conquista del potere*, particularly chaps. 1, 3 and 4.

¹⁷ Cf. Dino Grandi, "Le origini e la missione del fascismo," in De Felice, *Il fascismo e i partiti politici italiani*; and Ugo Spirito, "Il corporativismo come liberalismo assoluto e socialismo assoluto," in Casucci.

¹⁸ The literature on the Marxist interpretation of Fascism is abundant. There is, of course, no single Marxist interpretation. One version is that of orthodox Marxism-Leninism standardized by the Third International which includes among its major expressions the work of Guilio Aquila, "Il fascismo italiano," in De Felice, *Il fascismo e i partiti politici italiani*; Clara Zetkin, "Der Kampf gegen den Faschismus," in *Theorien ueber den Faschismus*, ed. Ernst Nolte (Berlin: Kiepenheuer & Witsch, 1967); Palmiro Togliatti, "A proposito del fascismo," in Casucci. R. Palme Dutt's *Fascism and Social Revolution* (New York: International Press, 1934) constitutes an effort to standardize and formalize

¹⁹ For the Fascist treatment of the notions of "liberty" and "equality," cf. Gentile, *Genesis and Structure of Society*; Oscar Di Giamberardino, *L'individuo nell'etica fascista* (Florence: Vallecchi, 1940); Gerardo Pannese, *L'etica del fascismo* (Rome: Voce, 1942); G. Silvano Spinetti, *Fascismo e libertà* (Padua: CEDAM, 1941).

fascism have always turned on the "material interests" or organized and unorganized economic groups, classes or fragments of classes, and their complex interrelationships, rather than on the taking up or abandonment of "ideas" by the faceless "masses." Allardyce, in his collection of readings, provides a selection from R. Palme Dutt's "classic" Marxist interpretation of fascism, *Fascism and Social Revolution*.

Actually, the work of Palme Dutt and the synoptic characterization of "Marxist" explanation given by Kedward represent only the "orthodox-Marxist" interpretation produced by the Third International in the mid-'thirties. Since that time even Soviet writers have themselves objected to such efforts as "stereotypic" and inadequate.²⁰ That such accounts were stereotypic and inadequate is about the *best* that can be said for them. Both Iring Fetscher and Renzo De Felice have weighted the "orthodox" interpretation and found it more than wanting.²¹ But in point of fact there are *many* "Marxist" interpretations—and the "unorthodox" efforts are considerably better than those

that have become "classic." Few accounts of the historic character of Italian Fascism written during the nineteen-'thirties, for example, are much better than that provided by Franz Borkenau.²² But more than that, the contemporary "orthodox" Soviet interpretation is an account far different from that of the Third International.²³

It is at this point that Reinhard Kuehn's new book on fascism, *Formen buergerlicher Herrschaft* has special significance. For Kuehn's book is a non-Soviet expression of the "Contemporary Soviet Standard Version" of fascism. Published about a year after the brief account given by Alexander Galkin²⁴ of the Soviet Academy of Sciences, Kuehn's book is its faithful German rendering.

Where Palme Dutt could argue that fascism was nothing more than "the most typical expression of modern capitalist policy," Galkin insists that fascism was a "crisis regime" that could not be conceived as "necessary" to capitalism.²⁵ Kuehn (p. 160) makes the same point.

For Palme Dutt fascism was the "instrument of capitalism," the "weapon of finance capital"—and was "maintained in funds and finally placed in power" by the "big-landlord, the industrialists and the big financiers"—to become the "terrorist dictatorship of big capital."²⁶ Galkin, in the more modern "Marxist" interpretation, conceives fascism to have been an *independent* mass movement which, because of the heterogeneity of interests that distinguished the groups that lent it qualified support, could act as an "independent force" with "definite autonomy." Using that autonomy, fascism went so far as to "infringe" on the "concrete interests" of "separate representatives and entire factions" of the "bourgeoisie."²⁷ Kuehn makes the same points. Fascism was a "spontaneous" mass movement which exploited crisis conditions in Italy and Germany (p. 103). "Fascism did not serve capitalists as its lackey [*als blosser Buettel*]." Fascism was *not* capitalism's "in-

the "orthodox Marxist-Leninist" account. John Cammett, "Communist Theories of Fascism, 1920-1935," *Science and Society*, 31 (Spring, 1967) 149-163, provides a reasonably competent survey of this version. S. M. Slobodskoi's *Storia del fascismo* (Palermo: Riu-niti, 1962) is a trivial postwar restatement of the prewar version. Daniel Guerin, *Fascism and Big Business* (New York: Pioneer, 1939) is a prewar Trotskyist version of the "Marxist" thesis. Leon Trotsky and Antonio Gramsci left (for the purposes of analysis) only fragmentary comments on the fascist phenomenon; cf. Leon Trotsky, *Fascism: What it is—How to Fight it* (New York: Pioneer, 1944) and *The Struggle Against Fascism in Germany* (New York: Pathfinder, 1971); Antonio Gramsci, *Socialismo e fascismo: L'Ordine nuovo 1921-1922* (Turin: Einaudi, 1967). For the "non-orthodox Marxist" assessments of Fascism, the views of August Thalheimer ("Ueber den Faschismus"), Arthur Rosenberg ("Der Faschismus als Massenbewegung"), Herbert Marcuse ("Der Kampf gegen den Liberalismus in der totalitaeren Staatsauffassung") and Otto Bauer ("Der Faschismus"), are available in *Faschismus und Kapitalismus*, ed. Wolfgang Abendroth (Vienna: Europa Verlag, 1967). Ernesto Rossi's *Padroni del vapore e fascismo* (Bari: Laterza, 1966) is an "independent Marxist" account. Perhaps the most astonishing "Marxist" interpretation of Mussolini's Fascism is that provided recently by Mihaly Vajda of the Hungarian Academy of Sciences, "The Rise of Fascism in Italy and Germany," *Telos*, 12 (Summer, 1972), 32-6, in which Fascism is conceived to have been the "only progressive solution" to Italy's retarded industrial development!

²⁰ Boris R. Lopukhov, "Il problema del fascismo italiano negli scritti di autori sovietici," *Studi Storici*, 6 (1965) 240-255.

²¹ Iring Fetscher, "Faschismus und Nationalsozialismus: Zur Kritik des Sowjetmarxistischen Faschismusbegriffs," *Politische Vierteljahresschrift*, 3 (March, 1962) 42-63; Renzo De Felice, *Le interpretazioni del fascismo* (Bari: Laterza, 1969).

²² Franz Borkenau, "Zur Soziologie des Faschismus," in Nolte, *Theorien ueber den Faschismus*.

²³ Resolution zum Referat des Genossen Dimitrow, angenommen vom VII Kongress der Kommunistischen Internationale, in *Utopie und Mythos der Weltrevolution*, ed. Theo Pirker (Munich: Taschenbuch Verlag, 1964), pp. 226-229.

²⁴ Alexander Galkin, "Capitalist Society and Fascism," *Social Science, Soviet Academy of Science*, 2 (1970), 128-138.

²⁵ Palme Dutt, *Fascism and Social Revolution*, pp. 27, 69, 72; Galkin, "Capitalist Society and Fascism," pp. 128f.

²⁶ Palme Dutt, pp. 29, 155, 77, 79f., 89.

²⁷ Galkin, pp. 129f.

strument" (p. 104). The victory of fascism was the consequence of a coalition of convenience, an association of a spontaneous mass movement, supported, with an eye to their own interests, by nonfascist allies (p. 119). But because the interests of its capitalist allies were so "divergent," fascism retained a considerable latitude for autonomous action—not infrequently sacrificing the interests of some or all of its allies (pp. 122–123, 130, 134). Fascism in effect and in substance, was not the "dictatorship of finance-capital," nor was its "leader" finance-capitalism's executive instrument (p. 140).

While for Palme Dutt fascism was simply the capitalist state sanctioned by violence, for Galkin, fascism produced a *new state form* called "state monopoly capitalism" characterized not only by the relative autonomy of the fascist leadership, but by the fact that the state became "the guarantor of economic development" by providing the social overhead for the improvement and expansion of the economic infrastructure, the maintenance of necessary but noncompetitive industries, and in general "spurring up economic activity."²⁸ Not only that, but this new form of state pursued a "home policy" based on "social philanthropy." "The policy of social philanthropy was elevated to a state level."²⁹ Kuehn does not seem prepared to go quite as far as Galkin, but he does admit that fascism did create a state structure that was "qualitatively new" (p. 118). One of its "functions" was to maintain high profit rates (cf. pp. 123–124) which is a left-handed way of saying that the fascist state maximized investment opportunities and effected considerable industrial expansion. Moreover Kuehn admits, with Galkin, that fascism provided for a "degree of vertical social mobility" (p. 131).³⁰ Kuehn also agrees that fascism set the "economy once more in motion" with large-scale public enterprises—perhaps a weak echo of Galkin's "social philanthropy" (p. 123).

What is more interesting about all this is not that the "Marxist" interpretation has significantly altered during the last thirty-five or forty years, nor that Kuehn's interpretation is singularly unoriginal. What is instructive is how unpersuasive the efforts of both Galkin and Kuehn are when they try to force these insights into a more orthodox Marxist framework. Both candidly admit that fascism is very threatening to the traditional elites, wresting from the hands of capitalists the "settlement of

questions which for centuries" had been their prerogative. They both grant that the "bourgeoisie," in "handing over . . . power to the fascists" had, at the same time, to "subordinate" itself to the regime.³¹ Even though the "capitalists" no longer can exercise their prerogatives in decision making and must subordinate themselves to the fascist regime, the regime remains for both Galkin and Kuehn a "form of bourgeois rule!" Even though fascism "ran counter to the intrinsic conservatism of the bourgeoisie," fascism was, nonetheless, "bourgeois."³² Even though Italian Fascism had so much "autonomy" that Paolo Alatri, one of contemporary Italy's Marxist intellectuals, could maintain that Mussolini was the "absolute master of the nation,"³³ Galkin and Kuehn still insist that Italian Fascism somehow served the interests of the "big bourgeoisie." Even though Kuehn carefully documents the use of political coercion and ultimately the use of terror against the "big bourgeoisie" (pp. 141, 147), he still insists that fascism served their "collective interests." With friends like the fascists, capitalism apparently doesn't need enemies.

The rest of Kuehn's discussion is filled with commonplaces. Fascism was an ideology of "terror," it was characterized by an "ideology of community" and "authority." It emphasized the function of the "leader." It suppressed "opposition" ruthlessly. It infused the "masses" with its ideological substance. It gave a "military cast" to its invocations. It dichotomized the world into the "good" and the "bad." It was "nationalistic."

Had Kuehn possessed a modicum of intellectual independence he would have noticed that none of these ideological traits distinguish fascism from contemporary "communist" regimes. Kuehn thus leaves the analysis where it should begin. While the contemporary Marxist interpretation is significantly better than that produced by the Marxist-Leninist orthodoxy during the 'thirties, it is still lamentably short of providing a plausible interpretation of fascism.

For all that, fascism, even in this account, begins to look like what Italian Fascists regularly insisted Italian Fascism was—a mass mobilizing developmental dictatorship under single party auspices. Such a movement invokes whatever mobilizable population elements it finds available, and enters into *contingent* alliances with nonmovement allies to effect its purpose.

³¹ Galkin, p. 130.

³² Galkin, p. 130.

³³ Paolo Alatri, *Le origini del fascismo* (Novara: Riuniti, 1963), p. 264.

²⁸ Palme Dutt, pp. 182, 193, 131.

²⁹ Galkin, p. 134.

³⁰ Galkin, p. 134.

Such a political system looks neither "capitalistic" nor "bourgeois." One could hardly even characterize it as "conservative" or "reactionary" for that matter, without considerable argument. In fact such a movement looks surprisingly like the mass mobilizing developmental dictatorships to which Mary Matossian and Robert Tucker allude.³⁴

Conclusion

One really doesn't know what to make of all this.³⁵ Contemporary social science clearly has not provided us with a clear definition of fascism. All too many historians have not been able effectively to coin or support generalizations that hold their narratives together. More theoretically oriented social scientists have produced many "theories" of fascism that seem to be clearly defective in particulars, and only partially plausible in general. Some things, on the other hand, seem reasonably clear. Biographies are, in general, not very helpful in coming to understand complex political sequences—unless those biographies are more than that—unless they constitute detailed histories of the entire sequence. Most biographies are far too schematic, and in the case of Mussolini, so burdened by prejudgment that they can ill serve the professional social scientist.

Histories, in turn, tend to be very eclectic. Generalizations are borrowed from social science literature without critical assessment. Even Kuehn, who is more the theoretician than the historian, uncritically invokes Reich's turgid account of "orgasm anxiety" in order to "explain" the "sado-masochistic" character of fascism (pp. 89, 152). In general, historians will invoke generalizations about the "national political psyche," "mass behaviors," "insecurities," "despair," "nihilistic impulses," and "irrationality" with considerable abandon in the effort to provide a plausible account of obscure sequences.

Finally, the more specifically theoretical works, more pretentious in their efforts to "explain" and "understand," vary greatly in quality. Those devoted to a psychoanalytic "theory" of fascism are particularly insubstantial. Curiously enough, in the collection we have under our immediate inspection, the neo-Marxist in-

terpretation of Kuehn, suitably qualified, seems to be the most plausible. Of the snippets in the Allardyce collection the selection from Friedrich and Brzezinski (pp. 67–85)³⁶ is per-

³⁴ Friedrich and Brzezinski's critical characterization of "totalitarianism" in *Totalitarian Dictatorship and Autocracy* is not, as both Friedrich and Brzezinski clearly understood, an *explanatory theory* of either fascism or totalitarianism (pp. vii, 7). What they offered was a *syndromatic and critical definition* of a class of political systems called "totalitarianism." The concept was to serve as a mnemonic repository of "reasonably well-established matters of fact" as well as pedagogical aid of some consequence (p. vii). That they permitted themselves to talk of their work in terms of "models" and a "theory" is unfortunate. It conveyed the impression that they were attempting to *explain* and *predict* (although they were careful to call their account a "descriptive theory"). Most of the criticisms of "totalitarianism" that have become so popular of late turn on this misapprehension. Alexander Groth ("The 'isms' in Totalitarianism," *American Political Science Review*, 58 [December, 1964] 888–901); William Ebenstein ("The Study of Totalitarianism," *World Politics*, 10 [January, 1958] 274–288); Herbert Spiro (Totalitarianism, in *International Encyclopedia of the Social Sciences*, ed. David Sills [Volume 16. New York: Macmillan, 1968]); Michael Curtis ("Retreat from Totalitarianism," in Carl Friedrich, Michael Curtis and Benjamin Barber, *Totalitarianism in Perspective: Three Views* [New York: Praeger, 1969]); Otto Stammer ("Aspekte der Totalitarismusforschung," in *Wege der Totalitarismusforschung*, ed. Bruno Seidel and Siegfried Jenkner [Darmstadt: Wissenschaftliche Buchgesellschaft, 1968]), and Peter Christian Ludz ("Entwurf einer soziologischen Theorie totalitärer Verfassungen," in Seidel and Jenkner) all concern themselves with the putative explanatory and predictive pretensions of "the theory of totalitarianism." There is, in fact, no "theory" of totalitarianism—and such criticism is misplaced (the discussion found in Frederic J. Fleron, Jr., "Soviet Area Studies and the Social Sciences," in *Soviet Studies*, 19 [January, 1968] 313–339 is about the best). Similarly, the more contemporary criticism of the notion of totalitarianism which conceives it as the product of the "ideological requirements" of the Cold War period (cf. Herbert Spiro and Benjamin Barber, "Counter-Ideological Uses of 'Totalitarianism,'" *Politics and Society*, 1 [November, 1970] 3–21, and Benjamin Barber, "Conceptual Foundations and Totalitarianism," in Friedrich et al.) is equally unpersuasive. The fact is that the concept "totalitarianism" i.e., the suggestion that fascist and revolutionary "Marxist" political movements shared sustained similarities, dates from at least 1922 (cf. for example, Rodolfo Mondolfo, "Il fascismo in Italia," in De Felice, *Il fascismo e i partiti politici italiani*; Panunzio, *Che cos'è il fascismo*; and Giuseppe Prezzolini, "Ideologia e sentimento," in De Felice, *Il fascismo e i partiti politici italiani*), and is hardly the consequence of Cold War ideological bias. The similarities were recognized throughout the 'thirties and 'forties by such notables as Trotsky (*The Revolution Betrayed* [New York: John Day, 1937]), Erich Fromm (*Escape from Freedom*), Bruno Rizzi (*Dove va l'U.R.S.S.?* [Milan: La prora, 1928]), as well as by Fascists themselves (cf. Agostino Nasti, "L'Italia, il bolscevismo, la Russia," *Critica fascista*, 15 [March, 1937] 162–163; Tomaso Napolitano, "Il 'fascismo' di Stalin ovvero l'U.R.S.S. e noi," *Critica fascista*, 15 [October, 1937] 396–398; Berto Ricci, "Il 'fascismo' di Stalin," *Critica fascista*, 15 [July, 1937]

³⁴ Matossian, "Ideologies of Delayed Industrialization"; Robert Tucker, "Towards a Comparative Politics of Movement-Regimes," *American Political Science Review*, 55 (June, 1961) 281–292.

³⁵ Cf. Leonard Shapiro, "What is Fascism?" *New York Review of Books*, 14 (February 12, 1970) 13–15; A. James Gregor, "Fascist Lexicon," *Trans-action*, 8 (May, 1971) 54–58; Gregor, "Totalitarian Age," *Trans-action*, 6 (October, 1969) 56–57.

haps most suggestive, and that of Eugen Weber (pp. 95–109) perhaps the best summary introduction to fascism in English.

Efforts to synthesize the surviving elements of “classical” and more contemporary theories of fascism have made their appearance in the last few years. Some of the more interesting have grown directly out of W. W. Rostow’s “stages of economic growth” thesis.³⁷ They in-

clude at least the works of Barrington Moore and A. F. K. Organsky.³⁸ The best of them all is perhaps that of Ludovico Garruccio,³⁹ but we still find ourselves without a viable and comprehensive theory of fascism. Whatever “understanding” we enjoy is partial—and must be painfully teased out of an inordinate amount of literature.

317–319. Certainly the conception of totalitarianism was not the *product* of the Cold War period. Its *popularity* might well have been. But that information helps us very little in assessing its cognitive merits.

³⁷ Walt W. Rostow, *Politics and the Stages of Growth* (New York: Cambridge University Press, 1971), *The Process of Economic Growth*, second ed. (New York: Norton, 1962), *The Stages of Economic Growth: A*

Non-Communist Manifesto (New York: Cambridge University Press, 1960).

³⁸ Barrington Moore, Jr., *Social Origins of Dictatorship and Democracy* (Boston: Beacon, 1966); Organsky, *The Stages of Political Development*; Organsky, “Fascism and Modernization,” in Woolf, *The Nature of Fascism*.

³⁹ Garruccio, *L’industrializzazione tra nazionalismo e rivoluzione*; Garruccio, “Le tre età del fascismo,” *Il mulino*, 213 (January-February, 1971) 53–73.

BOOK REVIEWS

Choice and the Politics of Allocation, a Developmental Theory. By David E. Apter. (New Haven and London: Yale University Press, 1971. Pp. 212. \$7.95.)

This slender volume, which won the Woodrow Wilson prize in 1972, is vintage Apter: original and suggestive; often dense and opaque; sometimes unnecessarily abstract and polysyllabic; and largely barren of concrete illustrative material. To summarize the volume is impossible. Let me, however, provide a few hints for the casual reader. First read the last chapter, then the penultimate chapter, then dip into the rest, especially the summaries and particularly Chapter 1, as time and inclination permit. More serious students, who should omit nothing, might still profit by beginning in this fashion. In spite of this advice, and at the risk of verging on Apterian paradox, it may be suggested that the book needs not a reviewer's summary, but the author's elaboration. Perhaps Apter's response would be that this book must be considered in the context of his earlier work, and that it is at once a distillation of the essence of that work and a further development of it.

The volume deals with both modernizing (especially late period) and industrialized societies. Both should be concerned with choice; the former with its expansion, the latter with its equitable allocation. As modernization proceeds, the need for coercion increases (even while choice is being expanded); increased coercion means that less information will be available to decision makers; yet an industrialized society requires a maximum of information. How then can the transition be effectuated? Not easily. Political instability is inevitable; violence likely. The path toward a reconciliation system will be marked by embourgeoisement, bureaucratization, radicalization, and, frequently, revolution.

For each of his two types of society, Apter proposes a model constitution, designed not so much to be adhered to in detail as to suggest the directions by which constitution makers should be guided. The "development constitution" stresses efficiency rather than equity. To expand freedom, order must be imposed, even at the risk of considerable "unfreedom." Great reliance is placed upon corporate structures of power. They would form the basis for representation in the populist lower house. Stratification groups, segmentary groups, fused groups, differentiated groups, and functional

status groups all would find place here. An important role would also be played by four "development commissions": "fiscal and economic," "education and manpower," "welfare and social services," and "security and justice." Representatives of these technical planning commissions would comprise an upper house of the legislature. One thinks, of course, of guild socialism and of all the problems to which critics have pointed in that system. Here they would be further complicated by the problem of relating the elected lower house to the non-elective upper house.

The "equity constitution," prescribed for the modern industrialized state, would be designed to aim at equity and the minimization of coercion. It should be democratic, decentralized, and highly participatory. In addition to a lower house that in this case would be constituted along rather traditional lines, the state would have an upper house composed of representatives of sociological and educational elites to serve as some sort of counterpoise to the "populist and interest elites" represented in the lower house. How the members of this body would be chosen, to whom they would be responsible, what would be their power base—these questions are left unanswered. The most important change from more conventional forms of government, we are told,

involves the role of government in the information system. The Civil Service would be enlarged so that it might undertake information screening, processing, and evaluation. The lines tracing the information system would be punctuated by committees on consultation, including resolution committees and joint *ad hoc* committees. Extraordinary sessions of both houses would also be possible, as well as hearings in which members of the planning commissions could present proposals (p. 185).

(Much of this, and perhaps even more of the "development constitution" is reminiscent of Sidney Webb's *Constitution for a Socialist Commonwealth of Great Britain*.)

In addition, the "equity constitution" would include a "political" high court, "whose task would be reconciliation of the normative dialectic" (p. 185). This court would be composed of planners, economists, sociologists, and other socioeconomic technocrats, rather than lawyers. Although its powers are not specified, its purpose would be the promotion of social justice. How this court would be related to the equity house is nowhere elucidated. Nor does

the author attempt to explain how a "court" so constituted, and lacking the guidance and support and legitimacy supplied by a written constitution, a body of precedents, and the mystique of the law could hope to reconcile society's conflicting norms and make judgments that would stick.

Apter's normative position is that of a liberal socialist, although he prefers to consider himself an adherent to "social liberalism." The system he envisages would involve a mixture of public and private ownership "in which many sources of interest mediated through diverse forms of access to participation will be translated into a stream of messages, the decoding of which will define policy needs and obligations, the objects of decision-making" (p. 202). The system as a whole, and planning for it, would be highly decentralized. It is recognized that this goal would not be easily attained, but no hints for its accomplishment are forthcoming. Indeed Apter states at one point that "because of the large number of decisional units working more or less at cross-purposes, it may be difficult or impossible for decisions to be made. The sheer number of representative bodies may confuse the political process." (p. 177).

Indeed Apter is not unaware of many of the difficulties his system would confront, as the following passage indicates;

The greater the degree of development, the wider the range of choice. The wider the range of choice in a system, the greater the degree of normative and structural imbalance, the greater the likelihood of alterations in the equity-allocation relationship and the legitimate authority of government. The greater the disruption in the equity allocation relationship and the reduction of the government's legitimate authority, the weaker the flow of information or the greater the need to apply coercion, the ultimate result being greater uncertainty. The way each political system responds to uncertainty, reaches its ceiling and alters, is our present concern (p. 141).

A final word. Apter's concern with the technical problems of government in dynamic societies, a concern that leads him into questions about the processing of information and the application of coercion and the relation between these two things, is paralleled by his concern for social justice. To too great an extent, in this reviewer's opinion, they remain discrete concerns, not adequately related in an integrated whole. The attempts that are made to relate them, as in the case of the "political" court, serve rather to highlight the difficulties than to assuage doubts. Yet I am loath to close on a

negative note. This book is not only hard reading; it both reflects hard thinking and stimulates hard thinking.

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The Legacy of the German Refugee Intellectuals. Edited by Robert Boyers. (New York: Schocken Books, 1972. Pp. 307. \$10.00.)

This is the reprint of a special issue published by the Journal *Salmagundi* (Skidmore College) in 1969-70. The very title arouses apprehensions. Which among the German refugees were "intellectuals"? There is no "Who's Who in German Emigration." The National Reference Service is quoted as listing 7622 "professional and academic" German emigrants between 1933 and 1945 (p. 47). Were all of them intellectuals, or only political leftists of a special literary caliber? Second, is it at all proper for German refugees—most of the authors are—to write on the relevance of contributions made by their peers to the host countries? Should not that judgment rather be left to the intellectuals of those countries? Would it not be more appropriate for us to testify to the influence America had on our own work?

The fourteen intellectuals honored by individual articles include T. W. Adorno, Hannah Arendt, Ernst Bloch, Bertold Brecht, Hermann Broch, Erich Kahler, Felix Kaufmann, Otto Kirchheimer, Thomas Mann, Karl Mannheim, Herbert Marcuse, Henry Pachter, Max Wertheimer, and even Karl Kraus because he, although no emigrant, "constituted a significant member of the emigree generation." Many others are mentioned incidentally. Yet the selection admittedly remains highly arbitrary.

Nonetheless, under the umbrella of these reservations I still wish to do justice to the book's considerable merits. Henry Pachter's introductory paper is above all a noteworthy, if in parts challengeable study on the experience of being an exile. In contrast to America, France, where he had sought asylum first, never accepted refugees fully as members of her society, he says. To the French authorities the German Left was at least as dangerous as Hitler. Once only did he see a chance of complete commitment, when the popular front emerged in France and Spain. He never felt so close to the masses, never experienced such a singular unity of cause and action. But even that did not bring about the integration he had sought.

In America, Pachter writes, democracy provides a favorable fighting arena for democrats; in Europe it often does this for its enemies. It is wrong to assume that refugees will always be

liberals. He refers to Professors Hayek, Mises, Possony, and Strauss-Hupé for the opposite who would, I believe, call themselves liberal; too. Americans see their own history as a success story, not as a story of conflict and tragedy. "American literature has produced no great tragedies and it does not deal with fate." Some refugees were proud to be citizens of this new Roman Empire, provided they could be "its Athenian teachers" (pp. 45, 46). Exile often destroys talent. There is nothing as inconsequential as emigre *querelles*. "We were writing about ghosts, writing for ghosts and gradually becoming ghosts" (pp. 15-19). Marxists and bohemians have in common that neither can bear the thought of being out of step; artists and exiles share contempt of the settled bourgeois style of life (pp. 24, 25). As regards success and influence, some emigres like Einstein, Koehler, Panofsky, Schumpeter, Freud, or Ernst Cassirer, had been world famous before; others, like Tillich, Fromm, Mannheir, Adorno, Eduard Heimann, rose from local to world fame only through their work in exile (p. 47). But many failed to make the grade. Ticholsky, Hasenclever, Stefan Zweig, Walter Benjamin, Klaus Mann, Ernst Toller took their lives (p. 20). Hollywood's cemeteries, Marcuse has said, are the resting place of many Weimar personalities, including Jessner, the stage director, several great actors and actresses, Heinrich Mann, Franz Werfel, Bruno Frank, Lion Feuchtwanger (p. 51).

Pachter enumerates four fields in which the influx of European scholars has had great effect: logical empiricism, Protestant theology (with and without God), philosophy of history, and history of ideas (p. 48). American mathematics "was never the same after Professor Courant settled in New York University," and American musicology had almost not existed before "we" came. In the social sciences he goes so far as to compare the impact of the European sociologists to the invasion of classical studies by Greek scholars after 1453. In politics European scholars had more to learn here than they could teach (he calls this reviewer's *Political Theory* more American than David Easton's), but this very fact was one of the reasons that they have become so well equipped to explain America to Americans. Pachter celebrates the New School for Social Research as a place of experimentation and complete freedom for teachers and students. He feels tempted to say that "a place so European is possible only in America." In the 'forties and 'fifties students came to the School's Graduate Faculty because "nowhere else could one get genuine Max

Weber, genuine Gestalt, genuine Phenomenology, or as close a reading of Plato as Leo Strauss offered" (pp. 34, 35). For his decision to stay in the United States after the War he gives a slightly escapist explanation. It was not, he writes, a sign of his predilection but rather that he "could be less involved here and need not identify with the burning issues of national politics to the extent one does in Europe" (p. 39).

Like Pachter, Professor George I. Mosse in a paper on the "Heritage of Socialist Humanism" seems to use the term "intellectuals" almost exclusively for political leftists of the nonviolent variety. Most left-wing refugees, he says, opposed the terrorist, totalitarian branch. He reaches the melancholy conclusion that the ideal of social humanism has been shown by recent history not to belong to the mainstream of human history which, he says, has stubbornly refused to follow the guidance of reason as the refugees have understood it. Their socialist humanism has remained a matter of European intellectuals.

Neither does existentialism owe its spread in America to the German refugees, for its reception had preceded their arrival. Thus concludes Professor Walter Kaufmann (Princeton) in his excellent essay on the origin and history of existentialism, a solid examination of the various ways the term has come to be used. He admits, though, that Tillich contributed to the fashion of authors presenting their own views as basically existentialist.

The papers on individual refugees are isolated studies with little interconnection. But most are worthy of praise in themselves as, for instance, Arnheim's fine essay on Max Wertheimer, a well-informed and ably written analysis of Gestalt philosophy in only seven pages, and those on Otto Kirchheimer by John Herz and on Felix Kaufmann by Reuben Abel. The following may be singled out for more expanded remarks.

"Thomas Mann and America" by Professor Hatfield (Harvard) and "Bertold Brecht and America" by Iring Fetcher (Frankfurt), while failing to say a single word on the influence these two men had on America, disclose remarkable parallels in their relations to America. Both had originally thought highly of the U.S. Both became increasingly critical, Mann after Roosevelt's death, Brecht much earlier, after his conversion to Marxism. Roosevelt appeared to Mann as a mythical figure, "almost a god," whose very shrewdness was a virtue since he used it towards meritorious goals. *Joseph in Egypt* is modeled in part after Roosevelt. In a

sense, "Mann was standing before Pharaoh when he visited Roosevelt" (pp. 180, 182). Mann's withdrawal was motivated in part by his growing tendency to mistrust ideas of equality enforced from below. His own ideal was a democracy guided from above, with liberty not suppressed by equalitarian trends. Bert Brecht, by contrast, felt alienated by the "ways and tricks" which served to withhold equality. But he kept being thrilled by the large views, the generosity and urbanity of America (pp. 250, 257). Also, Marx had taught him to differentiate between the people and the social roles they are forced to play in the capitalistic system. "Inconsequential goodness" (*folgenlose Guete*) was no way out. "See to it that when you leave the world, not only had you been good, but you leave a good world" Brecht wrote in *St. Joan of the Stockyards*. If children are to learn how to adjust to society, one should ask first of all: to what kind of society (pp. 263, 266).

Karl Mannheim, who as a youngster emigrated from Hungary to Germany and then, in 1933, on to England, where he died as early as 1947, undoubtedly exercised great influence at home and abroad. But again, Professor Edward Sagarin and his partner Robert J. Kelly say little thereupon. They follow the development of Mannheim's ideas from the early stage when he held that the scholar's dependence on the epoch in which he lives and the circumstances under which he works makes it impossible for him to find absolute truth, to the later stage when he wrote that intellectuals might be able to reach a "detached perspective." He did not exclude the existence of truth as an absolute; he merely questioned the ability to see the truth (p. 279). The authors censure Mannheim's failure to take account of the fact that he himself was committed to an ideology—untotalitarian and democratic in character. They see the solution of this paradox in the postulate that the intellectual's commitment to scientific analysis must take precedence over his commitment to an ideology (p. 283), a point Eduard Spranger and this reviewer too have emphasized (see my *Political Theory*, pp. 298 ff. and 62).

Hermann Broch is praised by Erich Kahler as a great innovator for his use of different narrative styles in describing the various phases of the great crisis of our civilization. Kahler calls Broch's main work, *Der Tod des Vergil*, "one of the greatest though most difficult works of world literature . . . in its entirety unprecedented and original" (p. 187). It describes the last eighteen hours of Vergil when he comes to realize that in the pursuit of artistic perfection he has missed "the principal duty of the human

being, the care of the good of his fellowmen" (p. 189). He even wants to destroy his *Aeneid*, but his friend, Emperor Augustus, wrests the work from him. Georg Steiner adds to this paper a note expressing his high esteem for Erich Kahler.

Ernst Bloch, the philosopher, is presented by Juergen Habermas (Frankfurt), as a "Marxist romantic" in an article which first appeared in Germany in 1960 and is here published in translation. Bloch's chief work, *Das Prinzip Hoffnung*, was written in the U.S., finished in East Germany and published in West Germany. To Bloch hunger rather than libido is the fundamental drive. Hope is man's most indispensable, most redeeming gift. Even the most evanescent wish-thoughts are elements of a great dream of the future. To a question I once asked him about how he would like to see the essence of his teaching described in a pocket-sized encyclopedia, Bloch replied: "S is not yet P," which was to mean that man, the subject, is not yet the predicate of what he potentially is. Habermas—although not immune to criticism of his own style of writing—blames Bloch's mannerism, pleonastic passages, metaphors "reminiscent of Böecklin's paintings," for the handicaps delaying his reception. "There are traces of the *Wandervogel's* juvenile sociology even in the concept of hope" (p. 297).

Herbert Marcuse is dealt with only tangentially by Professor Anthony Wilden (San Diego), in a long paper—highly complex, in parts incomprehensible—which compares certain points in Marcuse's theories with those of Freud. Wilden plans a longer treatment elsewhere.

Professor Hannah Arendt is the only woman honored by a special article, ably written by George McKenna (City College), who is planning a book on her. She has always been notable for her impassioned engagement in current political issues as well as for classical erudition. Her concept of politics is based on the Athenian-Periclean paradigm, interpreted by her as a combination of action and speech. Action without speech is meaningless, speech without action is arid. That men can group together in order to produce change by word and action distinguishes them from animals. Only man can defy nature by starting something new. No man can be considered free unless he participates in the political life, not only once every few years by voting, but also in between. But action must be recorded in order to be remembered. No value relativism should stay the scholar from expressing passionate concern when describing concentration camps. Right, although I would

say the scholar's exact description may be more telling than his emotional outburst.

This synopsis is apt to illustrate the variegated nature of the many legacies left by German refugee intellectuals and the lack of coherence among their messages, a universal crisis in present-day intellectualism. Just for this reason the book, although it does little to bridge the chasms that gape among the ideas of its heroes, deserves to be called important.

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The Costs of Accidents: A Legal and Economic Analysis. By Guido Calabresi (New Haven: Yale University Press, 1970. Pp. 340. \$10.00, cloth; \$4.50, paper.)

In a subfield which seems to thrive on the search for its particular scope and method, public law scholars should find *The Costs of Accidents* especially rewarding. There are two sets of reasons for this. First, this work by Guido Calabresi, a professor at the Yale Law School, provides a splendid introduction to and example of a new and rapidly growing form of sociological jurisprudence, the application of economic models to traditional problems of law. Second, his characterization and examination of accident law should convince even the most die-hard skeptic of the meaninglessness of the distinction between "private" and "public" law.

Abandoning doctrinal analysis for an examination of effects and purposes of the law, Calabresi views the law of accidents as a complicated court-administered means of governing behavior and resource allocation. "Since we are not committed to preserving life at any cost," he argues, "the question is the more complex one of how far we want to go to save lives and reduce accident costs" (p. 18). That is, rather than *one* narrow goal of accident law—the reduction of the number and severity of accidents—there are a number of other sometimes conflicting and competing goals. For example, there are also the goals of compensating the victims, spreading the losses to those most able to pay, reducing the costs of administering accident law, fostering socially desirable activities, and stimulating innovation of safety devices. Any complete system of accident law must consider them all. The common law with its separate doctrinal systems of fault, negligence, and strict liability has for the most part only indirectly and implicitly considered these questions. The aim of Calabresi's book is to

face the choices more directly and provide guidance for legislative modification of these traditional systems.

This analysis fits comfortably within the tradition of sociological jurisprudence in that the author's purpose is to provide a framework for evaluation and a source of guidance for judicial and legislative policy makers. Like sociological jurisprudence and policy science generally, it is an instrumentalist analysis which goes beyond precedent, principle, and doctrine to examine goals, functions, and consequences of the applied doctrines. In this sense it is comparable to Bentham's examination of laws from the perspective for his felicific calculus and to Roscoe Pound's examination of law from an interest-balancing framework. By adopting an economic cost-benefit framework, however, Calabresi offers considerably more sophisticated style of analysis. Unlike the empirical or historical traditions of sociological jurisprudence, the economic framework he uses, with its rational choice/utility-maximizing assumptions, allows him to make *deductions* about behavior and the consequences of rules rather than remaining bound to limited inferences drawn from a limited set of data. For instance he can convincingly predict the probable reactions of the market place to proposals which shift liability from pedestrians to motorists. With this "theoretical freedom" and high level of generality he considers the problems of externalities and free riders, develops an explicitly systemic framework, and examines the frequently ignored problem of transaction costs—all standard features of economic analysis.

On the other hand, whatever evaluative strength there is in modern welfare economics is substantially weakened when he abandons the Pareto optimality criteria and confidently dives into interpersonal comparisons of utility without clearly specifying any other criteria. There is, therefore, nothing overwhelmingly convincing about the evaluative framework, and the result is essentially a cataloguing of the numerous "factors to be considered" in the various accident law policies and proposals. No keys to the identification of optimal combinations of them are offered, although to specify the set of boundaries convincingly is a feat in itself.

Turning to another feature of the book, by focusing on *consequences* of various alternative legal rules and their combinations, Calabresi demonstrates that tort law, and more specifically automobile accident law, clearly involves important collective choices. Various systems

of liability and compensation represent important decisions about the exchange of lives and injuries for benefits, subsidization, income redistribution, and taxation—in short, all the problems associated with public regulation. For example, two of the problems Calabresi examines in great detail are the adequacies of the fault system as a general deterrent to accidents and its adequacies as a means of income redistribution—problems usually associated with criminal sanctions and taxation, not torts. That these types of policies have been formulated by courts (though increasingly by legislatures as well) relying exclusively on private law doctrines and not cost-benefit analysis (though some of the celebrated opinions of Holmes, Cardozo, and Hand approach the latter) does not negate the “publicness” of the results nor weaken the authoritativeness of the allocations.

It is an all-too-natural tendency for political scientists to focus on the “glamorous” battles and conflicts in the political arena. The danger in this focus, however, is that in complex, stable societies, the making of authoritative collective policies is likely to be fragmented, incremental and low in visibility, with one result being that political scientists fail to identify and concern themselves with many important types and sources of public policy. An emphasis on consequences rather than forum or fierceness of conflict would go a long way toward correcting this oversight. While such admonitions have repeatedly been made in efforts to direct attention to the impact of bureaucracy in modern societies, they have less often been made with respect to courts and common-law doctrines. Calabresi's book provides a compelling reason to consider the importance of courts and private law doctrine as important sources of public policies. No doubt the increasing interest in the use of tort law as a means of combating industrial pollution will also further sensitize social scientists to the public regulatory and control functions inherent in traditional “private” law mechanisms.

One last observation. While this book is explicitly evaluative, the public choice perspective does demonstrate the fruitfulness of the use of economic models for the characterization of legal problems. Consequently it also raises the possibility of the use of rational choice models for the development of *explanatory* theories of law and social control. To date only occasional and halting efforts have been made in this direction. Calabresi's book, along with Tullock's recent *Logic of the Law*, should, hopefully, stimulate additional interest in and begin to

point the way for political scientists to undertake this far more interesting pursuit, the development of *explanatory* social theory.

MALCOLM M. FEELEY

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Moralphilosophie und Naturrecht bei Samuel Pufendorf. By Horst Denzer. (Munich: Verlag C. H. Beck, 1972. Pp. 405. No price given.)

How the mighty have fallen! At the time of his death in 1694, Samuel Pufendorf passed for (and perhaps actually was) the most important German political theorist and jurist of the 17th century; he was taken to be the synthesizer of Hobbes and Grotius, of state-sovereignty and “secularized” natural law. Locke, describing useful works on the “rise and extent of political power,” called Pufendorf's chief work, *De Jure Naturae et Gentium*, “the best book of that kind”; Rousseau thought Pufendorf worthy of serious refutation in his *Discourse on Inequality*; and even Diderot's *Encyclopédie* followed *De Jure Naturae et Gentium* almost word for word in its articles on *états composés* and other questions of international relations theory. It is true that the two greatest German philosophers of the 17th and 18th centuries were not so impressed: Leibniz called Pufendorf “a bad lawyer and a worse philosopher,” and Kant characterized him as a “miserable consoler” (mainly because of his “just war” doctrines). But in general Leonard Krieger is right in saying (*The Politics of Discretion: Pufendorf and the Acceptance of Natural Law* [University of Chicago Press, 1965], p. 2) that Pufendorf had “a contemporary influence that was immense and an ultimate impact that was minimal.” Horst Denzer's new study, *Moralphilosophie und Naturrecht bei Samuel Pufendorf*, does not succeed in overturning that judgment.

The decline of Pufendorf's reputation is partly traceable to the difficulty involved in deciding whether his work is genuinely synthetic, or merely eclectic. If one reads the early chapters of his *De Officio Hominis et Civis*—an abridgment of *De Jure Naturae* made by Pufendorf for use in universities—one finds a definition of equity which is extracted from Aristotle's *Ethics*, a theory of sovereignty modeled on Hobbes's, a title borrowed from Cicero, and a doctrine of natural law ostensibly derived from Grotius (one must say “ostensibly” for reasons to be taken up shortly). Sometimes, however, one finds not only an eclecticism which does not quite qualify as synthesis, but actual inadequacy: in both *De Jure Naturae* and *De Officio*

Hominis, for example, Pufendorf claims (apparently with Hobbes) that the authority of law comes simply from "the power of the superior," but also (apparently with Grotius) that such superior power includes *both* "the strength to threaten some evil against those who resist him and just reasons [e.g., gratitude or a contract of obedience] why he can demand that the liberty of our will be limited at his pleasure." As long ago as 1706, Pufendorf's acute critic Leibniz observed in his *Opinion on the Principles of Pufendorf* that "if the source of law is the will of a superior and, inversely, a justifying cause of law is necessary in order to have a superior, a circle is created, than which none was ever more manifest." Krieger, indeed, makes much the same argument against Pufendorf in *The Politics of Discretion*, but traces the circularity of the argument to Pufendorf's "anxiety to adapt principles to practice," to be a *bourgeois* "trimmer" (p. 85), while Leibniz confined himself to charges of mere philosophical incapacity. But Denzer, in his *Moralphilosophie . . . bei Pufendorf*, touches on this point—indeed this *kind* of point—scarcely at all.

Despite his eclecticism and occasional inadequacy, however, Pufendorf is for the most part a solid, reasonable, and very thorough theorist who might well look better if not constantly compared with his greatest contemporaries. He is certainly far superior to every German political theorist of the 17th century with the exception of Leibniz, who was only a political thinker by fits and starts and never wrote a large-scale treatise as able as *De Jure Naturae et Gentium*. And Pufendorf could sometimes be quite eloquent, as some of the passages which Denzer cites show. Moreover, his distinction between *entia moralia* and *entia physica* anticipates Kant.

Like Pufendorf's work itself, Horst Denzer's *Moralphilosophie* is solid and careful and very thorough; it deals in detail with every concept that Pufendorf himself treated—including *entia physica*, *entia moralia*, natural law, the state of nature, the social contract, and sovereignty. If Denzer's book has a defect, it is that of being rather uncritical: to analysis and judgment of Pufendorf's ideas he prefers mere juxtaposition of those ideas with those of Pufendorf's "antecedents"; there is more historical comparison than philosophical evaluation. This is not surprising, given Denzer's stated aim of "placing" Pufendorf in the "field of intellectual and scientific history" (p. 22). But it is probably unjust to Pufendorf himself, who urged in his *Eris Scandica* that, "We should stop asking how

new or old a doctrine is and investigate only how true or false it is."

One could wish that Denzer had heeded this excellent advice, for while his searches for the *Grundlagen* of Pufendorf's doctrines occasionally produce good results, as in the illuminating comparison of Pufendorf and Locke on toleration (pp. 216 ff.), he often falls into unsubtle characterizations of important theorists ("For Aristotle, as for Thomas [Aquinas], the community, the state, is the whole in politics, the individual is only a part; the whole comes before the part"). This is not incorrect, but it is something that anyone likely to be reading Pufendorf need not be told. And sometimes Denzer's method leads him to historical claims which are not just unsubtle or suspect, but actually perverse: Robinson Crusoe, one is told (p. 108) stands for "the rational individual as the starting-point in modern theories of the state."

A more serious problem, however, arises in Denzer's treatment of Grotius as the most important antecedent of Pufendorf. When he urges (p. 264) that Pufendorf's disagreements with Grotius are only "particular," never "fundamental," that they have to do only with the "better interpretation of this or that matter," he is on doubtful ground indeed. Grotius, of course, had become famous by arguing that "just as even God cannot cause that two times two should not make four, so He cannot cause that that which is intrinsically evil be not evil." In Grotian natural law, in short, the validity of moral laws is at least *comparable* to the necessary truths of mathematics. Now Pufendorf—as even Denzer admits (p. 53)—did not really share this view; indeed Denzer, paraphrasing Pufendorf's words, suggests that for him "there is no normative rule which is separate from the will of God. Every objective normative rule is in the end an effect of God's will." How, in view of such a high degree of quasi-Hobbesian voluntarism in Pufendorf's natural law, Denzer can characterize his disagreements with Grotius as "not fundamental," it is not easy to see. Surely the Cambridge Platonists, Leibniz, and Kant are the true heirs of Grotius' view that a moral rule cannot even be *conceived* as the mere effect of the will of a powerful being.

Ultimately Denzer's book must give way to Krieger's, which is better argued and much better written. But Denzer's work has three advantages which deserve mention: (1) his exposition of Pufendorf, if uncritical, is at least not hostile, as Krieger's sometimes is; (2) he quotes at length little-known passages on Pufendorf's methods and motives from the *Eris Scandica*;

(3) some particular parts—above all the one dealing with Weigel as the teacher of both Pufendorf and Leibniz—are good and informative. And Denzer's vast bibliography, which seems to list every edition of Pufendorf ever printed in any language, is certainly good to have.

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Bakunin on Anarchy: Selected Works by the Activist-Founder of World Anarchism. Edited, Translated, and with an Introduction by Sam Dolgoff. (New York: Alfred A. Knopf, 1972. Pp. xxvii, 405, vii. \$10.00.)

Along with Herzen, Chernyshevsky, Lenin and Trotsky, Bakunin was one of the towering figures of the Russian revolutionary movement. Already during his own lifetime he acquired an international reputation as a militant revolutionary and theoretician of anarchism. The political history of his native country since 1917 and, in a larger sense, the development of authoritarian socialism in our century have made his ideas even more relevant than they were during his lifetime. No wonder that in recent years there has been a renaissance of interest in this romantic rebel: we can find echoes of his thought in the writings of Régis Debray, Frantz Fanon, Herbert Marcuse, and Eldridge Cleaver.

Bakunin developed a much more accurate vision of the nature and milieu of modern revolution than any of his contemporaries. Unlike Marx, Bakunin believed in the revolutionary potential of the landless peasantry and the *Lumpenproletariat* and recognized *déclassé* intellectuals as an important revolutionary force. Indeed, he envisaged the possibility of an alliance between the alienated intellectuals—the “dregs” of the middle class as Marx contemptuously called them—and the disinherited masses. From this perspective, the great revolutions of our century—in Russia, Spain, and China—were considerably closer to Bakunin's vision than to that of his great German adversary and antagonist in the International.

Bakunin, however, not only correctly anticipated the character and locale of modern revolution, but also foresaw its authoritarian consequences. As early as 1873, in his critique of the Marxist theory of the state, he predicted the emergence of a “new class” of political rulers, scientific experts, bureaucrats and state engineers who would once again enslave the people and nullify the initial gains of the revolution—all in the name of the “dictatorship of the proletariat.” Bakunin's attack on the centralized bureaucratic state, his contempt for middle-

class conventions, his insistence on “immediate revolution,” his passionate argument for equality and liberty, his emphasis on the “instinctive” as opposed to the rational and scientific, his warning against the evils of a technocracy and the dangers inherent in subjecting human life to the rule of science—all these aspects of his thought make him a highly attractive and relevant thinker for our age.

The present volume, which was edited by Sam Dolgoff, a free-lance writer, labor union propagandist, and lecturer, is not only timely but also fills a major gap in English literature on socialism. Most of the writings included have never before been published in English or have appeared only in disjointed excerpts. Four of the twenty selections are from the pre-anarchist period of Bakunin's life. In addition to excerpts from such major works as *Federalism, Socialism, Anti-Theologism* (1867), *God and the State* (1871), and *Statism and Anarchy* (1873), Dolgoff's anthology contains a number of letters, programmatic statements, and short essays. The volume is arranged chronologically and includes a biographical sketch of Bakunin (1907) and an essay “On Building the New Social Order” (1874) by James Guillaume, a preface by Paul Avrich, a general introduction by the editor, and a useful bibliography.

In the case of Bakunin, the compilation of an anthology is a formidable task. He was not a systematic thinker and left a literary legacy consisting of a “patchwork of fragments,” to use T. G. Masaryk's apt phrase. With the exception of *Statism and Anarchy*, Bakunin did not complete any of his major works, leaving even many short pieces unfinished. Furthermore, most of Bakunin's writings were polemical in nature, strongly reflecting the issues, political events, and circumstances of his time. For this reason it is impossible to read Bakunin's works meaningfully without a rather intimate knowledge of the historical context within which they were written. As in the case of any anthology, one may quarrel with the editor's choice of selections. In general, Dolgoff has exercised good judgment. He is less successful, however, in reconstructing the vital link between the ideas of Bakunin and his time. His introduction, notes, and amplifications, and Guillaume's biographical sketch do not always succeed in providing the reader with sufficient background information to understand and appreciate Bakunin's arguments and ideas.

While some will argue that there are limits to the benefits of neutral scholarship, the editor's introduction is too sympathetic. Dolgoff rightly stresses the libertarian principles expressed in

Bakunin's writings, but he does not really come to grips with the authoritarian implications of Bakunin's mania for secret organizations and conspiratorial revolutionary activity, his insistence on the necessity of an "invisible collective dictatorship" even after the revolution, or his confident prescription for "instant socialism." Moreover, Dolgoff too readily dismisses the arguments of those who have maintained that Bakunin was an authoritarian at heart and in some respects may be regarded as a forerunner of Lenin. In short, the interpretive mold into which Dolgoff seeks to force his subject, it seems to me, is too narrow, too rigid, and too simplistic to convey the full range, complexity, and ambivalence of Bakunin's spirit. In his preface, Professor Avruch is considerably closer to the mark.

Nevertheless, we are indebted to Sam Dolgoff for this anthology. The revival of interest in anarchism both in the United States and abroad and the continued vitality of the libertarian ideal of socialism as an alternative to the authoritarian perversions of that vision in our century suggest that the historical impact of Bakunin's ideas is far from having run its course.

ROLF H. W. THEEN

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Community and Purpose in America: An Analysis of American Political Theory. By Mason Drukman. (New York: McGraw-Hill, 1971. Pp. vii, 428. \$9.95.)

Professor Drukman's traversal of the old ground of "American political theory" is inspired by a purpose, which is to say that this long book has a thesis. In the first chapter, the thesis is stated. America today is without a national purpose. We are a nation "riddled by mistrust and discontent," a nation "in genuine danger of falling into disarray." (On p. 3 Professor Drukman cites two novelists, Clancy Sigal and Norman Mailer, to support these claims.) At one time, America did have a national purpose: "liberty defined as economic individualism." Mr. Drukman's "central thesis" is that this national purpose (which began "to lose currency at the turn of the century" [p. 21]) has worked "to prevent the country from achieving a national sense of community" (p. 6). The failure of American political thought is "to treat seriously the idea of community" (p. 4). Thus "the customary mode of American theory has left us ill-equipped to think effectively about the situation before us *today*" (p. 21).

Of Drukman's two stated reasons for study-

ing American political theory, understanding "the problems that confront us today" (p. vi) is clearly ascendant over the more theoretical activity of treating "it as real theory, material that . . . merits in its own right careful and thoughtful consideration" (p. 17). (Despite the reservations I will state, Mr. Drukman's own book deserves such consideration.) It is in terms of this ascendancy that his choice of American political thinkers can be explained.

There are chapters on Madison and John Adams, Paine and Jefferson, Hamilton, Emerson and Brownson, Calhoun and Fitzhugh, Sumner, Bellamy, Croly, Dewey, and Marcuse and Dahl. What seems to unite this group is that they are white males who thought about politics, hardly enough to define any sort of community. Drukman says that these men were chosen, with few exceptions, "from the mainstream itself." Some stream! Negro thinkers were excluded apparently because they were not "men who have mattered." "Impact of some noticeable significance constitutes the chief criterion for inclusion" (p. 23). "Impact" is obviously not confined to "political actors," although "the most renowned American theorists have in fact been themselves political actors . . ." (p. 15).

I think the author's criteria for inclusion and exclusion, the organization of his book, and the way he treats particular thinkers can be explained by his purpose (*supra*) and by his ideas about the limitations of "political actors" and his characterization of political philosophy.

Those "political actors" whom he does discuss in the first third of the book are, with the exception of Calhoun, founders: the men who shaped the institutions and categories of thought that enthroned "economic individualism" as the national purpose. We learn of the narrowness (and worse) of that purpose and therefore suspect the founders of myopia (if not worse). Being "political actors," these were "men whose ruminations could never stray from the object at hand" (p. 15). Thus, for example, Drukman criticizing Jefferson: "*reality*, with its inescapable set of limitations, incessantly crowded his style . . ." (p. 79); "[h]is ideal conceptions often succumbed to a mentality controlled by the dictates of reality" (p. 96).

If we are to get criticism, obviously we must abandon "political actors." Most of the remainder of the book deals with critics of "economic individualism" (except for the chapters on Sumner and Dahl, who are seen as overt or covert defenders of the old faith). But even these critics are limited; they do not achieve the

realm of political philosophy. Although there is little apparent consistency in Drukman's use of the terms "political thought," "political theory," and "political philosophy," it is clear that political philosophy is at the top and seems minimally to require "a thoroughgoing critique of society, and an understandable set of suggestions for meaningful change" (p. 302). These critics do not achieve such heights. Although they may present "a forceful, sometimes radical, critique of important institutions and procedures," this is usually "followed by a tempering or narrowing of the argument so that in the end it does not break too drastically from accepted norms" (p. 24).

The author who comes closest to Drukman's ideal of political philosophy ("dialectical myth-making," "the heights of imaginative creativity" [p. 15]) is the novelist Edward Bellamy, whose "own account of the workings of his mind is very similar to what I have suggested is the creative process of the best political theorists" (p. 232).

Without claiming that our greatest statesmen ("political actors") were the American equals to Locke, Aristotle, or Rousseau, one can reasonably argue that they are better political teachers than is Edward Bellamy. By seeing political actors as "succumbing" to "the dictates of reality" and then identifying that reality with "economic individualism," Drukman unnecessarily narrows the horizons of our statesmen and underestimates their awareness of the problems involved in conceiving a nation in liberty and dedicating it to the proposition that all men are created equal. And by ignoring Lincoln, he does not see the possible nobility of such a dedication, of such a national purpose.

The American *political* tradition is far richer than the author would have us believe, and, in part, that wealth derives from the confrontation of minds with that disciplining reality which differentiates thought from fantasy. (The essays on American statesmen contained in *American Political Thought*, edited by Frisch and Stevens, demonstrate these contentions.)

Mr. Drukman's reliance on novelists ("imaginative creativity") to delineate our current plight and to suggest, in the case of Bellamy, the model of a political theorist, is less than compelling. The so-called "adversary culture" with its familiar certainties about current political and social reality may have been refuted by America's refusal to obey the call of the pur-

Historians' Fallacies: Toward a Logic of Historical Thought. By David Hackett Fischer. (New York: Harper & Row, 1970. Pp. xxii, 338. \$12.00.)

Professor Fischer's intentions in this book are clear and simple. His concerns are the ways in which historians set about their inquiries, the nature of their explanations of what they have observed, and the manner in which they present their 'arguments'—i.e., their solutions to problems, for "history is . . . a problem-solving discipline" (p. xv). His approach to these matters, however, is via the propensity of historians to fall into error, and in particular into methodological error. Most of his pages are devoted to a demolition job, to pulling down some of the wrong signs (categorized as an infinity of fallacies) which are found along the historian's path. To this catalogue of misdirections he adds "a few crude but hopefully more correct markers" to the true highroad which a properly professional historian should tread. These indicators are not less important because they bulk smaller than the list of historical fallacies.

Professor Fischer illustrates circumstantially the frailties of his profession. Erring colleagues are paraded from all ages and all lands. They are led by Tacitus, whose fallacy was moralistic; but he is followed by Hobbes, who fell into the fallacy of the one-dimensional man, and by Voltaire who succumbed to the fallacy of élitism. Others in the procession are, inevitably, Macaulay and Lytton Strachey; A. J. P. Taylor, "that Paganini of historical prose" (p. 35); and Namier, "a great historian and a gross Tory" (p. 69), qualities which perhaps cancel each other out. As for Professor Fischer's colleagues in his own country, they are compendiously described as "the methodological primitives who inhabit the history departments of the United States" (p. 52). Individuals including Arthur Schlesinger, Jr., and H. S. Commager are treated with varying degrees of severity; Robert W. Fogel, it is suggested, has led "his Cliometrical colleagues down the methodological rat-hole of the metaphysical question" (p. 18); and Gordon C. Bjork is arraigned for devising an index which is "a complicated thing, calculated from three other indices, all of which are incorrect" (p. 127).

In all of this Professor Fischer displays immensely wide reading and a gusto which is immeasurable. At the same time, his pace is so fast that he is sometimes in danger of falling

well; but are there not traces here of the fallacy of ambiguity, since how do we determine whether the word "enthymeme" is being used in its Ciceronian or in its Aristotelian sense? Further, his canvas is so vast that not all his critical judgments can be fully argued. The consequence may be something we might describe as the fallacy of the counterassertion. There is something of this in his arraignment of Bjork quoted above; and the "false and misleading" character (p. 83) of Eric Williams's interpretation of the history of Trinidad and Tobago is indicated rather than demonstrated. This fallacy has the further disadvantage in that it may prevent our asking whether there is anything or nothing of value in the treatment of this history. Perhaps, too, we may detect a fallacy of excessive minimization, as when Marxist-Leninist history is dismissed in 49 words (p. 174); or a fallacy of understatement, when a passage in the preface of Namier's *Structure of Politics* is translated as a refusal to name any sources since he cannot name them all. In fact Namier's very next paragraph lists the sources upon which he has relied most heavily, and his book is equipped with ample footnotes.

It would, however, be less than fair to leave Professor Fischer's essay on a negative note. It is in fact a good deal more than a rumbustious romp among historians' reputations. It has a serious and positive purpose. First, Professor Fischer is convinced of the utility of historical study and of the writing of history: for history can clarify the context of contemporary problems, indicate some possible lines of development, and refine the theoretical equipment we apply to questions of our own day. Second, precisely for these reasons it is vitally important that historians should perfect their own professional equipment: and his book has been inspired by his conviction that "the trouble with professional historians is that they are not professional enough" (p. 315). This is not to say that he claims too much for history. "The whole truth" is an impossible object; and "progress . . . depends upon a sense of the possible" (consequently he is at one with M. M. Postan in rejecting as impracticable the way of "metahistorians" like Toynbee). In its own way, nevertheless, history belongs with the useful arts, "aducing answers to specific questions" by rational and logical processes. If Professor Fischer has made us more self-conscious about the ways in which we set about that task, and still more if he has successfully blocked off some short-cuts which turn out to be blind alleys, his purpose

will have been achieved and history will be the gainer.

EDWARD MILLER

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Capitalism and Social Theory: An Analysis of the Writings of Marx, Durkheim, and Max Weber. By Anthony Giddens. (Cambridge, England: Cambridge University Press, 1971. Pp. 261. \$11.00.)

This book is an important contribution to a growing literature on some of the classics of the main sociological tradition. Instead of emphasizing the duality of the sociological tradition—as Alvin Gouldner does—with one major branch stemming from Marx and the other from Durkheim and Weber, Mr. Giddens stresses the common features of their diagnoses and conceptualizations. I think it is fair to say that the author emphasizes the centrality of Marx, although he does not take a specific position in favor of any of the three writers and declares his intention to be more expository than critical.

As an expository work, it has many merits. I found the author's discussion of Marx's early writings, especially about the genesis of the conceptions of alienation and exploitation, very illuminating. There is less which is relatively new in his expositions of Durkheim and Weber, but these are, on the whole, specific judgments, such as that stated in the preface that Durkheim was not primarily concerned with the problem of order but with a succession of orders, which is, I think, inadequately clarified. The two statements are not necessarily incompatible, but I would contest the first proposition that Durkheim was not in any serious sense concerned with *the* problems of order.

My main line of criticism, however, relates to a lack of clarity in the nature of the task which Mr. Giddens has undertaken. There seem to be three primary components in his discussion which should be more clearly distinguished from each other than they are. These are first, a history of sociological or social theory, including a discussion of the relevant empirical problems and data. The second is a sociology of knowledge which attempts to use analysis of the social situation in which the three authors lived in order to throw light on their statements and the genesis of their theoretical views. The third, related to the second, is the development of intellectual guidelines for courses of action, especially political action. This last problem was indeed a central one to all three of the writers. Marx in particular em-

phasized it in the famous phrase, "the unity of theory and praxis." Durkheim was very much concerned with social reform, and on a different level, so was Weber, though the last two, of course, were not revolutionists.

With respect to the relations of the first and second themes, there is no doubt that much "understanding" of an author's ideas can be gained from an analysis of the social, political, and cultural situation of his time. I do not contest for a moment the importance to Marx of what Giddens calls the "backwardness" of Germany, to Durkheim of the difficulties of France following the defeat of 1870, and to Weber of the nature and international position of Bismarck's and Kaiser Wilhelm's *Reich*.

My plea, however, would be for according considerably sharper attention to the development of theoretical conceptualization as such. The school of the historical analysis of social thought is similar to the approach in literary criticism so sharply criticized by Northrop Frye—an approach in which personalities and social situations of literary authors are given undue prominence relative to the actual content of their work. (Frye, "The Critical Path: An Essay on the Social Context of Literary Criticism," *Daedalus* [Spring 1970], pp. 268–342.) In my opinion, this tends to blur and relativize the significance of the social thought of major figures. Especially with respect to the relation of both Marx and Weber to the tradition of German thought, Giddens skates on the edge of a "historicist" point of view. This trend was strong in Marx himself, who considered classical—and *pari passim* this would include "neoclassical"—economic theory to be relevant only to the capitalist system. As for Weber, Giddens comes perilously close to Reinhard Bendix's view that Weber's theory was a helpful scaffolding in the construction of historical interpretations rather than theory in its own right (Reinhard Bendix and Guenther Roth, *Scholarship and Partisanship: Essays on Max Weber* [Berkeley, Calif.: University of California Press, 1971]; see also my review of Bendix and Roth in *Contemporary Sociology*, Vol. 1, no. 3 [May, 1972], pp. 200–203). Indeed, Giddens sometimes refers in a skeptical and almost derogatory tone to tendencies in current sociology to attempt to develop generalized theoretical schemes independent of particular historical circumstances. As theory comes to be carefully differentiated from the sociology of knowledge, these tendencies will be increasingly accentuated. Both Durkheim and Weber went much farther than Marx in upholding not

only the desirability, but the feasibility of the development of generalized theory. Such theory built precisely on Marx's dialectical idea of the interplay between subjects and objects and the conception that social systems are essentially consequences of past human actions rather than elements of the world of nature in the physical-biological sense or consequences of a process of divine establishment.

Giddens makes a reasonably cogent case that the conflict between Marx on the one hand, and Durkheim and Weber on the other, over "materialism" is not adequately stated in terms of the inversion of Hegel's idealism. He is quite right that Weber was not a proponent of "determination" by ideas but a proponent of the extremely complex interdependence between ideas and other factors. A sharp difference between Marx and Weber lies in the fact that Marx tended to treat capitalistic society as a monolithic entity which included its political and ideological structure and to consider the political component of the capitalist firm as inseparable from the economic aspects of the firm's internal structure, that is, the capital-labor relation and the firm's involvement in the market nexus; Weber, on the other hand, developed a differentiated analysis of the complex modes of specified interdependence between "ideas" and economic and political structures. This analysis is related to his extensive comparative interests, whereas Marx's treatment of societies other than the capitalistic was, in Giddens's term, "sketchy." Similarly, on the methodological level, by distinguishing the "value free" status of the *cognitive standards* from the values operating in action, notably political action, Weber presented a much more differentiated view than the Marxian unity of theory and practice. Giddens also mentions Durkheim's significant stress on the differentiation in modern society between the state and what he called "society," which I would call the societal community.

The relatively low level of differentiatedness in Marx's theory is related to the sharpness of the dichotomy between capitalism in which men are alienated and that vaguely defined entity "the social character of human existence." Marx built this into a rigid dichotomy in which capitalism was held to be totally devoid of "sociality," if I may use such a term, whereas the ideal state of communism was alleged to be totally devoid of all the characterizing features of capitalism. By contrast, Durkheim and Weber refused this dichotomy. They saw modern society as involving, in different ways, elements of

alienation and exploitation but they did not see it dissociated from what Toennies later conceptualized as *Gemeinschaft*. Both, like Marx, were acutely aware of strains and tensions and personally critical of many features of the society of their time, but both thought an evolutionary change more probable than the Marxian apocalyptic vision of violent revolution.

Both were much concerned with the status of labor in modern society. They laid stress on a development which has occurred since early capitalism and which, without violent revolution, has profoundly changed the state of that area of modern societies most closely related to economic production. I refer to the development of an *occupational* system in which the performance of labor has come to be institutionalized in a highly ramified system of occupational roles, only a minority of which fit the Marxian characterization of labor's status, especially as subject to stringent supervisory authority, to minute fragmentation, and to reduction to minimal levels of monetary remuneration. The institutionalization of managerial functions has developed in an occupational framework quite different from the Marxian "capitalist" stereotype. Another development is probably just as important in the long run, that of the modern professions, which play a crucial part in large-scale organizations and bridge the structural hiatus between private economic and governmental organization and the gap between both and the private, nonprofit sector. While I would be the last to argue that late industrial society is no longer characterized by class differences, those differences surely do not fit the pattern of the Marxian bourgeois-proletariat dichotomy.

Though Marx emphasized technology, he failed to foresee the importance not merely of an altogether new type of technology, namely science-based technology, but also the relation of the emergence of such technology to the development of the system of formal education, and especially higher education and research. Durkheim and Weber, partly because they lived in a later generation, were aware of the importance of these developments, although many contemporary sociologists would emphasize them much more than they did.

Mr. Giddens has written an important book which deserves serious attention among the interested professional groups. It will be a very convenient book for people who would like a secondary introduction to the authors he treats. I would advise the serious student, however, to pay close attention to the original texts. Gid-

dens has made an important contribution to the interpretation of the significance of these major figures, but I think the last word is far from having been said.

TALCOTT PARSONS

Harvard University

A Source Book for the Study of Personality and Politics. Edited by Fred I. Greenstein and Michael Lerner. (Chicago: Markham Publishing Co., 1971. Pp. 572. \$16.50.)

The study of personality and politics has two inherent sources of major interest as well as several significant problems. The interest derives largely from the importance of the individual political figures studied by investigators in this field and also from the potential significance of reliable findings about how the political process is influenced by particular persons. Thus, for example, personality analyses of Woodrow Wilson and Richard Nixon appeal by virtue of what they suggest about underlying determinants of decision making at higher levels of government. Similarly, investigations of the personality characteristics and political actions of liberals or conservatives are potentially important because they suggest that many political decisions flow from deeply rooted personality dispositions, rather than situational considerations.

The field also has limitations that have reduced its influence and raised doubts about those findings that have emerged. Among the factors limiting acceptance of those findings is the absence of psychological theories and research evidence that would provide a solid basis for guiding studies, conducting research, and evaluating findings. Psychoanalytic theory previously served this function for many investigators, but that theory no longer commands the loyalty it once did, and no other general theory of personality has replaced it among social scientists. Another problem involves the reliability of present psychometric instruments and procedures. This problem plagued such earlier studies in the area, e.g., the authoritarian personality study, and questions about response sets and defensiveness are far from resolved.

Despite these problems, the study of personality and politics has been growing rapidly in the past decade and has produced increasingly sophisticated research and theory. This work by Fred Greenstein and Michael Lerner provides an excellent indication of the current state of knowledge in this field. In their careful

and scholarly sampling of the major research and theory in personality and politics, the editors present a portrait of a field that is striving for scientific respectability yet is bound to the present-day relatively limited armamentarium of psychology. The weapons in this armory include theories that can clarify murky uses yet are incapable of dealing with the complex interactions involved in political actions; individual studies that are well conceived, soundly executed and truly illuminating, but generally noncumulative. While the editors declare that the study of personality and politics is a lusty young infant, my reading left me with the feeling that the pregnancy is still in progress. I also had the impression that the interface between political science and psychology has serious limitations. At the interface with sociology, such concepts as orientation, style, and type are comfortably employed, and the complexities of personal exchange and interacting forces can be dealt with descriptively and without apology for lack of measurement data. In a certain sense, there is a sad but realistic recognition that present-day psychological theory and assessment is incapable of delivering the theory, concepts, and data that Greenstein and his coworkers so urgently require to attain their vision of a scientifically based field. Wisdom may require that the vision be examined to determine how it relates to the present, near future or long term future.

While it is possible to quibble with some of the choices and exclusions of the editors, there is little doubt that these twenty-eight articles represent significant research in the area and deal with the fundamental issues of the field. The articles are divided into four sections containing between four and ten articles each. The first section contains four papers dealing with general theoretical and methodological statements. The paper by Greenstein presents an initial overview of the field and, together with the paper by M. Brewster Smith, examines some of the major issues that underlie the complex interactions involved in personality and politics research. There follow two well-developed papers attacking simplistic efforts to reduce all political behavior to the psychological level (Bendix) and employ the concept of role without due appreciation of social forces (Levinson). The omissions of this section are as notable as the inclusions, for it contains no treatment of measurement problems, only passing treatment of research strategy, and little discussion of present limitations of this area of research and their implications.

The second section comprises five articles ex-

amining individual political actors including and applying depth psychological categories and procedures to the lives of Woodrow Wilson (Alexander George, Edwin Weinstein), Gandhi (Erik Erikson), the political attitudes of fathers and sons (Lane), and the resolution of decisional conflict (Janis). The hitherto unpublished essay by George provides a sound introduction to psychobiography, examines some of the deficiencies that occur in such works, and then considers how the approach can be productively employed by examining the personality of Woodrow Wilson. (This essay draws heavily on the booklength biography of Wilson written by George and J. L. George.)

Section Three presents ten papers dealing with personality analyses of types of political actors. These include noteworthy papers relating personality and political orientation (Di Palma and McClosky), overview and critique of the authoritarian type (Sanford, Shils), student protesters (Flacks), and presidential character and style (Barber). This section also includes classic papers on the functional approach to the study of attitudes (Katz), and the logic of political personality typologies (Lasswell). Section Four examines the effects of personality on the functioning of political institutions. Other articles present general conceptualizations of the individual and sociocultural system (Inkeles and Levinson), the impact of the leader's personality (Browning and Jacob), while others illustrate how survey (Converse and Dupeux), clinical (Osgood), and model-building (Greenstein) procedures can aid in the analysis of political actors. All in all, a nourishing and generally satisfying smorgasbord.

This work clearly attains its goal of providing a basic grounding on the subject for graduate students, advanced undergraduates, and scholars in other fields. While it complements Greenstein's book *Personality and Politics*, this collection can easily stand on its own in the quality and scope of its coverage. Although this collection reveals that the field is growing and improving, it appears presumptuous to call this work a "source" book. The word implies that the field has achieved an extensive body of knowledge and that the articles represent the major and definitive theory and research in the area. Both of these positions are open to dispute, and a less pretentious title would have underscored that this is indeed an excellent reader. I concluded that the study of personality and politics is to a considerable extent art as well as science and that pronouncements to the

contrary do not alter that opinion. Conceptual analyses, theoretical maps, and clarifications are sharpening the issues, but the very complexities of the subject matter appear to be well beyond presently available methods of objective assessment. While the editors dwell on the progress that has been attained, they might have devoted more time to assessing the current state of the science/art, and the directions that are likely to prove productive, given present methodological implications.

STANLEY COOPERSMITH

University of California, Davis

Cybernetics, Simulation, and Conflict Resolution: Third Annual Symposium of the American Society for Cybernetics. Edited by Douglas E. Knight, Huntington W. Curtis, and Lawrence J. Fogel. (New York: Spartan Books, 1971. Pp. 249. \$10.00.)

Will the year 2000 mark the beginning of a thousand years of peace—perhaps directed by ultraintelligent machines (UIMs) with unlimited storage capacity? Douglas E. Knight, an editor of this compilation of Proceedings of the Third Annual Symposium of the American Society for Cybernetics, answers with a qualified yes, assuming that the year 2000 will see both improved simulation techniques and a more predictable (i.e., easily simulated) human race.

The symposium's task, in the words of Knight, was to explore whether the simulation laboratory could "offer a testing ground for developing possible alternatives to violence" (p. xv). The sixteen contributors undertake this exploration on four levels, which constitute the four sections of the book: I. A Problem: Campus Confrontation; II. Simulation of the Problem; III. International Conflict Analysis and Simulation; and IV. The Technology: Cybernetics in the Seventies and Beyond.

In his keynote address, Lawrence J. Fogel attributes modern man's hostility to increasing interpersonal competition forced upon him by greater and greater overlap in his goals—the result of increasing worldwide homogeneity. He sees cybernetics (the comparison of automatic control systems in man and machine) as the key to handling conflict, with the papers presented at the symposium spelling out the whys and hows.

In the first article, "Must We Change for Change's Sake?" author Preston Valien concludes that we really must. He sees as the culprit of academic unrest traditional, process-oriented education, in place of an emphasis on human needs. One wonders why Valien does not

suggest that educational institutions teach change—the problems of impermanence and dislocation and how to induce change intelligently. David Gardner continues this line of thought in the next article by suggesting that the university break with past methods in order to defuse conflict.

In one of two articles in Section II, "Simulation of the Problem," Martin Shubik states that the value of simulation lies mainly in the bringing together of people and the mutual understanding which results. In the other article, "Take a Longer Look," Milton U. Clauser examines the possible roles of the university—creating a serene atmosphere or tackling problems head on.

Section III, "International Conflict Analysis and Simulation," brings together a number of interesting approaches. I would recommend highly to students of mathematical escalation John Voevodsky's "Modeling the Dynamics of Warfare." The Guetzkow piece about simulations and international relations may be familiar to some readers but will make important reading to others. It demonstrates that simulation can contribute to conflict reduction by fostering increased knowledge and understanding of other actors and thus decreasing the likelihood of misinterpreting their actions. In addition, simulation can pinpoint (and thereby aid in avoiding) any areas of conflict which might arise from a new policy. In another article, Ivo Feierabend hypothesizes that systemic frustration breeds aggression, which in turn breeds internal violence, which creates a predilection for international hostility. Conflict may be averted by intervening in this chain—i.e., by manipulating the levels of systemic violence.

Section IV, "Technology," deals with the potential feats of computers, including their use in decision making through benefit analysis and related techniques. The Carl Hammer article on interactive electronic systems smacks a bit much of "Star-Trek" reruns, but I. J. Good's extensive piece on computers' future social repercussions is extremely intriguing. The notion that computer-assisted organizations will by necessity move toward greater complexity (his fourth law of thermo-dynamics) bears attention. The nostalgia for von Neumann's lucid and public reporting of reasons for design change is unnerving to those of us who still have not demystified von Neumann.

In general, this volume is useful for understanding the technical innovations in conflict resolution, with emphasis on simulation, events analysis, and information theory and technique. I feel it has ample value for the study of con-

flict dynamics and the growth of conflict because of the variety of concerns it touches upon. There are studies on cross-national conflict, such as the Feierabend piece, as well as domestic (student) conflict, such as a statistical survey by Raymond Tanter. I would add as another note in its favor that the book does not deal analytically with the traditional international conflict issues—military structure and weapons management.

Its main failing is its inability to get to the real problem of conflict resolution, possibly the major ongoing problem which confronts conflict research in Europe and North America: the notion that conflict control equals conflict resolution. This is simply wishful thinking. The bulk of the research in the volume deals with conflict escalation and the mechanisms for increasing conflictive behavior. In short, all the authors are operating on a pre-resolution level, thus making the title a bit misleading.

Equating conflict resolution with the reduction or containment of violence ignores the fact that conflict de-escalation could take place in one dimension—such as reduction of violence and the absence of overt hostile or armed activity—while at the same time, other forms of escalation, such as economic sanctions, could increase.

The writings here also reflect a number of other conceptual problems. All the authors seem to assume, at least implicitly, that symmetry exists in conflictive situations. Partners in a conflict—e.g., students vs. an administration, small nations vs. large nations—may differ on so many dimensions that a thorough knowledge of conflict behavior demands better conceptual tools which can account for these differences. For example, the authors could have differentiated between the parties of a conflict on the basis of structure and organization, value systems, and interactive capacity.

Furthermore, the authors here (like many other social scientists) tend to assume that the only "real" conflict that exists, or at least that we can study, is overt and observable. What about the extremely important sphere of latent conflict? A thorough knowledge of the conditions which can reduce inequalities between national or international actors may help avoid future hostile interactions altogether.

I feel that future thought and research, perhaps by these very authors, will show that conflict resolution requires a decrease to "tolerable" limits of inequalities among the conflicting parties. This might require building those structures which facilitate the removal of

the asymmetries in value systems, conflict behavior norms, and interactional capabilities.

EDWARD E. AZAR

University of North Carolina at Chapel Hill

A Pre-View of Policy Sciences. By Harold D. Lasswell. (New York: American Elsevier Publishing Co., Inc., 1971. Pp. 173. \$7.50.)

In discussing the professional identity of contemporary policy scientists, Professor Lasswell observes:

In achieving the new identity it has been necessary to overcome the image of a second-class man of knowledge and a second-class man of action, and to perceive that the integrative role of the policy scientist is indispensable to the security and advancement of a world civilization of science-based technology (p. 120).

Throughout his career, Harold D. Lasswell has consistently and persistently called us to the larger challenge. In this book he sets comprehensive tasks, refusing to acknowledge our limitations as roadblocks to their accomplishment. If, perchance, we were suddenly equipped to meet Lasswell's challenges I suspect we would either be on that glory road to utopia or the society would come to a dead stop—a victim of decisional sclerosis brought about by too much planning.

The probability of a quantum increase in rational planning and policy development is, I suspect, relatively low. While I would never wish to belittle Harold Lasswell's impact, most of us maintain narrow interests, quite resistant to integrative pressures. Lasswell calls for the kind of comprehensiveness that characterizes his mind and style, but there really is only one Harold D. Lasswell. He is a brilliant man, as is evidenced by this latest book, *A Pre-View of Policy Sciences*. He classifies concepts as though his mind were equipped with multi-powered magnets—pulling units into various categories. He forces himself to be ever more encompassing, bounding ahead but reaching laterally as well. He experiments with integration by intentionally selecting illustrations that are out of the ordinary, that would seldom occur to most of us. And he constantly projects into the future—seeking a return for analysis of the present.

But that is Harold Lasswell. And who are these policy scientists he talks about? Presumably they will, or should, be even more comprehensive and integrative.

The emerging policy scientist in our civilization is not only a professional in the sense that he combines skill with enlightened concern for the ag-

gregate processes and consequences of decision. He belongs among the systematic contextualists who are also empirical. . . . the policy scientist is concerned with mastering the skills appropriate to enlightened decision in the context of public and civic order. . . . [H]e is searching for an optimum synthesis of the diverse skills that contribute to a dependable theory and practice of problem solving in the public interest (pp. 12-13).

One might immediately question whether an emerging class of policy scientists is compatible with democratic public order. That is of less immediate concern to me, however, than who these people will be (or are) and where they will be (or are) located. At the risk of insulting Dr. Kissinger, I am compelled to say that I don't know any such persons presently in decision-making capacities. Lasswell does observe that "not many full-time policy scientists can be identified at present," but I have trouble sensing where to look for them in the future. My own set of concepts permits me to envisage a "policy science," but not acting policy scientists (at least as described by Lasswell). I can understand the policy science role of academic advisers who are broadly trained, as well as of decision makers who are told "be comprehensive." What is never made concrete to me in this book is where I should expect to find these super-integrators in our system of political decision making.

The failure to indicate who these policy scientists will be and what they will do detracts somewhat from this volume, but the many other purposes it serves recommends it to political scientists. As with *Public Policymaking Reexamined* (Yehezkel Dror), this book sets the agenda for comprehensive, integrated decision making. One surely gets a feel for the task at hand as Lasswell outlines how policy scientists should look at the world, how they should pursue their tasks, what they have to offer both the "ordinary policy process" and the "constitutive policy process," how they might gain professional identity, and how they might be trained. Those acquainted with Lasswell's work will find much that is familiar here—though often set in a slightly different context or relied on for different purposes. As with Dror, Lasswell develops a strategy of rational problem solving: goal clarification; trend description; analysis of conditions; projection of developments; and invention, evaluation, and selection of alternatives. And the familiar, though slightly modified, "seven power outcomes" (intelligence, promotion, prescription, invocation, application, termination, appraisal) are invoked throughout. He presents a less-tortured

and more obviously pluralistic means for identifying goals than Dror does—advising the policy scientist to "search for postulates of sufficient generality to provide guidance in coping with the problems that arise in connection with the shaping and sharing of values" (p. 41). Among other services performed, books of this sort illustrate the complexities of achieving an applied social science—of meeting the challenges outlined by Professor David Easton in his 1969 presidential address before the American Political Science Association.

Lasswell's discussions of professional training are also valuable. Those of us interested in training political scientists to engage in socially relevant policy analysis need to review existing graduate programs. We need to find parsimonious means by which students can learn more about substantive issues and gain experience in decision-making processes. The internship has proved of only limited value in this connection. Lasswell proposes a "continuing decision seminar" that has much to recommend it, particularly, I believe, in graduate training. Each such group would be composed of a "small nucleus of self-constituted members" with a "determination to continue."

Like all the other books of Harold Lasswell, this one will find several audiences. The audience he intends is typically in the future—where Lasswell himself has always been most comfortable. Given Lasswell's integrative skills, a present- and past-oriented audience will also find their values and problems analyzed somewhere within these pages. Those who insist on getting the most for their money, however, should be encouraged to test the Lasswellian recipes. Though some are too rich for the common palate, they can be halved or quartered and still satisfy the creative urge of most of us.

CHARLES O. JONES

University of Pittsburgh

The German Historians and England. A Study in Nineteenth-Century Views. By Charles E. McClelland. (Cambridge: Cambridge University Press, 1971. Pp. x, 302. \$16.50.)

There are many ways of learning about the past; one of the best is to observe it through the eyes of the past. The experience is salutary. How subjective, how dominated by the obsessions and the wishful thinking of their generation are even the most scholarly writers; and how unscholarly are some of the most gifted and the most learned. Consider the authorized and the revised versions of the origins of the Cold War. Consider the perennial debate, never

more relevant than in 1972, on the character of American populism: Is it enlightened, or is it obscurantist? The facts, one would have thought, do not change, though new ones may well be discovered. But interpretations, especially influential ones, may in turn become part of the data. Truthful or not, they are truth-creating.

There is no better instance of this process than the varying German interpretations of the English experience between the 1770s and 1914: the development of the professional historian's craft coincided with the period when British government and society were widely regarded, for better or worse, as the world's most advanced. Even by the time of the French Revolution the main lines of German scholarly Anglophilia were visible. They radiated principally from Göttingen, the cradle of German historiography which was the port of entry for British political ideas, and, as professor McClelland might have emphasized more, economic theory and philosophy, too.

There were three principal themes. The first was admiration for British liberties, based partly on swallowing whole the simplicities of Montesquieu and Blackstone, but also on the undeniable freedoms enshrined in *habeas corpus*, trial by jury, a press uncensored at least by German standards, and the self-confidence of propertied classes who had tamed the executive. The second was admiration for British stability and respect for tradition; thus the same aristocratic independence and common law conventions that fired liberal hopes also reassured conservative publicists like Justus Möser. Both schools saw the key to British institutions, and their relevance to Germany, in a shared Anglo-Saxon-Protestant love of liberty.

All these perceptions were reinforced by the wars against Napoleon. Britain was now the defender of Europe's liberties, a role that was read backward, with all the unhistorical enthusiasm of the historicist, to the wars against Maria Theresa, Louis XIV, and Philip II. Professor McClelland writes, "Out of the Anglo-French struggle of these years grew the philosophical, political and ideological connection between Anglophilia and historicism that was to last for many decades into the nineteenth century" (p. 32).

The academic historians of the Restoration period were the principal driving force in German constitutionalism and therefore the chief publicisers of the British example. The majority of them saw in the balanced constitution of eighteenth-century theory the kind of polity they would have liked to see in Germany:

they desired, above all, to check the arbitrary exercise of power by princely bureaucracies, not replace these bureaucracies with a full-powered parliament. Impeachment, free speech and press, and limited ministerial responsibility were to be the weapons of an awakened bourgeois intelligentsia against arbitrary government, and these were best developed in Britain. What they did not see so clearly—the development of parliamentary predominance and government by interest-oriented parties—was something they did not want in Germany (pp. 78–9).

As before, this selectivity allowed moderate conservatives as well as constitutional liberals to hold England up as an example. It meant that Ranke, who towered above his contemporaries in methodological rigor, could be an Anglophile on grounds of English culture, English society and, above all, foreign policy.

Equally fascinating, but more problematic, is the question to which the author devotes his final section. Why should the consensus of historical opinion have moved, in the course of one generation, from Anglophilia to Anglophobia? In part it was, like all historical revisionism, a reaction against accepted oversimplifications. More important, the emergence of united Germany as a great power implied rivalry with Britain rather than dependence on her balancing role. The *Flottenprofessoren* (navy professors) of Tirpitz's propaganda machine—Hintze and Delbrück, Marcks and Schmoller—were a long way from Ranke's Anglo-German "brotherhood in arms," though they should not be thought of as a hate-group with a single party line (pp. 191–213).

German unification under Prussian auspices also made Britain less relevant as a guide. After 1867 it was obvious (never having been probable even before that year) that German constitutional life would not follow the British example. The existence of the new state made foreign examples not merely superfluous, but offensive. For three quarters of a century England had served as a counter-ideal to the baneful influence of the "hereditary enemy," France. Now, as Germany took her own increasingly illiberal path, parliamentary free-trading England in turn became a counter-ideal. In the hands of a sophisticated scholar, like the Prussophile Liberal Rudolf von Gneist, this emancipation from English apron-strings was healthy (pp. 135–44). In the hands of Treitschke it set a propagandistic tone that influenced a whole academic generation. Indeed, the notorious combination of insecurity and aggressive self-assertion that characterised the political debates of Imperial Germany required a

whole gallery of counter-ideals, including the patriotic but liberal Jewish community. McClelland alludes to the alliance of antisemitism and Anglophobia (p. 195), but omits Treitschke's role in this development.

McClelland's net takes in academic historians, constitutional lawyers, intellectual politicians (like E. L. von Gerlach and Lothar Bucher) and freelance publicists of the Right (like Gentz) and the Left (like Marx or Bernstein). He writes with a historian's perspective. The judgments are those of our day, the emphases reflect his subjects' importance in their own time. Treitschke and Ranke therefore rate a chapter each, Engels a few pages. Rather more regrettable is the almost total exclusion of Catholic scholars, even granted that interest in England was commonest among North German Protestants. Acton's friend Döllinger rates a footnote, Adam Müller a few sentences, Görres and Burckhardt are ignored.

Biographical and bibliographical appendices of 55 pages greatly enhance the value of the book, but its chief merit in an age of committed scholars is to submit a particular era of commitment to a critique. As the center of historical studies moved from Hanoverian Göttingen to Prussian Berlin, the scholars' view of the English past reflected their hopes for Germany's future. True, they thus "raised history above the level of dead men and mouldering paper" (p. 236), but they also fell into the trap of "backward projections that resulted in distortions of history which ultimately became policy" (p. 212). Intentionally or not, Mr. McClelland has written a very good tract against tracts.

PETER PULZER

Christ Church, Oxford

Hegel: A Collection of Critical Essays. Edited by Alasdair MacIntyre. Garden City, New York: Doubleday and Company, 1972. Pp. viii, 350. \$250, paper.

The latest addition to the Doubleday series of critical essays on great philosophers is both a credit to the series as a whole and a valuable addition to the rapidly growing literature on Hegel in English. Alasdair MacIntyre has included a variety of intellectual styles and philosophical approaches to Hegel, making for a highly readable volume with something of interest for anyone interested in Hegel, regardless of his approach or sympathies. The tone of the essays is for the most part detached, but sympathetic to the great German thinker. It is a tone admirably suited to help us, in the editor's words, "recover the Hegel of fact from the He-

gels of fiction and . . . recover him as a living philosopher concerned with genuine issues" (p. viii).

The anthology contains four essays not previously published. Prominent among them (and probably the outstanding piece in the collection as a whole) is Charles Taylor's lengthy and insightful discussion of the "epistemological" chapters of Hegel's *Phenomenology of Spirit* (the Introduction and the first three chapters). Taylor combines his clear, analytical turn of thought with a genuine sympathy for Hegel's position, and provides a paradigm of the sort of Hegel scholarship we need now. A second new essay of considerable interest is Klaus Hartmann's "Hegel: A Non-Metaphysical View." Hartmann attempts to interpret Hegel not primarily as a metaphysician speculating about the real as such, but as a philosopher concerned with the formulation of systems of categories in terms of which domains of reality may be "reconstructed" in a manner which is complete, coherent, and satisfying to reason. Hartmann's writing is sometimes difficult, and his philosophical style may make his essay hard going for some Anglo-American readers. But in my judgment the content of the essay justifies the effort. Hartmann's interpretation, however, seems to work more successfully with those works of Hegel which employ a strictly conceptual dialectic (the *Logic*, the *Encyclopedia*, the *Philosophy of Right*) than it does with works aiming at a historical dialectic (the lectures on the history of philosophy and philosophy of history, the *Phenomenology* itself to an extent).

A third new essay in the volume is MacIntyre's own "Hegel on Faces and Skulls," a surprisingly convincing discussion of a section of the *Phenomenology* that is often an embarrassment even to the work's most ardent admirers. MacIntyre argues persuasively that in this section Hegel is defending a conception of the explanation of behavior which constitutes an important challenge to models of explanation based on laws and generalizations. Richard Schacht's "Hegel on Freedom" is probably the weakest of the four new essays, but it too is a helpful discussion of one of the most important and confusing concepts in Hegel's philosophy.

The other seven essays in the anthology have been printed elsewhere. The book opens, after a brief introduction by the editor, with a short, light, and somewhat introductory essay by J. N. Findlay, whose subject is the "mood" or "tone" of Hegel's philosophy as a whole. Findlay portrays the boundless optimism, humanism, and rationalism which form the core of Hegel's outlook. His account makes it clear that Hegel's

proper philosophical antagonist is not at all the scientific humanist who has in fact so often rejected Hegel as an obscurantist. Hegel's proper foe is rather a thinker like Kierkegaard (who understood Hegel much better), a thinker who places little faith in the power of human reason to build a world in harmony with man's nature, and who looks upon all attempts to discover order and reason in the world as so many modes of flight from the absurd predicament of the solitary individual.

One of the finest essays in the volume is R. C. Solomon's discussion of Hegel's concept of *Geist* (Spirit or Mind). Solomon approaches this concept via the Kantian conception of the transcendental ego, and he views Hegel's doctrine of a trans-personal "spirit" as the result of a critique of Kant's "first-person oriented" theory of subjectivity. This essay, along with those of Taylor, Hartmann, and MacIntyre, belongs to the philosophical core of the volume.

George Armstrong Kelly's essay on the lordship-bondage theme in Hegel is an attempt to balance Kojève's "social" interpretation of this motif with an "intra-personal" one. Kelly seems to me to be attacking Kojève just where his position is strongest. Nevertheless, there is something to be said for the manner in which Kelly reads the lordship-bondage motif back into later stages of consciousness in the *Phenomenology*. Michael Kosok's "The Formalization of Hegel's Dialectical Logic" is perhaps as difficult and obscure as its title would suggest, but is nevertheless just the sort of article likely to disappoint expectations. It seems to me a mistake to suppose that Hegelian logic can be treated in a manner parallel to modern mathematical logic; both are indeed approaches to an understanding of the nature and procedures of rational thinking. But apart from this, it seems to me they have very little in common, and that no attempt to formulate Hegel's logic in a manner parallel to modern logic is likely to prove very illuminating.

Walter Kaufmann is represented in the volume by two essays. One of them, "The Young Hegel and Religion," is a lively discussion of some of Hegel's most accessible and most neglected work, the writings on religion and society which antedate his Jena period. As might be expected, the reader senses at times that it is Kaufmann rather than Hegel who is speaking to us out of the *Jugendschriften*, but it cannot be denied that the young Hegel provides good grist for Kaufmann's mill. The essay is marred, however, by a largely irrelevant and rather unpleasant personal attack on Georg Lukacs.

Kaufmann's other essay is his well-known re-

ply to Popper's hysterical attack on Hegel in the *Open Society*. Kaufmann painstakingly, if polemically, unravels the "quilt" of out-of-context-quotations on which Popper bases his reading of Hegel as a "proto-Nazi," and he shows quite successfully how completely unfounded this reaction to Hegel really is. The essay is Kaufmann at his best. It is from one point of view paradoxical that such an interpretation of Hegel could even be advanced by a mind as good as Popper's, since there is so little to be said for the interpretation itself, and since Popper argues for it in a manner which does not even come close to meeting the intellectual standards any respectable scholar would normally demand of himself. Kaufmann seems fully aware of this paradox, and maintains throughout the essay an attitude of respect for Popper and his philosophical work.

But Popper has certainly not been the last to brand Hegel as a "fascist" and a "totalitarian" and it is doubtful that scholarly replies will soon succeed in stripping such rubbish of its intellectual respectability. The reason for this, I think, is simply that Hegel's social thought contains a profound critique of the sort of liberal individualism on which Anglo-American culture has long prided itself. Those who look upon the liberal individualist ideology as a sort of moral fortress from which all "men of good will" must wage warfare on the "forces of darkness and oppression" have tended to react to the long tradition of Western political thought which criticizes or rejects this ideology by identifying the critics with whatever evil forces happen to be threatening the fortress of liberal individualism at the time. The groundless and anachronistic application of political labels to any critic of liberal individualism is often no more than a defense mechanism used by those who intend to close their minds to the defects of the "open" society. And since such attacks on Hegel and others are not rationally motivated, they are not likely to be silenced by rational replies. But this is not, after all, a matter for too much concern. For the harm done by this liberal manichaeism to the memory of Hegel (or Plato, or Rousseau, or Marx) is of no importance at all compared with the harm it has done and continues to do to a "free world" under its dominion.

Hegel's political thought is treated from another point of view in Shlomo Avineri's concluding essay, "Hegel Revisited." The essay is a little masterpiece, well written and surprisingly light for the amount of information and thought it contains. More essays like it, in fact, might transform "intellectual history" from a term of opprobrium into one of praise. Avineri

surveys the history of Hegel's image in Germany from Hegel's own time to the twentieth century, and attempts to evaluate the political meaning of Hegel's thought in its own and later contexts. The Hegel who emerges from Avineri's study is a cautious progressive, a supporter of a kind of enlightened constitutional monarchy which had already ceased to exist by the time both Hegel's defenders and his opponents began treating his philosophy as an apology for the Prussian status quo. Avineri concludes, in fact, that the meaning of Hegel's political thought for its own time can only be understood if we realize that Hegel was "utterly wrong in his understanding of his own *Zeitgeist* . . . Hegel's political ideas were always 'out' in Germany. Hegel never really had a chance" (pp. 347f).

ALLEN W. WOOD

Cornell University

On Colonialism: Articles from the New York Tribune (sic) and other Writings. By Karl Marx and Frederick Engels. (New York: International Publishers, 1972. Pp. 382. \$7.50.)

This edition of Marx and Engels on colonialism is a reprint of the Moscow, Foreign Languages Press edition which came out in the 1960s (undated), with the addition of five items, occupying eight pages of text. The Moscow edition was entitled simply *On Colonialism*, thus avoiding the sloppy reference to the *New York Daily Tribune* which appears in the title of the American edition.

As stated in the Publisher's Note, the selection of texts "was made with a view to presenting the Marxist analysis, as events themselves unfolded, of the predatory colonial policy of the capitalist states and of the national liberation struggle of the oppressed peoples" (p. 7). Hence what is equally important in Marx, the notion of the positive function of colonialism, is neglected apart from the inclusion of the well-known articles "The British Rule in India," "The Future Results of the British Rule in India," and some material from the correspondence.

While condemning the hypocrisy and greed associated with colonialism, Marx believed that it was a necessary experience for the non-Western world. According to Marx's analysis, the non-Western world was characterised by a non-dynamic socioeconomic system capable of perpetuating itself indefinitely in its backwardness. Marx saw the irresistible force of expansionist western capital as the historical means of bringing an end to the stasis of the east and introducing the dialectic of social change. One will

not find any editorial reference to this aspect of Marx's views on colonialism in the volume under review.

In general, the editing of the texts is careless, and there has been no attempt with this American edition to take advantage of the vast amount of research which has been carried out in recent years on the topic of Marx's analysis of the non-Western world. Footnote 38 still describes *Jagirdars* as "representatives of the Moslem feudal gentry in the Great Mogul Empire . . ." (p. 355), despite the publication in Moscow in 1958-59 of Marx's notes on Kovalevsky, in which he specifically rejects the description of the *Jagirdars* as a feudal class. (See, for example, *Sovetskoe Vostokovedenie*, 1958, No. 4, p. 18.)

The footnote to Marx's comments on the religion of Hindustan preserves in all its glory the obscurantism (prudery?) of the Moscow edition. Marx's comments read: "That religion is at once a religion of sensualist exuberance, and a religion of self-torturing asceticism; a religion of the Lingam and of the Juggernaut; . . ." (pp. 35-36). The footnote describes the religion of the Lingam as: "the cult of the deity Siva; particularly widespread among the southern India sect of Lingayat (from the word 'linga'—the emblem of Siva), a Hindu sect which does not recognize distinctions of caste and denies fasts, sacrifices, and pilgrimage" (footnote 18, p. 352). This is Soviet obfuscation at its best; what Marx meant by the "religion of the Lingam" was simply the erotic or phallic element in Hinduism, and nothing more nor less.

In one of the texts included in this edition the translation is so bad as to be seriously misleading. For example the sentence "Da kamen die Engländer und erzwangen sich den freien Handel nach fünf Häfen" is translated as "Then came the English and won free trade for themselves by dint of force in five ports" (p. 17). This seems to imply that the free trade was general, rather than restricted to the five ports.

On the whole, however, this volume provides a reasonably handy compendium of the Marxist critique of colonialism, provided complete textual accuracy is not required, and the largely illiterate footnotes are ignored. If one is still looking for ammunition with which to attack Western colonialism this is the place to find it.

The selection includes a number of pieces on the subject of the colonial relationship of England to Ireland, which are of contemporary interest. However, a full-length edition of Marx and Engels *On Ireland* has recently been published (London: Lawrence and Wishart, 1971) if one wants to find out in detail the views of

Marx and Engels on the Irish "national liberation struggle." Otherwise, a far more comprehensive collection of texts from Marx and Engels on colonialism is to be found in the volume edited by Shlomo Avineri, *Karl Marx on Colonialism and Modernization* (New York: Doubleday, 1968).

MARIAN SAWER

Australian National University

The Justification of the Law. By Clarence Morris. (Philadelphia: University of Pennsylvania Press, 1972. Pp. 214. \$12.50, cloth; \$3.45, paper.)

This book is a collection of essays organized around the central theme stated in the title. Most of the essays are taken from Professor Morris's previously published work, although they have, in most instances, been revised and, in some cases, expanded. Of the essays which appear for the first time in this book, the principal one is Chapter 4, "Law and Logic." That chapter, like much of the book, is directed toward nonlawyers who are interested in law and its role as a social institution. Morris explains why judicial decision making is not primarily an exercise in deductive logic nor even a form of inductive reasoning. Nevertheless, Morris takes pains to show, through cogent examples, that lawyers are not deluding themselves when they assert that the common-law doctrine of precedent does sufficiently restrict the scope of judicial decision so that it is not what Max Weber called "khadi justice."

The principal point of emphasis of the entire book, however, is that law and the judicial process are not self-contained activities. In Morris's own words (p. 1), "the more that law implements the public's genuine and important aspirations, the more just the legal system becomes." Accordingly, Morris strongly affirms the imaginative use of precedent to realize the public's "genuine and important aspirations" that is exemplified in the judicial opinions of Benjamin Cardozo. He recognizes that there are clear limits to what courts can or even should attempt to do. What he fears, however, is not that judges will abuse their authority to change the law but, rather, that the innate conservatism embodied in the techniques of judicial decision making will unduly inhibit judges from exercising the creative powers they inevitably must possess in a legal system like ours. Morris recognizes, nevertheless, that the complex problems with which our society is confronted—such as those involving social welfare and the accelerating exploitation of our natural environment—require solutions that can only be generated on the basis of substantial empiri-

cal research. If, therefore, the public's "genuine and important aspirations" are to be achieved, we must inevitably resort to legislative solutions. Only in that way will it be possible to utilize fully the data that one hopes will be generated by the behavioral and natural sciences. Morris's recognition that the demands for justice in a dynamic society require continual adjustments in legal relationships also leads him to consider (particularly in Chapter 3) the question of the ability of a parliamentary democracy to respond adequately to changing social conditions. He contrasts the views of John Stuart Mill and Herbert Marcuse. Morris himself is a Millian, but he does not dismiss out of hand Marcuse's skepticism about the adaptive qualities of the traditional legal structure.

Professor Morris has had a distinguished career as a legal scholar and teacher. The essays reflect the wisdom that such a man acquires over the course of such a career. As a lawyer, I was particularly interested in Morris's discussion of Chinese law, a subject to which he has turned his attention in recent years. I found particularly stimulating and instructive the contrast he draws between the uses of law and the nature of the judicial process in China—particularly with regard to Chinese criminal law before the overthrow of the Empire in 1911–12—and those that have prevailed in the West.

GEORGE C. CHRISTIE

Duke University School of Law

Magna Carta: The Heritage of Liberty. By Anne Pallister. (New York: Oxford University Press, 1971. Pp. 133. \$6.50.)

Anne Pallister's book is an attempt "to explain the growth, development, and transfiguration of Magna Carta" (p. 6) since the seventeenth century. The result is a first-class study—sensible, penetrating, scholarly, well-written, and exciting. It is also short. A less able writer would have made it longer.

In excellent chapters on the seventeenth century, Anne Pallister shows how the supporters of the cause of Parliament used Magna Carta to strengthen their attempts to limit the royal prerogative. The Long Parliament moved away from Sir Edward Coke's belief in the supremacy of law toward the new doctrine of the supremacy of Parliament. Meanwhile the Levellers and Independents united to oppose the radical claims of Parliament. The Levellers "developed a constructive program of reform based upon Magna Carta, which they elevated to the status of a political bible" (p. 15).

The latter part of the century saw the resumption of the conflict between the prerogative powers of the crown and the insistence of

the Whigs upon the sovereignty of the law and the customary rights of the people. The Declaration of Rights of 1689, another symbol of liberty, was called the "Second Magna Carta" because it seemed to proclaim the triumph of the principles of Magna Carta. The Revolution of 1688 made it certain that supreme power was to lie in the hands of Parliament.

The early eighteenth century saw the extension of the powers of Parliament and the steady decline of the importance of fundamental law. At the same time, appeals were made both to Sir Edward Coke and John Locke when acts of Parliament seemed to threaten individual rights. Whig and Tory disputes multiplied about the old issues of liberty, the origin of Parliament, the nature of the constitution, and the corruption of politics in the age of Walpole and Newcastle. Late in the century the authority of Parliament was challenged both by the radicals in England and by the American colonists. Camden and Chatham believed that the actions of Parliament must be limited by fundamental law. Blackstone argued that power resided in Parliament. "What the Parliament doth, no authority on earth can undo." He did admit that there were rights, such as personal liberty and property rights, that he traced from Magna Carta, above the power of Parliament.

Anne Pallister's comments about the reformers of the eighteenth and early nineteenth centuries (John Wilkes, John Cartwright, William Cobbett, and Sir Francis Burdett) are interesting and excellent. She remarks that

during the controversy surrounding Wilkes, Magna Carta was widely used both to justify ancient rights and as a slogan calculated to stir the patriotic emotions of the populace in Wilkes' favour. By this time the Charter had become totally divorced from its historical setting and it is doubtful whether half of those who referred to it had any personal knowledge of its contents; they merely read back into it whatever political maxim or precedent they required (p. 62).

This has often been true.

In "The Charter Attacked" (Chapter 6) the author shows how many men in the eighteenth century abandoned the appeal to history in favor of an abstract natural law that stressed the rights that belong to all men. Thomas Paine, for instance, departed from the common-law tradition and insisted that every generation has a right to act as it sees fit. Edmund Burke stressed the feudal idea of the Charter and rejected Coke's idea that it was a statement of natural liberties. There are excellent passages (pp. 82-88) explaining how Jeremy Bentham and John Austin developed a justification for

parliamentary sovereignty by substituting a principle of utility for earlier philosophical theories based upon historical or natural rights theories.

The author entitles the last chapter of her book "The Charter Repealed." By 1970 only 4 chapters of the 37 chapters of the Confirmation of the Charters of 1297 survived. (The Confirmation is printed in an Appendix.) The other 33 were repealed between 1828 and 1969 as a part of an attempt to reduce and clarify the laws of England. Thus Magna Carta has been "repealed." It is also true that acts of Parliament cannot destroy Magna Carta as a symbol of freedom, a milestone on the road to liberty.

GOLDWIN SMITH

Wayne State University

Friedrich Meinecke and German Politics in the Twentieth Century. By Robert A. Pois. (Berkeley: University of California Press, 1972. Pp. x, 164. \$7.50.)

This earnest, somewhat labored volume attempts to relate the "political thinking" of a famous German historian to "German internal politics during the twentieth century."

Friedrich Meinecke, who died in 1954 at age 92, was a major figure of German historiography and the modern school of "historicism." Student of Dilthey, Sybel, Droysen, and Treitschke, contemporary of Max Weber, Troeltsch, and Rickert, Meinecke had a career that spanned six decades, and he remained active to the end. One of his best-known books was a slender 1946 volume in which he reflected on "The German Catastrophe." His most important scholarly works were "Machiavellism: The Doctrine of *Raison d'État* and Its Place in Modern History" (1924) and "The Origins of Historicism" (1936). He has already been the subject of several books written in Germany and the United States.

While Meinecke is known as an important historian of ideas and theoretician of "historicism" (which he defines as "replacing a general and abstract contemplation of human affairs by an individual one"), he is also often held to have been one of the moral leaders of the attempt to understand and overcome the origins of Nazism in Germany, and a man especially open to revision of his political thinking. In this interpretation, for example, Meinecke is held to have moved from being a proponent of Prussian-German authoritarianism to the position of tolerant democrat, even as early as the years of the Weimar Republic.

It is such interpretations which Professor Pois, himself an historian, is concerned to ex-

amine—and reject. He finds Meinecke's political thinking to have been muddled, especially in misunderstanding the characteristics of Nazism and failing to provide a "meaningful approach for the investigation of Germany's past" (p. 143). He writes of Meinecke's movement from "statism" to a "confused melange" of ideas, ultimately oriented around a vague and apolitical "cosmopolitan Romanticism" which was neither intellectually convincing nor based on an awareness of the political realities of modern mass movements. (p. 48). According to Pois, not only was Meinecke frequently in "a curious sort of nonideological proximity . . . to the extreme Right" in coping with the collapse of the Empire, the radical attacks on Weimar, and the emergence of Nazism—he was also a "curiously nationalistic Goethean cosmopolite adrift in a world of mass chiasm and mass politics" (p. 118).

Despite the author's frequently unclear language, and his intolerably loose use of concepts such as "class" (he often refers to Meinecke as a representative of an undefined "class," and on at least two occasions seems to dismiss the whole rationale for his own study by asserting that Meinecke, after all, "was neither an ideologist nor was he capable to understanding [sic] an ideology"; he "was merely a rather conservative, very cultured, rather pleasant, and somewhat prejudiced German burgher" [p. 114]), the book does draw attention to some important themes. Pois shows, for example, that Meinecke's persistent view of the Hitler movement as "divisive" blinded him to the integrative function of the Nazi appeal for "unity" and a new "*Volksgemeinschaft*." Presumably, had Meinecke himself been more aware of the dilemmas of reconciling diversities within a body politic, and less a "bourgeois German" who fundamentally longed for the "unity" of Imperial times, he would have been able to understand the powerful appeal of a movement which promised a unity that was classless. Even Meinecke's 1946 reappraisal of German history in light of the Nazi phenomenon is shown by Pois to have been quite unclear and actually beside the point, for it reflected more the confusions of a "decent German" than a persuasive interpretation of the roots of the German problem.

Professor Pois' work, based on a doctoral dissertation, hardly settles the question of Meinecke's political development, despite its contributions. Much less does it shed great light on what is surely a fundamental theme: the relationship between German humanistic scholarship as a whole since the "discovery of

history," and political conceptions. The relationship between Meinecke's "historicism" and the confusions in his political thought is not clarified by the author. Insofar as he refers to this theme, Pois seems to think that Meinecke should have been a more consistent historicist, and that this would have helped him understand the temporality of phenomena (and, therefore, the appearance of new phenomena such as "mass politics"). Such assumptions are popular, but one wonders whether they are anything more than an intellectual fad. Humanistic scholarship as a whole may be endangered, not because it is insufficiently attuned to change, but because its own endeavors may be debunked as the mere luxury of a particular time, place, or class. While one is not convinced by Professor Pois' book that a study of Meinecke would be the most appropriate way to begin a clarification of the assumptions of "historicism," the author might have presented more clearly the relevance of that way of thinking to Meinecke's political views.

GEORGE K. ROMOSER

University of New Hampshire

The Defense of Gracchus Babeuf Before the High Court of Vendôme. Edited and translated by John Anthony Scott. With an essay by Herbert Marcuse. (New York: Schocken Books, 1972. Pp. 120. \$1.95, paper.)

Gracchus Babeuf was not a major political philosopher. What then is the value of this volume for those who have no special interest in French socialist thought? Besides providing the reader with a neat synthesis of primitive communistic philosophy based on total equality, this work contains an elaborate argument in defense of conspiracy against the state. Specifically, Babeuf attempts to convince a court—appointed by the government which he has sought to overthrow—that he should not be punished for plotting against the established regime. Herein lies the interest of the *Defense* and its "relevance" to an important contemporary issue.

Babeuf's defense is based on (1) a redefinition of the concept of "conspiracy," and (2) an argument for innocence by association.

(1) The charge of participating in a conspiracy is unfounded, because, according to Babeuf, "a conspiracy not directed against the people cannot be a conspiracy at all" (p. 28). A conception of conspiracy which equated it with the intent or act to overthrow any "established" government, would condemn people to remain subjugated to unjust regimes, and is, therefore, unacceptable. Likewise, "to work for

the overthrow of a constitution freely adopted by the people" is not necessarily conspiratorial, for "the people might, with apparent freedom, have adopted a radically vicious Constitution" (p. 35). Rather, the only truly subversive act would be a conspiracy "to bring about the overthrow of legitimate authority"—i.e., authority established according to "the true principles of popular sovereignty; which governs in accordance with those principles; and which dedicates itself to the welfare of the nation, to the enhancement of its glory, to the defense of its freedom" (p. 36). Hence, Babeuf argues, his intent to overthrow the French government established by the Constitution of 1795—a freely adopted but "vicious" constitution—is not a conspiracy. Rather, he and his associates were motivated by the desire to replace an "illegitimate" authority with a regime calculated to achieve both "the common welfare, which is the purpose of social existence," and "the happiness of the greatest number, which is the purpose of the Revolution" (pp. 44–45).

(2) Most of Babeuf's defense is devoted to a summary of his ideas and a demonstration of the identity between these conceptions and those advanced by some of the most "revered" and "admired" French political philosophers. This tactic rests on Babeuf's belief that "the man who wills the end also wills the means to gain that end":

It is clear that these changes [proposed by the "conspiracy of equals"] cannot be achieved without the overthrow of the existing government and without measures to eliminate whatever obstacles stand in the way. This overthrow of the established order, with whatever repressive measures accompany it, is a necessary consequence of willing the original goal (p. 48).

Thus, if we are to accept Babeuf's argument, he and his comrades are on trial for their ideas—ideas which were first propounded by such "eminent men" as Rousseau and Mably "who command the respect of everyone" (p. 63).

The reasoning within Babeuf's *Defense* is not rigorous. For instance, legitimate authority is, effectively, made equivalent to the form of government he prefers—i.e., a communistic system. Every other system is illegitimate, because even if supported by the overwhelming majority of citizens, Babeuf could nevertheless claim the people were misinformed or had been tricked. To cite another example of the weakness of Babeuf's reasoning, the idea of innocence by association is not only as logically untenable as the more common guilt by association arguments, but also is somewhat inapplica-

ble. For after all, Rousseau, as well as Babeuf's other philosophical ancestors, did not attempt to develop a revolutionary organization and prepare to overthrow by force the established regime; yet, this is precisely what the "conspirators" did seek to do, and the communality between their ideas and those of their predecessors is irrelevant.

But to regard Babeuf primarily as a thinker and a philosopher seems somewhat erroneous. His rather primitive communism and concept of pure egalitarianism is largely derived from the works of others, as he himself repeatedly claims in his *Defense*. Babeuf's major contribution was as a political organizer. He sought to develop a party, an elite which would spread the "proper" ideas among the "masses" and convince them of the need to seize power for themselves—i.e., what in modern terminology would be referred to as the "vanguard of the proletariat." It is regrettable that neither the editor, John Scott, nor Herbert Marcuse devoted any of their commentary to this important aspect of Babeuf's historical role. In addition, their otherwise insightful and useful remarks about his life and philosophy are somewhat marred by painting his picture in exclusively glowing terms. We are told how Babeuf rises from poverty to play the noble role of leading a revolutionary struggle against an unjust and evil government. We see an uneducated man who learns to be a philosopher; a man motivated entirely by his desire to do good and to make a better world for his compatriots. But even so important an event as Babeuf's attempted suicide following the Court's verdict—which might be regarded as a rather unheroic act for someone presented to us as having heroic stature—is not mentioned either by the editor or by Mr. Marcuse. Rather, Marcuse cryptically refers to this event as "the horrible scenes following the pronouncement of the death sentence" (p. 103). In summary, this book is to be valued not for its rich theoretical insights, but rather for its presentation of the interesting process by which a condemned man attempts to convince a court (appointed by the government he sought to overthrow) that he should not be considered guilty as charged.

WILLIAM R. SCHONFELD

University of California, Irvine

Xenophon's Socrates. By Leo Strauss. (Ithaca: Cornell University Press, 1972. Pp. 181. \$8.50.)

In this book Professor Strauss offers an account of Xenophon's *Memorabilia*, *Apology of*

Socrates, and *Symposium*. The account is textual analysis, narrowly construed. It is devoid of footnotes, and ignores the mass of secondary literature on Xenophon and the Socratic Problem generally: no attempt is made to assess the accuracy of Xenophon's portrait of Socrates, nor to relate it to the differing and often conflicting accounts in Aristophanes' *Clouds*, Aeschines of Sphettos, and most especially, Plato. Professor Strauss also ignores such questions as these: Is the *Apology* attributed to Xenophon spurious? Was the *Memorabilia* directed, as Cobet thought, not to the speeches of the original accusers, but to the "Accusation of Socrates" written by the sophist Polycrates well after the trial? Was Xenophon's portrait of Socrates largely derivative from Plato's, as Burnet thought? Failure to take account of the scholarly tradition dealing with Xenophon is a serious flaw in this book.

Nor is the flaw redeemed by Professor Strauss's analysis, which is generally hard to follow, and often insensitive. A sample will do. At *Memorabilia* IV, iv, 19, the sophist Hippias is made to agree with Socrates that there are unwritten laws made by the gods for men, for example, honoring gods and parents; but Hippias denies that the prohibition against incest is among those laws, on the ground that some nations transgress it. Professor Strauss infers that, "He obviously implies that the unwritten laws are never transgressed and in particular that the laws enjoining honoring the gods and the parents are never transgressed by anyone; whether he thinks so from innocence or from the lack of it, we are in no position to tell" (p. 113). The obviousness is unobvious. Hippias is proceeding on an assumption common to his time, that divine law is, not untransgressible by individuals, but universal among peoples—an anticipation of the later association of *jus gentium* with *jus naturale*.

R. E. ALLEN

University of Toronto

Development Assistance in the Seventies: Alternatives for the United States. By Robert E. Asher. (Washington, D.C.: The Brookings Institution, 1970. Pp. 248. \$6.95.)

This study considers the future of American aid to low-income countries. It covers a broad range of topics, including the rationale for aid, important aspects of the development process, the case for a multilateral program, and foreign trade and investment. The author, a Senior Fel-

lowship holder, discusses the nature, scope and administration of American aid.

As a background to the discussion of aid, Asher outlines the changes occurring in the socioeconomic, political, and military environment in America and internationally. Unfortunately, he does not consider the fundamental changes in communication, technology, and ideas; he concentrates on the more visible disturbances, such as poverty in the United States and protests, and on policies for dealing with these symptoms.

Asher assumes "that the economic, social and civic development of low-income countries . . . is in the long-term interest of the United States" (p. 2). Whether this is so is a cardinal question. It has not been answered convincingly, and this has affected America's willingness to give, as well as practically every other aspect of her aid policy, including the relations with the recipient countries. Often, the relevant arguments contain a number of steps. Thus, economic development might or might not increase a country's political and social stability; either outcome might or might not enhance the country's disposition for international accommodation; and this, in turn, might or might not serve the interests of the United States. Sometimes, one or more steps are missed; the effects, economic and political, of using funds for aid or, alternatively, for other purposes are not compared; or the development abroad is confused with the aid motives or interests of the donor. Some might argue that too big a jump is involved even in the argument that "recognizes the drive to conquer poverty abroad as a logical extension of the same pressures and considerations, moral and material, that make urgent the conquest of poverty . . . at home" (p. 103).

Like the corresponding chapter of the Pearson Report, Asher's chapter on the rationale for aid raises at least as many new questions as those it tries to answer. The reader who is concerned about the future of aid will find little help for persuading others that aid serves the long-term interests of America. Asher recommends educating Congress about development assistance and persuading the President that vigorous leadership in favor of aid is in the best interest of the United States. Alleged weaknesses in America's leadership have been blamed too easily for the flagging of aid, while the experts have failed to provide unequivocal arguments in its favor. Attempts to educate Congress might sharpen the question of the re-

Asher quotes widely diverging projections of income growth in the poor countries and asserts that a 4 per cent yearly increase in per capita GNP can be obtained in the 1970s. He does not examine the conditions in which such a rate of growth might be feasible and desirable. Various rates of change in economic and social factors affect incentives and responses differently, and they may seriously qualify the possibility of obtaining given rates of growth in income; also, they have a different bearing on welfare, on political and economic stability, and on a host of other developments. Asher is satisfied that in developing countries income inequality will decrease. However, the very creation of a middle class as the rate of development increases, combined with advances in communication, might enhance interpersonal comparison and lower welfare. Asher views population growth mainly as an obstacle to increasing per capita income. But the rate of population growth reflects also social factors and goals; it has important implications regarding the relative shares of world resources which the various nations control, as well as possible claims for redistribution; and it is related to the world problem of growth of agricultural and manufacturing production, pollution and resource depletion, and the constraints on growth and its structure.

In his discussion of private foreign investment Asher might have provided some indispensable background: often, in the poor countries the demand for capital is low, like the supply, and capital earns a higher net return in the wealthier countries. This explains why private capital must be enticed by the poor countries or induced by the advanced countries to migrate. It underlines the importance of policies to raise the demand for capital in the developing countries. It also suggests that "vigorous promotion" of foreign investment by the United States might involve a substantial aid element, and, since such investment might also involve political and other risks, in many cases straightforward aid might be preferable.

Aid from the West, especially the United States, has been floundering. The spirit as well as the flesh has been weak, while the arguments advanced in support of aid have proved insufficient. Asher's stand and efforts to convince are, in many cases, based on personal preference and individual value judgment, rather than rigorous analysis. This kind of approach is becoming more common in the literature on development and aid as it is being increasingly recognized that the problems and goals are more

than strictly economic or political. The power of aid evangelism remains to be proved.

S. G. TRIANTIS

University of Toronto

Revolutionary Politics in Massachusetts: The Boston Committee of Correspondence and the Towns, 1772-1774. By Richard D. Brown. (Cambridge, Mass.: Harvard University Press, 1970. Pp. 282. \$10.00.)

During most of the eighteenth century, the colony of Massachusetts was a stable society—indeed, a deferential society in which plain citizens followed the leadership of men of superior social and economic status. And yet when the Stamp Act, the Quartering Act, and other assertions of British power in America became manifest in the years from 1765 to 1772, Massachusetts took the lead in fomenting revolution. The beginnings of revolution are traced briefly in two chapters of Professor Brown's study. His primary interest is in the ideology of revolution in the Bay Colony, and particularly in the political techniques the revolutionary forces evolved.

Unlike the birth of most new nations today, the movement in Massachusetts was a revolution of continuity of thought, as it drew on a long-established rationale. This was the radical-whig conception of balanced government that the colonials borrowed from England, a tradition that reached back to Machiavelli and James Harrington among others—whig in the sense of ideology, not party. Much of the revolutionary rhetoric stemmed from the classical whig notions of "virtue" and "corruption." The domineering royal Governor, Thomas Hutchinson, was identified with corruption of the whig balance of executive and legislature; and the colonial cause was portrayed as an effort to restore the balance, and thus virtue in the system. The ultimate result was the American experiment in "republican government." But only later did liberal, Lockean ideas come to the fore.

The crisis also generated new forms of political action, and most of Professor Brown's study is devoted to this aspect of the revolution. The chief device was the "corresponding committee," first launched in Boston; and the Boston committee became the central, coordinating engine of political activity throughout the province, as other committees emerged in town after town. The Boston group not only initiated leads for action, but also provided encouragement, reassurance, and reinforcement. The movement also generated public assem-

blies, loose conventions composed of members from different towns (one of which included delegates from 100 of the 250 towns in the province), "instructions" for action at official town meetings, and ultimately county conventions. Men like James Otis, Samuel Adams, and Josiah Quincy, Jr. took the initiative in propaganda and rationale—Bostonians all. The famous John Hancock came in later.

Although Professor Brown's study is sometimes prolix and repetitious, it is based on thorough and often sophisticated research as he carefully analyzes the first stirrings of the first colonial revolution.

WILLIAM NISBET CHAMBERS

Washington University

Free Press and Fair Trial. Edited by Chilton R. Bush. (Athens, Ga.: University of Georgia Press, 1970. Pp. 133. \$3.75.)

At a time when the subject "free press" brings quickly to mind disputations on the legal and political status of the Pentagon Papers and protection for confidential news sources, it bears noting that another part of the cacophony of opinion on the relationships between government and media has not gone away. This is the free press-fair trial debate which, as one might expect, ebbs and flows in intensity. At the time of this writing the tide seems to be out, but the elements of the debate remain, involving what the late Justice Frankfurter might have called a "clash of rights." That is, the controversy thrusts to the foreground the continuing conflict between the public interests in free expression and due process of law.

The book under review was published near the end of the last round of national debate on the subject, and I have the impression that not very much use has been made of its findings, certainly not among political scientists. Perhaps the book will be remembered for the next round, but for now it may be that the volume has fallen victim to cyclical relevancy. Nonetheless, there is much here of interest to students of the judicial process. The studies which comprise the little volume were written by Fred S. Siebert, Walter Wilson, and George Hough III, each a student of journalism and communications. Their work complements some of the data and conclusions of Professors Kalven and Zeisel in *The American Jury* (Chicago: University of Chicago Press, 1966), even though the two groups of scholars employed somewhat different methodologies. Looking for the principal factors influencing jury verdicts in criminal trials, Kalven and Zeisel evaluated question-

naires completed by 555 judges reporting on 3576 jury trials. In the volume under review, efforts were made employing this and other methods to explore the impact of various kinds of pretrial publicity on jury deliberations and verdicts, a factor not really treated by Kalven and Zeisel. One of the studies in the Bush volume surveyed trial judges' opinions on prejudicial publicity, another noted the frequency and intensity of press coverage of felonies and resulting trials, and a third analyzed the findings of several jury simulations.

Taken together, the three suggest two major findings. First, the commission of most felonies and the great majority of trials attract very little newspaper attention. For instance, of 11,724 felonies committed in New York City in January of 1965, the editors of the *New York Daily News* chose to report only 41. The smaller number itself says nothing of course about the intensity of publicity accorded "newsworthy" crimes and trials; indeed, the impact on the trial process could be significantly greater with such selective coverage. No account is taken, however, of radio and television coverage, and this omission is a weakness, given the extent to which television is the main news source for most Americans. Second, a single pretrial cognitive element—a defendant's criminal record—will very probably have a disproportionate effect on the jury; its members are more likely to return a verdict of guilty when they know the accused has committed offenses in the past. Presumably, news of past criminal acts may result in an almost unconscious conclusion of guilt by the potential juror. It is also significant that knowledge of this record of prior violations can easily survive in the minds of potential jurors from initial exposure to this information to an eventual rendering of the verdict. Other kinds of pretrial publicity, such as publication of a confession or a description of evidence later held inadmissible at the trial, either do not survive or have little measurable effect on the verdict. These findings thus tend to weaken the argument of those who advocate a broadening of informal agreements between the media and government to discourage "trial by newspaper." They will certainly weaken the forces of those who go farther by advocating the application of legal sanctions to reporters and policemen who violate rules on pretrial publicity. With newsmen especially, the state would be required to show a compelling need for the particular infringement on free expression, and this will now be more difficult to establish except of course in the case of a de-

fendant's criminal record and in those rare instances of intimidation of jurors by media.

There is a collateral question, however, which the authors overlook. Assuming that tentative conclusions presented here do indeed prove in further studies to be sound ones—that is, if it is later demonstrated conclusively that very little pretrial publicity is prejudicial against a defendant—this does not mean that current legal rules and the few informal agreements in this area ought to be relaxed. There is, after all, the matter of the internal, as well as external, perspectives on an operable legal system. If there is the appearance of injustice when mounds of pretrial publicity do arise in selected trials, the legal system risks a diminution in the legitimacy accorded the trial process. This risk could be much higher if some of the procedural requirements were slackened. Thus, public policy decisions in this area may be more influenced by symbolic concerns than established fact.

In summary, the volume adds appreciably to the stores of knowledge in this area and deserves attention from students of the trial process, especially jury behavior.

D. GRIER STEPHENSON, JR.

Franklin and Marshall College

Big Brother's Indian Programs—With Reservations. By Sar A. Levitan and Barbara Hetrick. (New York: McGraw-Hill, 1971. Pp. 214. \$8.95.)

The relationship of the federal government to American Indians has never been easily summarized. Nor can it be today. For limited purposes, a comprehensive description of functional activities on Indian Reservations is of use. This is such a book: anyone intending research on a reservation will find here an accessible description of most active Reservation programs, including brief histories of these activities, their evolution, and recent levels of funding. These program reviews include touches of what the Bureau of Indian Affairs might do "better" in the view of the authors at what the Bureau supposedly intends to do.

In light of the high quality of Professor Levitan's previous published work (*Federal Aid to Depressed Areas* and *The Great Society's Poor Law*), and his well-publicized testimony before many legislative committees of Congress, this volume is a disappointment. There is no internal indication that any but official data sources, (e.g., Indian Health Service obligational authorities, Bureau of Indian Affairs internal memoranda, statutes) were consulted. It is not

revealed if the authors visited a reservation or talked with an Indian. They caution officialdom to remember that not all wisdom is to be revealed in Washington. The same caution can be voiced as regards evaluation of Indian programs; not all assessment can come from the Federal City either.

Criteria of assessment and evaluation remain implicit, murky, and official. Thus we are told the industrial park development in the Gila River Indian Community (not the Pima-Maricopa Reservation which they inaccurately call it) is "relatively viable." Relative to what? Similarly, the tourism complex at the Warm Springs Indian Reservation is thought to be "viable." What, one wonders, is the basis of such conclusions. The Warm Springs operation in Oregon is regarded by the Bureau as exemplary. An analysis might well have gone beyond an official view so as to enlighten students of development and mobilization about the political and social impact of virtually eliminating unemployment in a community where previously more than half the adult population sought work unsuccessfully.

No mention is made of the sizable urban "relocation" program of the Indian bureau, which when ended in early 1972, was at the \$100-million-dollar annual level. Although that effort to press Indians to make their way in the slums of our great cities was going full throttle when this book was researched, the authors pass it by altogether.

The great bulk of the book, then, is a description of those functions which are relatively stable, politically accepted, and supported in Washington. The resulting analysis is ahistorical and apolitical. More egregiously, it is a summary without criticism.

The complex rage many Indians feel about BIA programs is left out. The BIA's adjustments to Administration pressure is nowhere mentioned. The intramural battles of the Indian bureau are omitted from the analysis, as are local community hostilities toward neighboring Indian settlements. If such matters were beyond their intentions and somehow therefore excusable, the failure to analyze extant subeconomies within reservations is not so. One central element, commercial life pivoting on the trading post, is nowhere depicted. The low multiplier effect within the reservation is one of its crucial economic features. When this book was written these trading post operations had received no official attention. Within months of publication the configuration of pressures had shifted. Thus do reviews of programs based

only on official sources suffer, even in the short term.

A more comprehensive treatment of the factors affecting, indeed moulding Indian development is still needed.

JOHN E. CROW

University of Arizona

The Private Nuclear Strategists. By Roy E. Licklider. (Columbus: Ohio State University Press, 1971. Pp. 213. \$11.00.)

"The Private Nuclear Strategists"—what a rich and fertile topic for research! For these are the men (and women?) who "have written most of the new extensive literature on strategy and disarmament since World War II; the new language and terminology of deterrence is largely a product of their work . . . they have conducted a public strategic debate over the past ten years that has significantly raised the general level of public information and debate and that has at least accompanied, if it is not the sole cause of, major changes in strategy by the American government" (p. 8).

Roy Licklider divides the strategic community into three groups: the professional military, civilians employed by the government and civilians outside of government. It is this latter group, the private nuclear strategists, who are the subject of his Ph.D. dissertation. Licklider is correct in stating that "our knowledge about the inner workings of the strategic community is rather scanty" (p. 7) and "that we know remarkably little about the individuals who make up this group" (p. 9).

Unfortunately after reading the book we know little more than before. The study is wooden (there is no real feeling for the substantive content of nuclear strategy) and bloodless (it lacks any real sense of who these people are, what they are really like, what their relationships are with each other, or with their employers or how they view their work).

Licklider's population consists of 491 private civilian strategists who have written at least one book or three articles on nuclear weapons policy between 1946 and 1964. The 191 respondents (only 39 per cent) who completed his mailed 11-page questionnaire constitute the sample. In addition, 16 personal interviews were conducted with respondents.

The central hypothesis of Licklider's study was that "the private nuclear strategists were divided into a few distinctive subgroups or communities" (p. 14). He expected "that communication would be largely *within* rather than *between* communities and that the little intercom-

munity communication that existed would be largely restricted to influentials" (p. 15). The data fail to substantiate the hypothesis of "communities within the population."

The study illustrates one of the most disturbing trends in contemporary political science, i.e., blind or crude empiricism, infatuation with methodology and quantification solely for their own sake. Without any underlying broad organizing formulations or conceptualizations, factor analyses with verimax and quartimax rotations can be just plain data grinding. J. Scott Armstrong has pointed out, in an article aptly subtitled "Tom Swift and His Electric Factor Analysis Machine" (*The American Statistician*, 21 [December 1967], 17-21) that "it is now much easier to perform the factor analyses than to decide what you want to factor analyze."

The author performs sophisticated factor analyses of a "binary choice matrix, using the Cholesky decomposition method, a square root technique and varimax rotation of factors with one or more high loadings" (p. 32). Out tumble two tables with 6 and 20 factors respectively which show us that Thomas C. Schelling is linked with Henry Kissinger on one factor, with Donald Brennan on another, with Walter Millis on yet another and also with Albert Wohlstetter on a fourth factor, and that there is a Paul Ramsey factor. But the possible reasons for or implications of such linkages are never discussed. Are Schelling and Kissinger linked because they both taught at Harvard, or because both have been frequent government advisers? Why is it that Donald G. Brennan and Jeremy J. Stone, whose strategic views are now so disparate, constitute a factor? The tables are simply presented, with no attempt to probe the relationships, or to uncover the underlying factors.

In thinking about the private nuclear strategists and about the issues on which they have been really effective, two items seem to be of overwhelming importance. One is that at one time or another most of these men were associated with the RAND Corporation. Although the RAND Corporation appears six times in the index, it is never discussed in any depth or detail. Nor is any cognizance taken of the association between the civilian strategists and the "think tank." In a similar vein, the Daedalus volume which resulted from the 1960 Summer Study on Arms Control is probably the single most influential volume on arms issues to date. Although many of the subjects of the book were associated with the study and indeed many of their seminal writings appeared in that

volume, Licklider makes no mention of either the Summer Study or the resultant publications.

In addition to mindless factor analyses, most of the fifty tables sprinkled throughout the work are admittedly not statistically significant. Even where statistical significance is claimed, a large number of the tables appear meaningless. For example, in Table 31 we are told percentage figures total across but they only total 86%; in the same table, with an N of 9 and category percentages of 33,11,33 and 11 we are told the significance level is .02; Table 34 finds a significance at the .01 level for cross category percentages of 8,8,6 and 8; and Table 38 quotes a "total sample of 145" based on the Ns of 51,14,15 and 45. In short, senseless data manipulation is here compounded by carelessness.

We are left pretty much where we started:

Real insight into and understanding of the members of the strategic community, their power and their influence in nuclear strategy and disarmament, their ideas, goals, aspirations—all this remains virgin territory, virtually unexplored by *The Private Nuclear Strategists*.

ANNE H. CAHN

Massachusetts Institute of Technology

The Accessible City. By Wilfred Owen. (Washington, D.C.: Brookings Institution, 1972. Pp. 150. \$6.50.)

Wilfred Owen, a transportation economist, has brought together in this slim volume a good deal of useful information for the student of urban affairs. He reviews transportation programs and problems, urban development activities, and comprehensive planning ventures in the United States and a number of other nations, both industrialized and developing. The comparative perspective is particularly welcome given the growing interest of urban specialists in comparative analysis. In examining urban transport and development issues in a comparative framework, Owen emphasizes the extremely powerful impact of the automobile on urban development, and thus on the urban political system, in every urban society. Regardless of whether government has encouraged or discouraged automobile ownership, built vast or meager highway networks, and succored or starved public transportation, the automobile has fundamentally altered the urban pattern, promoting decentralization and mobility, and at the same time causing congestion and health hazards.

Owen treats the demand for transportation in metropolitan areas as a function of the pattern of urban development. Thus, he reminds us that demands for public goods and services

such as highways, parking facilities, and mass transport systems are not immutable givens, as some models of the political process seem to assume. Demands result from complex and dynamic social, economic, and political interactions. In the case of urban transportation, Owen concludes that governmental strategies designed to alter the demand for transportation services are likely to be more successful than are mere responses to the existing or forecast patterns of demand, especially for automotive transportation. Thus, he argues for more comprehensive planning and stringent public controls on the location of houses and jobs to create new urban environments that are both accessible and livable.

Owen does not examine the political processes that have contributed to the creation of the existing urban pattern of development. He does not attempt to analyze the complex differences in political culture, national history, institutional arrangements, intergovernmental relations, taxing systems, size and scope of the public sector, and other factors which help explain both the urban policy patterns of different nations and their capabilities to undertake the kinds of programs that he advocates. As a result, the political analyst will find more questions than answers in this volume; but both the questions Owen raises and the information he presents provide a useful starting point for more systematic comparative analysis of the politics of urban transportation and metropolitan development.

MICHAEL N. DANIELSON

Princeton University

Reapportionment in the 1970s. Edited by Nelson W. Polsby. (Berkeley: University of California Press, 1971. Pp. 296. \$10.00.)

Those who think reapportionment is a dull subject which has attracted pedestrian minds should take a look at this collection of essays by first-rate scholars, each followed by a thoughtful discussion by a well-qualified commentator. For my part, I approached the book with definite preconceptions. I felt that the Supreme Court had dealt with apportionment problems in a mechanistic way, assuming that a bit of tinkering here and there would produce the desired results. The Court's response to reapportionment had also been simplistic, for nowhere except in minority opinions and stray dicta had it faced the basic complexities of representation. For example, the Court never recognized that American government has traditionally provided multiple points of access to decision making, and that groups losing in one

political arena could be doing quite well in another. More specifically, the opinions of the Court showed little understanding of the legislative scene and of two points in particular: (1) In numerous legislatures the major line of division was not territorial but partisan, so that the real question about apportionment was not the one-man/one-vote rule but the effect on the partisan division. (2) Recruitment and career patterns may have more to do with effectiveness in the legislature than do numbers per representative. My own impression is that rural members are likely to be disproportionately effective, because they, more than urban members, are recruited from the capable membership of their communities, have few career alternatives, and tend to stick it out in the legislature.

Given these dim views, I was delighted to find corroboration in the essays by Robert Dixon and Alexander Bickel in this volume. From Dixon, "We expect far more from the election system than mere filling of legislative seats . . . A further goal . . . is that there be room in the system for all significant interests to acquire spokesmen—preserving the right of all to be heard, if not to be in control" (pp. 10–11). And more from Dixon: "Wielding one man, one vote, like a meat-ax, the Court has not been content only to lop off extreme population malapportionment. It has come close to subordinating all aspects of political representation to one overriding element—absolute equality of population in all legislative districts" (p. 11). Looking at that statement in the light of the New York and Missouri cases of 1969, one can only object that the Court didn't just come close—it actually *did* subordinate all else to absolute equality of population. Dixon's view is that the Court should have followed a substantive due-process argument in the apportionment cases, thereby allowing much more play for legislative judgment but at the same time disallowing the grossest forms of malapportionment.

Bickel expounds his more elliptical position very skillfully. Consider: "From the beginning, the Warren Court's apportionment decisions have consistently asked the wrong questions about American political institutions" (p. 60). And: "The sensible question to ask about any institution of government . . . [is] whether it tends to include or to exclude various groups from influence, and whether, if it assigns disproportionate influence to some groups, they are the ones which are relatively shortchanged elsewhere . . ." (p. 60).

Gordon Baker offers a concise and precise review of the cases, and goes on to consider the

mysteries of single-member districts and multi-member districts as well as other things. Note Brennan for the Court: "It might well be that . . . a multi-member constituency apportionment scheme . . . would operate to minimize or cancel out the voting strength of racial or political elements of the voting population. When this is demonstrated, it will be time enough to consider whether the system still passes muster" (p. 124–125). That's pretty naive. Any MMD system will badly penalize minorities given only the assumptions that the majority will vote a full and straight ticket. Furthermore, SMDs will badly penalize minorities to the extent that they are spread equally among constituencies or concentrated in a few. At any rate, the Supreme Court backed out of this thicket in the Marion County (Indianapolis) MMD case, saying in effect that minority losses in this situation were just the fortunes of politics and not a matter for the Court. Earlier Court minorities were somewhat amused.

Douglas Rae gives a formal, symbolic exposition of the effect of reapportionment on the norm of equalitarian democracy. Later Milton Cummings sounds a different note from those expressed earlier. He takes reapportionment as reality, and estimates the probable effect on the membership of Congress given existing population distributions. His estimate is that things are not likely to change much but that differences in the constituencies of the President and Congress may decrease.

In an arresting essay, William Bicker looks at the studies of state political system outputs which hold that apportionment really doesn't matter. He argues fairly that these studies are seriously defective since they do not measure crucial variables very well.

In a final essay, David Mayhew writes on theory and practice in drawing congressional districts. The theory part seems to me a remarkable exercise of independence and good judgment. At one point, Mayhew examines various propositions about the desirability of creating competitive districts, including the proposition that congressmen from safe districts behave differently from others. In writing of "the popular myth that electoral insecurity . . . induces congressmen to be 'liberal' . . ." (p. 257), Mayhew contends that "most of the scholarship on competitive and safe congressmen is speculative rather than conclusive" (p. 258). This and more refreshing cold water in a balanced discussion.

Unlike so many collections, this one is a remarkably successful attempt to cover an important issue from various perspectives. Despite its

title, however, this book will *not* tell the reader what is going to happen to apportionment in the 'seventies. Crystal balls seem to be a bit cloudy. My own best guess is that President Nixon will continue to reconstruct the Court and that the new Court may be open to the making of distinctions, if not reversals. If that is so, then the questions are how will the legislatures be chosen and how able and determined will they be to change what was forced on them in the 'sixties.

THOMAS A. FLINN

Cleveland State University

The Discovery of the Asylum. Social Order and Disorder in the New Republic. By David J. Rothman. (Boston: Little, Brown and Company, 1971. Pp. 376. \$12.50.)

Perhaps I can best review this book by attempting to answer three major sorts of queries: What has the author undertaken, and what does he conclude? How well does he succeed, why and why not? And what special interest does the volume hold for political scientists?

Rothman himself begins by asking the question: "Why did Americans in the Jacksonian era suddenly begin to construct and support institutions for deviant and dependent members of the community? Why in the decades after 1820 did they all at once erect penitentiaries for the criminal, asylums for the insane, almshouses for the poor, orphan asylums for homeless children, and reformatories for delinquents?" (p. xiii). He subsequently declares that proper answers must be located in the social attitudes and republican culture of the period, roughly, 1820-1850; and he concludes that legislators, philanthropists, local officials, and others became convinced that the nation faced grave perils from individual instability, family disintegration, and community disorder. "The asylum, they believed, could restore a necessary social balance to the new republic, and at the same time eliminate long-standing problems" (p. xviii).

Rothman, professor of history at Columbia University, succeeds brilliantly on many counts. He has unearthed source materials hitherto unknown or neglected for the early history of public institutions. He has integrated a vast array of demographic, institutional, reportorial, and contextual data into a clearly controlled structure (thematic as well as chronological) for our better understanding. And, especially, he has freshly recaptured the symptomatic vocabulary of an age. Over and over again his aptly chosen quotations contain the imperative watchwords of anxious Americans: order, reg-

ularity, system, stability, discipline, industry, social obligation, and obedience.

Rothman succeeds less well when he insists upon excessive contrasts between colonial practices/attitudes and those of Jacksonians toward the socially disinherited. He exaggerates the extent of colonial apathy, and minimizes the real continuity of psychological and physical brutality from the eighteenth century long into the nineteenth. Americans went from barbaric punishments on the books, but sometimes unenforced, to institutionalization that was often barbaric and even absurd. Finally, in overdramatizing an attitudinal shift between the 1790s and 1830s, Rothman ignores some basic economic considerations underlying the colonial tendency toward noninstitutionalization: neither the public fisc nor the private purse could readily afford widescale incarceration.

There are many reasons why political scientists should examine this book. Its two concluding chapters on "The Legacy of Reform" and "The Enduring Institution" are excellent and make vital contact with the penal dilemmas of our own time. The ways in which an earlier generation of Americans tried to make policy and rationalize solutions in response to crime, poverty, alcoholism, and insanity are intrinsically fascinating. Moreover, the traditional sources tapped for the history of political and social thought before the Civil War—Marshall and Taney, Kent and Story, Calhoun and Webster—are in many respects less revealing than are popular attitudes toward family and community, liberty and authority, tradition and change, social reform, and the role of public institutions.

In recent years there has been a tendency among students of antebellum culture to view this period in terms of a psychic "identity crisis." Perhaps there was such a crisis initially, in the years before and following the War of 1812, for example. But the crucial years 1825-1850 might better be characterized in terms of a "quest for society." Most of those heady Jacksonians knew *who* they were, but were much less certain about how they were going to relate to one another. Ultimately, as Rothman helps to demonstrate, the quest for community (or society) failed. Perfectionism and utopianism proved to be passing fancies. The potential for individualism proved stronger than the penchant for collectivism. And, most difficult of all, the Jacksonians' sense of the tension between self and society was strangely skewed, in my view, because they juxtaposed a new and naive, optimistic sense of self with an older, more cynical sense of society: relatively struc-

tured, agrarian, and restrictive in its relationships. That incongruous juxtaposition made for misunderstandings then and institutional anomalies thereafter. Many of these incongruities will be discussed by Rothman in a sequel to *Discovery of the Asylum*; it will concern the problems of incarceration in America since the 1890s, and I look forward to its appearance.

MICHAEL KAMMEN

Cornell University

The Jacksonians Versus the Banks: Politics in the States After the Panic of 1837. By James Roger Sharp. (New York: Columbia University Press, 1972. Pp. 329. \$12.50.)

For some time past controversies over Jacksonian democracy have yielded diminishing returns. This book shows that study of politics in the states opens a new field for inquiry and may force the abandonment or revision of several familiar theses. Interest in the Bank question did not die down after Jackson's reelection; it remained the focal point of state politics. In all the states studied, the hard money faction in the party prevailed over the "softs." The dominant ethos of the party was neither rising capitalism nor eastern radicalism.

The Democratic party did not engage in the battle over banks and currency as the party of the entrepreneur in an age of enterprise. In contrast, the party's appeal was in the nature of a plaintive warning against the increasing commercialization and vulgarization of American life. If the laissez faire logic of the Jacksonian anti-bank and anti-corporate argument eventually led to the enthronement of the business ethic in the last half of the nineteenth century, it is one of history's cruelest paradoxes (p. 321).

Has Sharp gathered enough evidence to support a generalization which demolishes an interpretation that has been widely accepted? Mississippi, Ohio, and Virginia are examined in depth; three other states—Louisiana, Pennsylvania, and New York—are considered more briefly. Some other states are noted in general reviews of the different regions. The author suggests that some of Lee Benson's findings in New York are atypical, but he himself may not have covered enough ground to say what is typical. Some apparent anomalies are not examined at all: Why, for instance, were the Whigs so strong in North Carolina and Tennessee amongst the small farmers who elsewhere provided the backbone for the hard money interest? Or why did New Hampshire stand so firmly as a Democratic bastion in New England, and why was Connecticut a marginal

state? However, Sharp can probably give a convincing answer to these criticisms. He has done enough to demonstrate the need to re-examine the whole problem of Jacksonian democracy, and to do so by asking what the Democrats really stood for in the separate states. To have done more at this stage would have postponed (perhaps for years) the presentation of an important argument to fellow scholars. It is important because it has wider implications for the study of American political behavior; for many years political historians have been aware that the Washington story alone is not enough; here is convincing proof that the study of politics in the states may transform the national picture.

A further criticism, which may be more difficult for Sharp to answer, is whether an analysis that depends upon the treatment of a single issue can be adequate. He proves that the bank issue remained vital in the politics of the states he has studied, but it did not stand alone and political behaviour must have been affected also by internal improvements, public and private bankruptcy, public land policy, the tariff, and race relations. How often did one or more of these questions push the Bank question onto the sidelines? Or how often was the popular anti-Bank rhetoric used to promote a less popular aim which was of paramount interest to a group or faction? It is also arguable that the emphasis on state issues has gone too far. Even within his own terms of reference one would expect some examination (particularly in Virginia) of reactions to the abortive Whig attempt to revive the National Bank (as "the Fiscal Corporation") in 1841. A final criticism is that Sharp is sometimes incautious in using political labels. "Conservative" was used by contemporaries, but usually as a term of commendation without modern implications; "radical" was used, but seldom to describe the kind of primitive populism that he associates with hard money; "political broker" is a modern concept which may be useful in analysis but should not be identified as a group with a conscious political purpose. The statement that "at the time of the Panic the Democratic party in the Northeast was firmly in the hands of brokers" (p. 319) does not help one much, particularly when it has to be qualified by saying "this, of course, meant different things in different states." So far from explaining the structure of parties, the incorporation of an anachronistic concept may obscure their character.

Despite these criticisms the book is based on solid research, restores life to a jaded controversy, stimulates further inquiry, and demon-

strates the danger of generalizing about the center before one has looked at the periphery.

W. R. BROCK

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The Growth and Decadence of Constitutional Government. By James Allen Smith. (Seattle: University of Washington Press, 1972. Pp. 287. \$8.50.)

As an intellectual leader of the Progressive movement of the early twentieth century, Professor James Allen Smith was second only to Charles A. Beard. Together, Smith and Beard, perhaps more than any other two men, shaped much of the political thought of the past generation. Considered historically, their chief contribution to the Progressive movement was their explanation of the democratic shortcomings of that era ("the frustration of majority will," "Gilded Age plutocracy," "Tory political practice," "machine politics," etc.) in terms of Constitutional sources. The democratic character of the American Constitution—except for early Abolitionist opposition—had never previously been questioned. In the early twentieth century, however, Smith and Beard were about to launch the first attack. In its initial phase the new approach concentrated on political factors, particularly an examination of the Constitution in terms of the class alignments of its framers. The second phase was economic, an examination of the Constitution in terms of the clash between financial and agrarian interests. James Allen Smith's most widely read and loudly acclaimed book, *The Spirit of American Government* (New York: Macmillan, 1907) was the most authoritative embodiment of the initial, political phase and preceded by six years the publication of Charles A. Beard's *An Economic Interpretation of the Constitution* (New York: Macmillan, 1913), which embodied the second, economic phase.

By 1920, however, James Allen Smith, was "disillusioned and inconspicuous" (p. xvii). Before his death in 1924, he finished the book here being reviewed, *The Growth and Decadence of Constitutional Government*, which dramatizes the depths both of his personal disillusionment and of the dilemma of the liberal reformer in America in any age. The book was first published posthumously in 1930 by Holt, but was almost entirely ignored and sparsely and poorly reviewed. Now, in 1972, it has been reprinted by the University of Washington Press with a concise yet superb new Introduction by Dennis L. Thompson. Before deciding that this book deserved to be reviewed today, I

was urged to be convinced that its republication is more than an act of historical piety. I *am* convinced because even though the historical accuracy of the specifics of a Beardian thesis may no longer be central to the concerns of American political science, the problem of political reform within our constitutional framework certainly is. Thompson is correct when he concludes: "The dilemma of constitutional government was the dilemma of J. Allen Smith and is ours as well" (p. xxiv).

The first problem which Smith soon recognized had been posed by the progressive social legislation he had earlier advocated was the inevitability of its centralizing influence, an influence his Jeffersonian sympathies could not tolerate. He had always been willing to admit that although American constitutionalism was essentially countermajoritarian, it had at least accomplished the original objective of liberal, eighteenth-century constitutional thinkers which was not to establish a "democratic government with the people as the recognized source of authority" but to provide a system of restraints surrounding the exercise of *any* type of political authority (pp. 3, 19). But now, he felt, even this had been lost. Not only had progressive reforms (e.g., initiative, referendum, recall) failed to solve America's political and economic problems, and not only had "democracy" (defined in terms of majority rule) been thwarted by continued judicial vetoes and by increased popular subservience to excessive presidential power, but also "individual liberty" was now being sacrificed on an altar of an increasing centralization. In the end, Smith lost his faith in public opinion completely. "Political majorities, no less than public officials," he wrote, "are prone to take into their own hands as much power as circumstances admit" (p. 163). And so he turned (or perhaps returned) in a nearly Aristotelian sense to a distrust of centralization of all sorts, including state encroachment on local government. He claimed that the difficulties with government increase with the size of the governmental unit, that even state governments are purely artificial, arbitrary divisions while the city is the "natural and organic unit with interests peculiarly its own" (p. 193).

It is perhaps the tragedy of James Allen Smith that he could find no adequate means to the noble ends he postulated. Progressive reform, he knew empirically, had largely failed. Public opinion, he finally concluded, was no suitable substitute. And he never recognized that Jeffersonian agrarianism, with its emphasis

upon the merits of locality, was an outmoded concept. Possibly Walton Hamilton (one of the few reviewers of this book when it first appeared) was correct when he concluded that Smith's first book was too early and this book was too late (*Yale Law Journal*, November, 1930, pp. 152-53). But I do not think so, because what may have been a personal tragedy to James Allen Smith is simply a professional burden each of us must continue to bear. In the 1920s, the problem was stated to be the task of adjusting a cumbersome governmental machinery to changing popular needs. In the 1970s, the problem might be stated somewhat differently (yet similarly) as the task of adjusting an increasingly technological and socialized governmental apparatus to changing popular demands but still within the context of traditional constitutional principles. Does that mean that we should abandon the instrumentalist view of the state as a mechanism to be used only to obtain ends higher than itself, or must we return, like Smith, to the organic, Aristotelian view that the state is an end in itself, a fellowship in which there can be no outcasts and no inequalities? Finally, what about the fundamental question of popular participation? Smith's principal thesis, you see, was twofold: (1) The Constitution contains undemocratic elements; and (2) it was deliberately devised for undemocratic ends. Although few political thinkers today would quibble about the accuracy of these two statements as empirical observations, they will continue to differ about the proposition of some of the Founding Fathers that the Constitution is *wisely* undemocratic. It remains a task of both empirical investigation and normative speculation to determine whether a perfectly democratic instrument can sustain stable government.

Thus, Smith's tragedy, in the end, is our legacy. I doubt that we will be any less self-contradictory in handling these problems than he was. But we must try, and a good place to begin is with Smith's book. It is a general treatise on political institutions and democratic theory. And although it may not be that classic book about which a reviewer hopes to say, "This need never be done again," it certainly goes a long way in plotting the pitfalls and delineating the dilemmas of constitutional government. I applaud its republication. I hope it receives a wider readership this time around. I think the 1970s in America are a propitious time for a second attempt.

WILLIAM C. LOUTHAN

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L'Election Présidentielle des 5 et 19 Décembre 1965. Edited by Gérard Adam et al. (Paris: A. Colin, Centre d'Etude de la Vie Politique Française, Cahiers de la Fondation Nationale des Sciences Politiques, 169, 1970. Pp. 571. \$20.00.)

L'Election Présidentielle de Décembre 1965 is part of a continuing series of election studies of the Fondation Nationale des Sciences Politiques. As the title implies, it is concerned with the first presidential election under universal suffrage in the Fifth Republic. It is an immense work, crammed with articles and data of all kinds. There are, for example, chapters on the strategy of the groups involved in the campaign, the role of the unions, the Church, employers' groups and farmers' unions. In addition, at least one third of the book is devoted to a geographical, attitudinal and statistical analysis of the results. If there is something of practically everybody, however, perhaps one of the main defects is that there is simply too much for any one person. Especially in the last 200 pages or so, the plethora of charts, graphs, tables and diagrams makes one wonder whether the passion for detail and definitiveness is not carried to unnecessary lengths. In the future, one would wish for more brutal editing, or perhaps two volumes, one containing the interpretive essays, the second containing the hard data.

The book raises a number of important questions, but one of the most interesting is the extent to which the 1965 French presidential election led to any serious changes in the structure of the French party system. After all, one of De Gaulle's motives in creating the presidential system in the first place was to force the creation of larger and more representative political parties. Yet it would appear that the 1965 election had no real effect on the party system. From Defferre through Barbu, Lecanuet and Marilhac, to Tixier-Vignancour, the process by which candidates emerged was more like the imperceptible oozing of toothpaste from a leaky tube than a series of conscious choices made by party structures.

Defferre's ephemeral campaign for example, was the result of inspired journalistic and public relations techniques which finally ran up against the less inspired but much more formidable resistance of the Socialist and MRP party machines. As Collette Ysmal points out in her excellent essay on party strategies, François Mitterrand emerged and then won support as the Left candidate both because of his political acumen and because he was wise enough not to

attempt the impossible task Defferre had set himself of trying to reform the very parties whose support he was soliciting for his campaign. Mitterrand shrewdly aimed at uniting the Left party leaders around himself and a common program, nothing more than that. By leaving the party structures untouched he assured himself the support of the party hierarchies. As for the MRP, their nomination of Jean Lecanuet was a defensive gesture against the possibility that a straight fight between Mitterrand and De Gaulle might lose them support on the center. And the remaining minor candidates simply pushed themselves forward onto the political stage.

If the process that led to the 1965 election did little seriously to alter party structures, however, the surprising showing of Mitterrand, who received 32 per cent of the vote on the first ballot, forcing De Gaulle to a second turn, certainly demonstrated the latent strength of the united forces of the Left. But, as François Goguel has pointed out for years now, and as he reiterates in his essay, much of this support rests "less on the existence of factors of an economic or social order, than in the persistence of an old historic tradition, solidly rooted, which leads the majority of the voters in almost a quarter of the French departments . . . to vote for the Left" (p. 407). Equally important, however, is the fact that the 1965 presidential election gave Mitterrand—a man who had never belonged to the Socialist or Communist Left, but to a minor left-wing party and then to a political club—the opportunity to establish himself as a national representative of the Left. When the time came for the old SFIO to reconstitute itself as the Socialist Party, and more recently, when the time came to negotiate a common program between the Socialist and Communist parties, this image, despite the temporary tarnishing during May 1968, was of the utmost importance in attracting the support of the large numbers of traditionally anti-Communist Socialist party members and supporters.

Obviously, it is impossible to discuss any but a few of the themes in a work of this scope and comprehensiveness. It should be mentioned, however, that those who delight in hunting down and then slaying old or new myths will find weapons for their armory here. For example, against those who intuitively assume that farmers would support De Gaulle, there is the article by Marie-Elisabeth Handman and Yves Tavernier to demonstrate that the agricultural unions "intervened directly in the campaign taking a position hostile to General De Gaulle" (p. 286). Or, against those who always assume

an important role for the Catholic Church in French politics, one can point out that the Catholic hierarchy scrupulously avoided taking any position at all during the election. The fact, however, that 23 per cent of practicing Catholics voted for De Gaulle, 40 per cent for Lecanuet and only 6 per cent for Mitterrand might prove to some people that indifference is merely a pose when the dice are loaded.

Finally, the ever-shifting nature of French politics is illustrated in the concluding essay by Jean-Luc Parodi's statement that "the direct election of the principal leader of the people . . . has established a new hierarchy which lowers the parliamentary election to second rank" (p. 535). In the light of the absolute indifference that greeted the subsequent presidential election of 1969 and the excitement stirred up by the legislative elections of March, 1973, it is clear that caution should be the watchword of those who make predictions about French politics.

HARVEY G. SIMMONS

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The Chilean Senate: Internal Distribution of Influence. By Weston R. Agor. (Austin: University of Texas Press, 1971. Pp. 206. \$7.00.)

The publishing explosion in Latin American politics in the last ten years has produced some excellent country-studies as well as a number of monographs on the military, the church, and several Latin American political parties. Yet perhaps because of an assumption that conventional legal institutions are almost meaningless in the unstable politics of Latin America, there has been no previous full-length study of its executives, legislatures, or court systems.

The lack of attention to these institutions may be explicable in "banana republics" where military coups and revolutions are regular occurrences, but there are other countries (Chile is certainly one of them, and so in recent years are Colombia, Costa Rica, Uruguay, and Venezuela) in which these institutions are, in fact, institutionalized and play an important and permanent role in national politics. Chile, as demonstrated by the astounding survival of its constitution and political structure in the midst of intense ideological polarization and social conflict, has the most institutionalized political system of all. In fact, Professor Agor reminds us, the Chilean Congress with a history that goes back to the 1830s is the third oldest national legislative body (next to those of England and the United States) in the world. (Sweden and Holland should be added to the list but Chile still ranks in the first five.)

Rather than study the entire Congress, Agor has chosen to focus his attention upon the Chilean Senate. At the time that he conducted his field research, it had only 45 members (since raised to 50), 43 of whom he was able to interview at length. Elected in separate elections from those for the presidency, it was and is a principal center of opposition to the executive. It has a relatively large bureaucratic staff and a complex structure of committees and decision-making bodies, and it operates in accordance with elaborate written and unwritten rules of procedure (including a 755-page Senate Manual) which demonstrate a high degree of institutionalization.

The book uses approaches developed in the study of the American Congress by Matthews, Fenno, Manley, et al. to examine the structures of formal and informal influence in the Senate, to show how it has maintained its independence despite the increasing power of the executive, to describe the internal environment of its specialized committees and interparty conferences, and to delineate the formal and informal processes of socialization which enable it to act effectively in the "management of conflict." Of interest to students of parties and legislatures is the evidence that the Chilean experience contradicts the generalization drawn from the English and American cases that strong parties mean weak legislative committees and vice versa. All parties, especially those on the left, are strong and disciplined in Chile, and yet the Senate appears to have effective and expert specialized committees. Those who wondered in 1970 how an opposition-dominated Congress could have elected Allende in the Congressional runoff for the presidency will find illuminating the description of Allende's success and impartiality as president of the Senate in the late 'sixties—paradoxically, at the very time that he was also serving as an officer of the Latin American Solidarity Organization—OLAS, a Cuban-inspired group dedicated to the promotion of continent-wide revolution.

An epilogue added when the book was in galley contains the author's initial reactions to Allende's Popular Unity government. It raises questions about the future of civil liberties in Chile as economic pressures on the media make it increasingly difficult for the opposition to express its views. Aside from the epilogue, the rest of the book seems to have been left unchanged from the time that it was written as a dissertation in the late 1960s. Thus in a book published in late 1971, it seems strange to see a reference to the 1968 Salary Readjustment Bill as "a recent example" (p. 8), a description of

the Chilean government as one "which leans to the left" (p. 13), the assertion that the Socialists and Communists have "imperfect access" to the bureaucracy (p. 157) and a discussion of the refusal by the Senate to allow President Frei to travel abroad in 1967 which makes no reference to the constitutional amendment adopted at the end of 1969 which permits presidential trips for 15 days without senatorial authorization. The more frequent use since 1970 of the impeachment procedure against cabinet members also modifies the assertion (p. 4) that impeachments have "seldom passed both houses."

Agor twice (pp. 143 and 158) quotes Eduardo Frei's tribute to the importance of the Senate as a training ground for national leadership in Chile, and perhaps the statement bears repeating as another product of many years in the Chilean Senate faces a hostile Senate, and may be tempted as Frei was in 1967, to resort to measures of doubtful constitutionality. To anyone interested in comparative legislatures, political socialization, or simply why the Chileans have been able to pull off their political balancing act in the midst of economic and social troubles that border on chaos, this book is a most useful contribution.

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Indian Political Thinking from Naoroji to Nehru. By A. Appadorai. (Madras: Oxford University Press, 1971. Pp. xxvi, 189. Rs. 10.)

With few exceptions, notably Gandhi's theory of *satyagraha*, the political ideas of modern India have been neglected in the literature on nationalism, on India, and on political thought. Two reasons are noted in this succinct, informative survey by the dean of India's political scientists. First, the principal concern of "thinking men" in 19th-century India was social and cultural reform, not constitutional change. And even after the formation of the Indian National Congress in 1885, attention was riveted to the important but narrow issue of self-government and the methods of attaining it. The second reason is that the national movement and its ideology drew their inspiration from the West: the first generation of nationalists perceived their task as the transplanting to India of those representative institutions and liberties extolled by British liberalism of the age—institutions and liberties which the Moderates accepted as the norm of a "good society." Many in the succeeding generation were at-

tracted to socialism, some to communism, a few to fascism; and all of these ideological persuasions, which moved the vast majority of Indian nationalists until 1920, were acquired through India's connection with the West.

Dr. Appadorai's survey takes the form of capsule summaries of the political ideas of a score of Indian leaders and intellectuals from Naoroji to Nehru. The scope is broad: a dependent people's conception of imperial rule—its merits and defects, and the duties of the colonial power, especially in the sphere of social reform; the case for self-government and the relationship of political to social change; the methods of achieving freedom, from constitutional agitation and voluntary efforts, to individual terrorism, to *satyagraha*; the concepts of nation, state and nationalism; the definition of a good society, including the optimal relationship of state to individual, the role of religion in politics, freedom, economic equality, the equation of means and ends; the conditions of democracy and its compatibility with caste; and the theory of *sarvodaya*, or "people's socialism."

Dr. Appadorai displays a sure grasp of the literature relating to Indian political thinking during the past century. He has succeeded admirably in extracting the essence of views expounded by Indian nationalists, presenting them often in direct quotation, or in a terse, readable style. He has drawn upon half a dozen types of primary sources: essays, for example those by Dayananda, Tagore, Aurobindo, and M. N. Roy; autobiographies by Gandhi, Nehru, Azad, Prasad, and others; works on contemporary issues, such as those by Lajpat Rai, Aurobindo, and Subhas Rose; periodicals, especially the articles by Gandhi in *Young India* and *Harijan*, and by Rajagopalacharia in *Swarajya*; symposia; and letters—between Gandhi and Jinnah, to and from Nehru, by Sprinivasa Sastri, Golwalkar, and others. The bibliography will be helpful to students, as will the brief biographical sketches and glossary of terms.

Among the early nationalists and publicists the greatest attention is given to Naoroji, Gokhale, R. C. Dutt, Tilak, and Surendranath Banerjea, with occasional references to Ranade, Pherozeshah Mehta, and less well-known figures, such as Sayani and in addition Bomanji, who in 1863 urged limited British immigration to India to stabilize imperial rule (!). Gandhi's ideas, especially those relating to political methods, are treated at some length, as are the views of Aurobindo on religion and politics, and Tagore on nationalism. And among the contemporary figures, Nehru, Vinoba Bhave, and Jaya-

prakash are discussed and the fundamental ideological cleavage between Gandhi and Nehru is laid bare.

The survey also contains, though much too briefly, summaries of uniquely Indian ideas on politics—Dayananda's essay on "The Science of Government," Vivekananda's theory of political cycles, and Narayan's concept of communitarian democracy. The last deserves more attention in the West. While acknowledging the heavy debt to Western thought (in Aurobindo's words, "servile fidelity"), the author discerns some "fruitful ideas" deriving from Indian values and experience: the notion that imperial rule, no less than self-government, must rest on consent, not on force; the view that self-government is not only preferable but necessary to good government; the principle that desirable ends can be attained only through right means; the aim of "welfare for all" instead of the utilitarian maxim; the limitation of material wants as necessary for a good society; the doctrine of trusteeship; *satyagraha*; and the *sarvodaya* on political and economic decentralization. Most of these, however, as the author notes, have not yet been developed sufficiently to be regarded as contributions to political thought.

This is not an original book, nor was it intended to be. It can serve, however, as a very useful introduction for Western students, to the political thought of a dependent people during its struggle for independence. It also reveals the extent to which Indian nationalism was influenced by the West in articulating its claims to freedom. The distinctively Indian dimension of those ideas emerged late in the struggle and was dominated by Gandhi, with Aurobindo, Tagore, Golwalkar, and others on the periphery. This book would have been more valuable had greater attention been given to these Indian ideas. Nevertheless, it will be a handy guide to Appadorai's *Documents on Indian Political Thought 1857-1964*.

MICHAEL BRECHER

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Peasants Against Politics: Rural Organization in Brittany, 1911-1967. By Suzanne Berger. (Cambridge, Mass.: Harvard University Press, 1972. Pp. 298. \$11.75.)

In the social sciences, case studies are a difficult art, because of their contradictory demands. Suzanne Berger must have known this in heading for Landerneau (Finistère) to study the powerful agricultural organizations of that modest rural metropolis. Inspired both by the questions of a political scientist and by the taste for the unusual that motivates an

anthropologist, she spent a year in a Breton "tribe" that must have seemed very strange to a Harvard student. To this surprising choice we owe a masterpiece that demonstrates remarkable competence, reflecting an intimate knowledge of French and Breton affairs as well as sound scientific procedure, a rare capacity to pull out of a unique situation a theoretical lesson of general importance, and a talent for writing that makes the book enjoyable to read.

"Landerneau": the word sums up almost a century of the political and agricultural history of France. Suzanne Berger tells the story in linking local events to national decisions. The refusal of the royalist and Catholic nobles to compromise with the Republic led them to shut themselves up in their chateaux and to take up caring for "their peasants," then to fall to squabbling when some, heeding the Pope's words, decide to betray the cause and to enter the system; for this deed Albert de Mun lost his seat as a deputy. Soon the conflict shifted to one between monarchists won over to the Republic by realism but not conviction and those Catholics who were genuinely republicans: the Sillon movement resonated powerfully and aroused the outrage of the traditionalists. Between the World Wars, the Socialists appeared on the scene, and the rivalry between the conservative Vicomte de Guebriant and the Socialist Tanguy-Prigent expresses in microcosm both local and national history: the Popular Front; the Peasant Corporation of which Guebriant was national syndic; the Liberation, when Tanguy-Prigent became de Gaulle's Minister of Agriculture and founded the Confédération Générale d'Agriculture; the Fifth Republic, when a new generation of young "revolutionaries" finally appears, men more concerned with economic organization than with political games who by violent action succeed in imposing their will on the government and on other producers. What a long road lies between the Breton nobles coming to pay homage to the Count of Paris, the royalist pretender at Jersey in 1887 and the battle of the artichokes in 1961: economics instead of politics; values of efficiency instead of moral values; mass action in place of elite intervention; the defense of successful farmers instead of the defense of all the rural world. At the end of this long road, Landerneau consults with young economists who are Marxists and technocrats from the Ministry of Agriculture. It is as if no longer knowing which saints to pray to, the heirs of the Vicomte have turned to the devil.

The principal mechanisms of this slow evolu-

tion which ends in a brutal revolution are meticulously described by Suzanne Berger, who also emphasizes the permanence of certain structures. In the nineteenth century, this peasant society lived completely on the margins of the French nation to which it was linked only by the mediation of several powerful noble families. The Church was the only institution that included the village and went beyond it. Thus Brittany was monarchist even though the Bretons were not; the handful of nobles who went to greet the Count of Paris represented all there was of political life, and the fight between those won over to the Republic and the faithful was a battle fought out only in the salons of the chateaux. The Sillon, on the other hand, took root in the rectories. A national institution created by the Republic, if not Socialist (like the Office of Wheat in 1936) could become the best reinforcement of Landerneau's authority by allowing this agricultural cooperative to appropriate state powers on the local level, in sum by allowing it to carry out the functions that notables play in peasant societies. This is indeed one of the characteristics of the exercise of power in France:

Each group strives to be sovereign in its own domain and to reduce as much as possible the overlap of functions and the sharing of power that characterize decentralized political systems. It is not difficult to understand why many Frenchmen fear decentralization as a prelude to the disintegration of the state, for, until now, whatever powers autonomous groups have acquired they have tried to exercise beyond the reach and influence of the state (p. 9).

In this isolated peasant society, political life penetrated through the Church—through it and its internal quarrels, and against it when anti-clerical republican notables appear on the scene after the separation of church and state. It is therefore not surprising that today the church and politics are still inextricably linked, for where the grandparents' generation saw one through the lens of the other, the children's generation see the two as the same.

All this would not account for the differences between Cotes-du-Nord and Finistère, which are analyzed in depth by Suzanne Berger, who asks why two neighboring rural societies that are virtually identical should have chosen two different paths to integration in national society: syndical organizations in Finistère, political parties in Cotes-du-Nord. When all is said and done, the only explanation appears to be historical, which of course, simply pushes the explanation further back in time:

The emergence of the corporative group in Finistère and of the political part in Côtes-du-Nord as the dominant modes of linkage of the peasant to the nation depended on the decisions of elite groups, and their choices between the two organizational models were determined by combinations of circumstance, personality, and preference, and not by the press of economic, social, or political factors. . . . Here, as for special movements that have been studied in other countries, the question appears to be which one of a number of "possible" groups manages to organize the peasants first" (p. 161-2).

These examples show that Suzanne Berger has used the case of Landerneau to ask questions so penetrating and so illuminating that her book makes important contributions to the understanding of French society, to the theory of the peasantry, and to political science. It is a fair wager that it will rapidly become a classic in each of these three areas.

HENRI MENDRAS

(Translated by Peter Gourevitch)

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The Theory of Moral Incentives in Cuba. By Roberto M. Bernardo. Introduction by Irving Louis Horowitz. (University: The University of Alabama Press, 1971. Pp. xix, 159. \$7.50.)

Revolutionary Change in Cuba. Edited by Carmelo Mesa-Lago. (Pittsburgh: Pittsburgh University Press, 1971. Pp. xvi, 544. \$14.95.)

These volumes are a welcome addition to the literature on the Cuban revolution. The period of polemics is not over, but it must make way for more scholarly analysis. These two books are part of the new day.

The Mesa-Lago volume is a comprehensive treatment, covering not only the political and economic aspects of the revolution, but also the changes in the social structure and the institutions of education and religion, along with the theatre and literature. Besides the editor, there are sixteen contributors, each an acknowledged authority in his field. Mesa-Lago, himself one of the leading authorities on Cuba, is the author of two very important chapters, and joint author of three others. The book, in general, must be considered the most comprehensive and authoritative coverage of the revolution that has appeared to date. Books such as these are laying solid foundations for further works.

In the opening chapter, "Leadership, Ideology, and Political Party," Professor Andrés Suárez presents indispensable background in-

formation for the subject matter that follows. He rightly emphasizes the central importance of Castro as the charismatic leader throughout the Revolution, and minimizes the role of ideology and the political party. Castro, he says is not an ideologist and has shown little interest in it. Moreover, he has forestalled any tendency toward institutionalization of the Revolution. The Communist Party of Cuba, Suárez notes, fails to meet a crucial test laid down by Lenin as being unrepresentative of workers and peasants.

James Malloy, an authority on the Bolivian revolution of 1950, sees the Cuban revolution as a "modern developmental" one, defined as "an attempt by means of revolution to pull a society out of economic backwardness and set it on a course of rapid economic development" (p. 23). He accepts the Cuban revolutionaries as having "successfully erected . . . a new order . . . out of fragments of the old . . . and with such speed and relative lack of bloodletting that the Cuban experience stands in stark contrast to all other modern revolutions, which went through long bloody epochs . . ." (p. 23). One must agree that a "new order" has replaced the old in Cuba, but question the permanence of the "replacement," as long as the country is unable to support itself.

Cole Blasier, an ex-foreign service officer, and now Professor of Political Science at Pittsburgh, in the chapter on "The Elimination of United States Influence," rightly considers "The Cuban-United States break . . . the single most politically significant aspect of the Cuban Revolution" (p. 43). The chapter presents an excellent review of the salient events that led up to the tragic severance of relations in January, 1961.

This break in Cuba's relations with the United States, as Blasier remarks, left Castro at the mercy of the Communists. "Relationship with the Soviet Union" is expertly described and analyzed by Professor Eduardo Gonzalez in Chapter 4. These sometimes rocky relations, conflicting policies vis-à-vis other Latin American countries are well described. A further elaboration is contained in Chapter 5, "Exporting Revolution to Latin America," by Ernesto F. Betancourt. Stressing the differences between Cuba and other Latin American countries. Betancourt presents convincing reasons why the strategy of export has failed.

The first major section of the volume ends with a provocative essay by Professor Irving Louis Horowitz, "The Political Sociology of Cuban Communism." His theme is the "Stalini-

zation of the Cuban Revolution." Stalinization is defined essentially as involving "emergence of a leader and his small coterie as exclusive spokesmen for the Communist party" (p. 127). Horowitz suggests that Cuba is entering a "new stage" which simplifies the "sociological problem" because it is necessary only "to pay attention to what the leader says and having it stand for what the nation ostensibly believes" (p. 128). This hardly seems a new stage for Cuba, although there was apparently more freedom of discussion when Guevara was on the scene. But all will agree surely that there is only one spokesman for the Revolution.

The section on "Economy" opens with an able exposition of the evolution of central planning, including the choice made among three alternative methods, and the organizational structure designed to implement it. Mesa-Lago, with the assistance of Luc Zepherin, has put together what is undoubtedly the best existing description of this crucial aspect of the Revolution. Cuban planners, the authors conclude, are still experimenting, not unmindful, perhaps, that Castro himself has complained of too much centralization of planning and administration. The authors note a serious failure to allow grass-roots participation in the planning process: "Decisions are made at the top of the hierarchy . . . and the function of trade unions and farmer associations is mainly to endorse the plan and mobilize their membership for its implementation" (p. 180).

In Chapter 8, Professor Roberto M. Bernardo of the University of Guelph, discussed "Managing and Financing the Firm." His book, listed at the top of this review, will be treated below. In this essay he describes and illustrates with charts the structure of both the nonagricultural and the agricultural firm. The Revolution grouped all enterprises engaged in producing the same or similar items into *consolidados*, and the agricultural enterprises into *agrupaciones*. These are the subject of Bernardo's discussion.

With Roberto E. Hernández, Mesa-Lago also shares authorship of an important chapter on "Labor Organization and Wages." Leading authorities on this topic, the authors ably recount the destruction of the free labor movement and the subsequent employment of the organization by the regime as a "transmission belt" to implement its decrees. In short, the Union leaders no longer concern themselves with the welfare and interests of the workers, but rather with enforcing labor discipline in the interest of increased production.

The serious condition of Cuba's international trade imbalance is presented in Chapter 10 by Eric N. Baklanoff. Making judicious and effective use of available statistics, the author contrasts the more favorable conditions that existed in the 1950s with the increasingly unfavorable trends under the Revolution.

Mesa-Lago in Chapter 11 contributes another outstanding discussion, this time on "Economic Policies and Growth." He concludes that although the economy has steadily "deteriorated" since 1961, production should be expected to increase in agriculture with the utilization of larger areas because of land-clearing, increased mechanization, and use of fertilization and irrigation.

Nelson Amaro and Mesa-Lago in their essay, "Inequality and Classes," summarize the effect of the Revolution on the social structure of Cuba. The authors note that old differentials between rural and urban sectors have been reduced, overt discrimination against blacks abolished, although subtle racial differences remain. The wealthy class has been removed from the scene through liquidation, exile and expropriation of property. The lower classes, on the other hand, have had their status improved. The net result has been a trend toward egalitarianism.

What many observers regard as the most important achievement of the Revolution is in the field of education. Professor Roland G. Paulston in Chapter 13 reviews briefly the history of education under the Republic and gives a good exposition of what he terms "Innovations in Cuban Education Under the Revolution." These developments include the construction or renovation of numerous schoolrooms, the impressive gains in enrollment particularly in the primary grades, the literacy campaign, and the establishment of a pervasive program of adult education. Boarding schools which provided food, clothing, shelter, medical care and regular instruction, brought thousands of rural children into the schools.

Mateo Jover Marimón in Chapter 14 reviews the history of organized religion under the Revolution, including the early conflicts and the final development of a *modus vivendi*. Surely one of the best expositions of this problem.

This reviewer is not competent to give any judgment on these chapters: "Theatre and Cinematography" by Julio Matos; and "Literature and Society," by Lourdes Casel.

The essay by José A. Moreno, "From Traditional to Modern Values," should be read in connection with Chapter 12 on "Inequality and

Classes," which it illuminates by describing the modifications in values associated with the trend toward equality and away from "hierarchical elitism."

In the concluding chapter of the volume, Mesa-Lago first summarizes the present situation of the Revolution, then speculates on the future.

In *The Theory of Moral Incentives in Cuba*, Professor Bernardo has selected a limited but highly important aspect of the Revolution for in-depth analysis. As I see the work, it seeks to answer three main questions: (1) Why did the Cuban leaders finally adopt the moral incentives system? (2) What have been the consequences of its adoption? (3) What is the outlook for the future?

In a well-documented account of the "great debate" during the "formative years" of the Revolution, Bernardo argues convincingly "that the leading Cuban revolutionary intellectuals possessed a classical (anti-market) ideology which influenced the choice of organizational means" (p. 3). The debate was between Ernesto "Che" Guevara and his followers and those who wanted something like "market socialism," something midway between capitalism and communism. But, the Guevarists won out, not because the alternative systems could not achieve the desired goals of development and a more egalitarian society, but because of the ideological commitment to classical Marxism.

The second question on how the system has worked up to around 1970 cannot be answered with precision because of the paucity of data. Bernardo attempts to find some usable estimates of the trend of the national income (GNP) over the years since 1958, but with little success. He submits, tentatively, a hypothesis of the variations from year to year based upon what he could find (p. 115); the net trend seems to be a horizontal or even downward line. He lists the sugar production by years, but despite the harvest of 8.5 million tons in 1970, the average annual production showed little change from the pre-revolutionary years. If the author could have added the 1971 crop of under 6 million tons (the target was 7 million) and the crop of 1972 (which the regime has failed to report, but which other sources estimate at a disastrous level under 4 million tons) he would have had to report an even poorer average.

The third point on the possible future of the system can only be answered speculatively, of course. The author suggests for one thing that the "heavy reliance of Cuba on international

trade . . . will call for market-minded experiments . . ." (p. 128). Since 1963, the Cuban leaders have sought to achieve economic development through purchase of needed machines and other items from abroad, paying for them primarily through the exports from agricultural production. Since agriculture has not proved an adequate source of exports up to now, the author seems to think the government will face the necessity of making some compromises. Another factor which may prove important is what he calls the "ideological erosion stemming from the ideological transformation in Eastern Europe" (p. 133).

Bernardo says nothing about the possible dictation by the Soviets of a change which they are in position to insist upon and which would result in greater production than the moral incentive system. For Cuba is increasingly costly to the socialist countries, who may regard the ideology of Cuba as a luxury they can ill afford.

This work is the most extensive treatment of what is and has been a crucial factor in the Revolution up to now. But one suspects that Bernardo would be among the first to disclaim it as "definitive" as the book jacket asserts.

LOWRY NELSON

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Organized Complexity: Empirical Theories of Political Development. By Ronald D. Brunner and Garry D. Brewer. Introduction by Harold D. Lasswell. (New York: Free Press, 1971. Pp. xvii, 190. \$7.00.)

Considerable time has passed since the profession has seen the appearance of a book unabashedly proclaiming the merits of a particular methodological approach, especially since the advent of what some call the postbehavioral era. Ronald D. Brunner and Garry D. Brewer have written such a book, and have done so with brilliant competence and verve. *Organized Complexity* even evokes a nostalgic twinge for an earlier time when battles were fought over methodologies. The authors' enthusiasm for the approach they are advocating in fact interjects one of the only weaknesses in what is otherwise the most important book yet written on the uses of computer simulation for the analysis of political development. Their desire to talk to the general subject of how a computer simulation model can advance our ability to understand *complex systems* on occasion diverts their attention from political development and specifically from the empirical settings—Turkey

and the Philippines—from which the model was created.

The book is organized as two distinctly separate essays, although the model around which the first is built serves as the basis in the second essay for discussing how computer simulation can be put to use to solve problems found in fields having scientific, historical, normative, and policy implications, as well as in such empirical approaches as cybernetics, structural-functionalism, and political culture. Their demonstrations of how computer simulation might be applied to solve problems in these fields are uniformly ingenious, sophisticated, rigorous, convincing. But while they are all these things, the demonstrations are not always germane to the themes of the first essay—modernization and mass politics—despite the fact that each draws upon the model and its Philippine and Turkish applications. The reason for this deficiency is that the authors' primary concern in the second essay is with making a convincing case for their methodological package, not with subjecting their model to critical analysis nor with applying the insights gained from their work as a basis for a probing discussion of the theories—especially political development—to which they would apply computer simulation.

The fundamental proposition underlying both essays is that political systems are complex and that the complexity is not random but organized. The authors build on a position advanced in the scientific community in the late 1940s that simple systems characterized by a few variables are readily amenable to mathematical analysis and that problems of disorganized complexity can be handled by statistical analysis but that structured systems having a large number of interrelated variables demand some alternative approach. They believe that the use of computer simulation in the form they have developed it provides the best method available for dealing with the analytical problems arising from the study of complex systems—or organized complexity—of which political communities are an example.

Their introduction of the actual modeling process is made through a gentle overview of the weaknesses of Daniel Lerner's analysis of the modernization process, weaknesses they see as due largely to his use of regression analysis to delineate *patterns* from cross-national data. They would have developmentalists concern themselves with *processes* of change, and with underlying structures. They share with Lerner recognition of the importance of urbanization to modernization, but insist, in contrast to his

position, that “. . . urbanization need not be treated as an uncaused cause, but rather may be considered in large part a function of relative economic performance in rural and urban areas” (p. 10).

After this introduction, the authors proceed to construct a model containing a set of relationships they believe central for defining the structures and for accounting for the processes of change taking place through time that roughly approximate their understanding of two closely similar empirical political systems—the Philippines and Turkey during the 1950s. The model is built with considerable thought and infinitely careful tinkering. On the basis of an examination of relevant theoretical literature on political and economic development and of case materials from the two nations, they proceed to stipulate the relationships they believe central to the interactions of the rural and urban sectors of the countries and of three subsystems within each sector (demographic, economic, and political).

The complexity of the working model they construct defies verbal description in a short review. In brief outline, however, the model is divided into two-sectors (rural and urban) and incorporates 22 variables and 20 parameters; 12 basic relationships are spelled out in equations governing the model's performance. By way of example, one of these relationships is given its verbal description in this manner: “Expenditures by the government in each sector vary with the number of people in each sector who support the government, the change in the number of supporters in each sector, and with total government revenues” (p. 20). After the initial conditions for the variables have been established and the values of the parameters set, and after “fine tuning” has gotten the ten-year generated runs to approximate the historical time-series, then the model's two reference sets are ready for use. The authors' primary application of the model to the two sets of inputs was to explore the public policy implications of small changes in one or more of the parameters on system performance, e.g., whether birth control policies had significant effects on urbanization rates, or what effects austerity programs in one sector have on living standards and on support for the government. As mentioned earlier, the entire second essay is devoted to discussion and illustrative application of computer simulation modeling to various other fields of political science.

Two weaknesses of the specific simulation-modeling enterprise as applied to the Philippines and Turkey deserve comment. Since the

model is intimately linked with empirical conditions in the two nations, the theoretical constructs and the types and quality of case materials utilized in constructing the model are of strategic significance. An examination of the case materials used for one of the nations—the Philippines—shows that the authors relied on an extremely limited quantitative and qualitative range of description about how the Philippine political and economic systems operate. While their grasp of the theoretical literature seems to permit them considerable room for creativity, the conventional orientation of that literature minimizes the importance of outside influences on the political development process, and thereby reinforces a similar bias in the case materials. The result is that the model contains no provision for inputs into the system from outside on any basis comparable to the 12 relationships built into the model (provision was originally made to have one outside input “read in [to the model] as data” [p. 19], but lack of Philippine data and a decision to drop the variable for Turkey removed even this limited recognition of the larger environment). With one of the two economies basically dominated by outsiders, with each of the nations’ military and paramilitary systems heavily supported from outside, with sizable loans and grants flowing (quite often irregularly) in, and increasingly sizable capital flows (profit remittances; debt management payments) moving out, the decision to construct a model based only on indigenous variables and parameters should, at a minimum, have been defended openly and not developed as if no other theoretical alternative were possible.

The second weakness is lack of data. The authors have displayed considerable ingenuity in generating their own data. For example, out of the 16 initial conditions for the Turkish model, only two are derived from direct measurement. All others were arrived at by intuitively disaggregating national figures into the two sectors. In the case of parameters, none could be derived from independent sources, all had to be generated through trial and error during the early stages of model construction. The result obviously is that the model’s performance cannot be judged a successful or an unsuccessful depiction of the structures and processes modelled until independent measures of the variables and parameters have been found. The authors are perfectly aware of this weakness and are careful to make clear the limits imposed by the data problem.

Despite these reservations, *Organized Complexity* is a masterfully executed argument for

the use of computer simulation in building empirical theories of political development and for its more general use throughout the profession. It is also much more than that: it is an important substantive contribution to the theory of political development.

ROBERT B. STAUFFER

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Revolution in Central Europe, 1918–1919. By F. L. Carsten. (Berkeley and Los Angeles: University of California Press, 1972. Pp. 360. \$11.95.)

In his latest study Prof. F. L. Carsten surveys the upheavals which attended the close of World War I in Central Europe, their immediate antecedents, their successes, and their failures.

Revolution in Central Europe is primarily concerned with events in Germany and German-Austria; there are digressions at appropriate points on Hungary and Czechoslovakia, but these, while perfectly sound, are brief and do not add much to our knowledge of events in those countries. They are largely based on secondary sources in Western languages; the pages on Germany and Austria are based in good part on archival materials.

The heart of Carsten’s book—Chapters 2 through 7—deals with workers’, soldiers’, and peasants’ councils in Germany and Austria, and with the emergent Austrian home defense unit, the *Volkswehr*. Particularly valuable is the extensive coverage of events in the provincial cities and the countryside, as well as in the capitals. Succeeding chapters are concerned with organizations of the extreme right and extreme left: *Freikorps* units, Communist and left Social-Democratic parties. Scholars in search of a consecutive narrative of the events of 1918–19 will be disappointed; Carsten’s focus is not on the policies of the governments of the day or the overall diplomatic picture but rather on the various extragovernmental groupings which shared or disputed power with the recognized authorities. Nor will political scientists find here any attempt at a comparative analysis of events; Carsten is content to trace the course of developments in Germany and Austria without undertaking to set up any political or sociological framework for comparison.

Revolution in Central Europe concludes with chapters documenting frontier conflicts with Poles and Yugoslavs, and the inevitable reaction to the upheavals of 1918–19—the drift to the right and the eventual failure of the revolutions. Carsten’s conclusions are somber, but there are few today who would dispute them.

The councils in Germany and Austria were not revolutionary but auxiliary institutions, which willingly yielded up their authority to their respective National Assemblies when their task was completed; dissension between Communists and Social-Democrats hobbled the Left and opened the door to the Right; nationalization of industry and democratization of society were strictly limited, while the powers of the old regime—the old civil service, judiciary, and General Staff—largely remained or returned; reform affected only the political and constitutional sphere, not the social and economic structure, creating the conditions which were to prove responsible for the weakness of interwar democracy.

Revolution in Central Europe is a lucid, balanced, factual, and thorough study of popular councils, paramilitary organizations, and extreme parties in the German and Austrian revolutions of 1918–19. Political scientists will find in it a wealth of empirical data, from a wide range of primary and secondary sources, on those several sorts of groups, though they will still have to go elsewhere for a comprehensive account of the post-World War I revolutions in Central Europe.

RICHARD E. ALLEN

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Changing Latin America: New Interpretations of Its Politics and Society. Edited by Douglas A. Chalmers. (New York: The Academy of Political Science, Columbia University, Vol. 30, No. 4, 1972. Pp. 193. \$3.50, paper.)

This volume is an outgrowth of papers presented at a conference sponsored by the Academy of Political Science in May, 1972. The goal of the conference was to present "a part of the effort to find a more realistic view of this vast region" (p. ix), and to replace the enthusiastic simplifications of the early 1960s with "a subtler, more realistic appreciation of the complex and profound forces shaping Latin America's future . . ." (p. x). The pursuit of this goal consists of thirteen readable and interesting essays by a judicious combination of academicians and members of governmental and international agencies.

The essays in the book, far-reaching in scope, can be divided in two groups. The first set discusses new challenges for policy and politics. Included here are articles on the areas of major reforms of the 1960s—agrarian reform (Carter), education (Meyers), and population (Viel). Two potential agents of both change and constraint are examined—urban migrants

(Cornelius) and radical priests (Vallier). Arturo Valenzuela offers an interesting analysis of the political constraints in Chile that create a pronounced incongruity between the ideology of socialist planning and the country's incremental bargaining system, the end result being a "politics of conciliation." Finally, Philippe Schmitter trenchantly analyzes previous models of Latin American political development, but goes on to argue for future paradigms that can deal more effectively with Latin American diversity. Contending that "several, if not most, of the Latin American polities are 'spaced-out' along the corporatist-authoritarian path" (p. 94), Schmitter suggests that future investigations explore "three clusters of allegedly determining factors, the Ibero-Mediterranean ethos, delayed development, and external dependence" (p. 95).

The second set of essays deals with the changing bases for Inter-American relations, particularly Latin America's relation to the United States. Topics covered here include Chalmers's critique of "developmentalism" as an explanatory concept for Latin politics, scholarly exchanges between Latin America and the United States (Wood), the increasing importance of relationships among Latin American countries (Agor and Suarez), technology transfer (Morgan), Latin American diversity and United States foreign policy (Bronheim), and the implications for Inter-American relations in the post-Alliance for Progress period (Levinson).

As might be expected, this work, like most collections of essays with a very eclectic orientation, appears frustratingly incomplete and uneven in its coverage. Although only a partial effort is claimed on behalf of the participants in the symposium, one cannot help but question the logic of including full-length articles on urban migrants and clerics as agents of change or constraint, and not providing for a similar article on the military. In Brazil, a conservative, modernizing military regime has effectively coped with a staggering inflation, although it has done so by using political repression and neglecting the thorny problem of income distribution. General Velasco's revolutionary military government in Peru has embarked upon many major social reforms, while Omar Torrijó's highly nationalistic military government in Panama has consistently been at loggerheads with the United States over the future of the Panama Canal. With these and other examples of military "Nasserism" or popularly based dictatorship, one wonders if the neglect of this topic is not a major omission. Although articles

on other topics occasionally mention the military, the absence of more complete coverage mars the symposium's goal of "a subtler, more realistic appreciation of the complex and profound forces shaping Latin America's future. . . ."

Furthermore, this endeavor stumbles in its effort to provide new interpretations of Latin American politics and society. Much of the material criticizes other concepts without suggesting alternatives (as does the editor's own article on "development" and "developmentalism"). It repeatedly emphasizes the diversity within Latin America, and stresses a need to examine changes in fundamental assumptions underlying the United States government's attitudes toward the area. While all of this material is in many ways useful, it does not provide us with the alternative conceptual foci necessary for a more sensitive and realistic understanding of the complexities of the region. An exception is Schmitter's imaginative essay on paths to political development, for it goes beyond critiques of past conceptual efforts to formulate some alternative possibilities.

The articles by such reputable scholars as Vallier and Cornelius are insightful, the former for its typological construction of clerical radicalism and pastoral radicalism and the latter for its synthesis of available data on political attitudes of urban migrants. They are, however, basically syntheses with minor elaboration of material more fully developed elsewhere. These interpretations might be new to some readers but certainly not to individuals working in the area.

Overall, this volume constitutes an adequate but hardly exceptional contribution to a field in which the necessary new interpretations have only begun to appear. It does compare favorably with other existing essay collections. The more noteworthy essays, in my judgment, are those by Schmitter and Valenzuela. The primary value of this undertaking would be for assigned or outside reading in undergraduate classes and introductory graduate seminars.

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Korea: The Third Republic. By Kyung Cho Chung. (New York: The Macmillan Co., 1971. Pp. xii, 269. \$6.95.)

One of the hazards of studying and writing about societies in flux is the relative rapidity with which the fruit of one's painstaking labor becomes obsolete. Herein lies the key to the plight of Kyung Cho Chung's third book on

Korea: No sooner had it been published than it was overtaken by events. The "Third Republic," the subject as well as the *raison d'être* of the book, abruptly disappeared from the scene, being succeeded by the "Fourth Republic" whose constitutional order bears only a faint resemblance to that of its predecessor.

The purpose of his book, Chung writes, is "to present a complete study of the Government of the Third Republic and its political development and an analysis of the critical problem of Korean reunification . . ." (p. x). Although the author does provide ample information on the constitutional-legal framework of the Third Republic, its institutions and standard operating procedures, and the principal events that befell it, it is debatable whether his is indeed a "complete study."

To a political scientist, a major drawback of the study appears to be its preponderantly descriptive orientation. Not only is the book barren of any theoretical concern, either explicit or implicit, but it also lacks any coherent thesis. More than anything else, the book is at once a verbalized organization chart, an annotated constitution, and a journalistic chronicle of the Third Republic. For the experienced student of Korean politics, it offers very little that is new; for the layman it probably provides more factual details than he would ever need or care to know. But, then, the study may not have been intended as a scholarly work. Indeed, there is not a single footnote in the entire volume.

What makes this lacuna disturbing, however, is that the author has incorporated in his book not simply the ideas but the *words* of other people without specifically revealing their sources. That is to say, he quotes without really quoting. To be sure, he does acknowledge at the outset his indebtedness to "authors, publishers, . . . for permission to quote . . ." (p. xi). And he even mentions specific individuals who have "allowed [him] to quote from their valuable publications" (p. xii). Nevertheless, in the main part of the book, one finds passages from other sources with neither quotation marks nor footnotes—a practice which is certain to be frowned upon by the affected authors.

If there are any parts of the book which still remain relevant notwithstanding, or, perhaps, because of, recent upheavals in Korea, they are the two final chapters dealing with democracy and reunification, respectively. In the chapter on democracy, Chung briefly dwells on the possibility that "South Korea's new elite of politically favored businessmen may attempt to lengthen the tenure of President Park to make

him a life-time president, by opening the door officially for the fourth term" (p. 190). Under-scoring the need for an electoral transfer of power, he cautions that, should such a transfer fail to materialize "within a reasonable period of time, public discontent may reach the boiling point, leading to popular demonstrations, coup attempts or revolutions" (p. 191). "These," he continues, "may be momentarily controlled by increasingly repressive government actions, but eventually a political chaos may arise and the president may be deposed by some nonelectoral means" (p. 191). In view of the traumatic events in South Korea since October, 1972—the declaration of martial law, the drastic curtailment of political liberties, and the extension *ad infinitum* of the tenure and powers of President Park in the name of "revitalizing reforms"—one wonders if the pattern postulated by Chung will indeed be set in motion.

In the final chapter of the book, Chung argues passionately that reunification is the only remedy for Korea's chronic problems, exhorting "North and South Korean authorities" to "put forward a proposal to meet at Panmunjom for a transitional step to conduct correspondence, travel, commerce and cultural intercourse between the North and the South" (p. 227). In August, 1971, the two Koreas reached a dramatic agreement to initiate a dialogue at Panmunjom through their respective Red Cross organizations. This historic step was followed by top-level secret political contacts culminating in the publication of a joint North-South communiqué on July 4, 1972. In short, "North and South Korean authorities" have been meeting periodically not only at Panmunjom but also in Pyongyang and Seoul since late 1971. If such contacts have lessened tensions in the Korean peninsula, they have yet to produce tangible results that would pave the way toward reunification.

Somewhat enigmatic is Chung's assertion that "a united Korea is foreseeable only when the great powers and the United Nations will exercise the principles laid down in its Charter" (p. 228). For the greatest impediment to Korean unity appears to be neither the great powers nor the United Nations but the vested interests of the power elites in the two Koreas. In his very last sentence of the book, Chung expresses the hope that "the fourth Republic of Korea will be a 'unified, independent, and democratic' 'Land of the Morning Calm'" (p. 228). What a shame it is that the "Fourth Republic" has thrust itself upon the scene so soon

and that it can flaunt neither unity nor independence nor democracy!

B. C. KOH

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Centre-State Relations Since 1967: An Interim Assessment. By Ramesh Dube. (Delhi: Kanpur Printing Press, 1971. Pp. 96. No price given.)

The author attempts to study the functioning of the Indian federal system since 1967. Dr. Dube points out that "the expression 'Centre-State relationship' has often come to connote the attitudes, actions and interaction of the parties at the Centre and in the States" (p. 2); "The idea of a 'State' within a 'State' is a concept which is becoming familiar to us now" (p. 10). In general, the right-wing parties—the Jan Sangh and the Swatantra—wanted a strong Centre so as to prevent Communist States from going too far, while the two Communist Parties were afraid of a sharp swing to the right in New Delhi, and wanted to limit the possibility of interference by the Centre particularly in West Bengal and Kerala, where they were dominant partners in non-Congress coalition governments. But what complicated further the Centre-State relations was the fact that the coalition politics undermined the powers of the Chief-Ministers of West Bengal, Kerala, and Uttar Pradesh, and this made difficult the settlement of any dispute between the Centre and these States. Although the Centre tried hard to rise above party considerations and conduct itself as the national government rather than as a party, the Centre was unable to evolve healthy conventions to guide its relations with the States; in particular its handling of the State Governor's discretionary power in dismissing non-Congress governments was often partisan.

Finding the confrontation between the Centre and the States "a matter of serious concern" (p. 18), the author argues that the Indian federation is a compromise between a purely federal structure and a unitary government with overriding powers in most spheres allocated to the Centre; it is "a functional devolution rather than a conferment of sovereign rights as envisaged in the idea of a federation of the conventional type. The framers wanted a governmental system in which local initiative was blended with a strong central control" (p. 3). The author argues that the opposition parties had not only been critical, but they cooperated in maintaining and strengthening the Centre-State relations. He, however, suggests that the

Centre should make room for "the urge for autonomy" on the part of the States (p. 50).

The author seems, however, to have been more successful in mapping out than in tackling some of the important political problems in the Centre-State relations. Before 1967 the Centre-State relations could largely be regulated within the framework of the Congress Party; but Dr. Dube does not show to what extent the State Congresses operating within the context of State politics became autonomous centers of power; nor does he refer to extra-constitutional agitations that developed out of political controversies such as those over linguistic reorganization of States, the position of Hindi as the national language, and allocation of resources—controversies which could not be solved within the framework of the Congress. The book not only lacks a proper background, it also lacks analysis in depth of any problem in the Centre-State relations since 1967; in fact, the book is superficial. Much has been written on this subject in India and a number of papers have appeared in scholarly journals including this *Review*, but the book does not make any reference to these. Nor does the author mention the political process leading to the Congress split in November, 1969, and in particular the Centre's helplessness to check the breakdown of law and order in West Bengal—a helplessness that arose partly because of Mrs. Gandhi's dependence on communist support in Parliament. The book is, therefore, very disappointing.

S. C. GHOSH

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Kurt Riezler Tagebuecher, Aufsätze, Dokumente. Edited by Karl Dietrich Erdmann. (Goettingen: Vandenhoeck & Ruprecht, 1972. Pp. 766. DM 142.)

Until his death in 1955 Kurt Riezler, professor at the New School for Social Research, was known to Americans mainly as a classical scholar. Generally unknown, however, were Riezler's political activities in the First World War, during which he produced one of the most significant political documents about Imperial Germany ever published.

In 1906 Riezler became a press adviser in the German Foreign Office. His performance attracted the attention both of Kaiser Wilhelm II, who expressed satisfaction "that such a clever man was won for the press office," and of Chancellor Bethmann Hollweg. From 1912 until July, 1917, as Bethmann Hollweg's closest

confidant and aide, Riezler was at the center of German policy making and recorded his observations and impressions in notebooks and on loose papers. Later in life Riezler saw his early political career, in the words of his brother, "as a detour which distracted him from the actual center of his essence," and he ordered his notes to be destroyed after his death. But the efforts of such friends as West Germany's first president, Theodor Heuss, made publication possible.

On August 22, 1914, the Chancellor asked Riezler "for peace terms and [his] ideas" (p. 201, all quotes are my translation). At the time of the outbreak, Riezler had considered that the goal of German policy was to break up the Entente, not to fulfill dreams of territorial expansion. After the war began, however, his view changed. Riezler's famous "September Program," advocating limited annexations, a hard peace for France and a Belgian vassal state, was a temporary catalog of possible war aims. The Chancellor rejected the document but liked Riezler's idea of indirect domination, a sort of middle road between *status quo* and annexationism. Riezler observed that "Athens fell because of the brittleness of its constitution, because it could only conquer. Rome founded an empire because it understood how to expand its constitution and could incorporate the conquered" (p. 249). The outdated, brittle Prussian constitution should have been replaced by a "central European empire of the German nation."

This new empire would have been like a joint-stock company which could always absorb other firms: Riezler envisioned a "federation of European states around the German Empire in which Germany has a majority of shares, just as Prussia has in the present German Empire" (p. 268). Riezler never denied that this and Bethmann Hollweg's "moderate power policy" had something in common with the war aims of the Pan-Germans and the military, namely the strengthening of the continental and global position of the German Empire. The new structure, however, would not have been an "offensive and seemingly domineering political structure" (p. 365). Riezler advocated copying England's practice of controlling without really seeming to control. Most important to Riezler, such a Europe, with Germany as its heart, could offer "an idea corresponding to the greatness of the time," which could serve as a barrier against the "West European freedom slogan. It would be terrible if this flat spirit should

hold the world captive forever" (p. 210). Riezler never indicated exactly what such a great "idea" should be.

Riezler and Bethmann Hollweg agreed that the sturdiest foundation for this federation would have been the working masses and a supranational movement, not the "junker-industrialist leadership." However, Riezler became increasingly pessimistic that such a foundation could be laid: "All of Germany seems to have fallen into two parts," he observed (p. 435). The split between left and right, which seemed to have been healed in August, 1914, became so aggravated by the strains of war that reform seemed no longer possible, and revolution seemed inevitable. "The wound, if it is not fatal already, will break open again and again" (p. 485).

Inner reforms and what Riezler and Bethmann Hollweg considered to be a "rational foreign policy" proved to be impossible because of Kaiser, military, and Prussian bureaucracy. Riezler especially despised the military, who had no appreciation for the difficulty of political business. He noted that "the concept of the purely military is celebrating orgies here" (p. 207). Germans' respect for the military placed political leaders in a weak position. "No one dares to speak against military necessity" (p. 217). The restraint required for "modern domination" could not be expected of Germany's ruling class, which "only believed in the steam-roller." Bitterly Riezler wrote: "Never was a people more capable of conquering the world and more incapable of ruling it" (p. 461).

Revolution played an important role in Riezler's thoughts at this time, for he had a part in both the Bolshevik and German revolutions. Riezler and Bethmann Hollweg considered a separate peace with a weakened Russia much more important than a revolution, but Russia's determination to stay in the war led to a change of policy. The diary reveals nothing about Riezler's role in the fateful decision to transport Lenin in a sealed train from Switzerland to Russia, but it is certain that only German political interests motivated Riezler. By spring, 1918, Riezler, temporary head of the German diplomatic mission in Moscow, advocated open support of counter-revolutionary forces in overthrowing the Bolsheviks, who, in Riezler's opinion, could neither stay in power long nor be of any help to Germany; "Understanding? An entirely non-Bolshevik thought" (p. 465)! All was outdated, however, by Germany's military collapse and the German revolution, which

Riezler, as the central government's representative in Bavaria, helped to crush. Riezler lamented: "The war ends in revolutions all around us" (p. 455). Riezler saw little hope for the future after the long destructive war. "Humanity will learn as much as nothing from all this misery; it will only wake up with a more confused head" (p. 435).

Riezler's diary is valuable to political scientists because in it he combines first-hand observation of methods of rule, military-political relations, European federation, and early German *Ostpolitik* with speculation on morality and ends in politics.

WAYNE C. THOMPSON

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Politics and Privilege in a Mexican City. By Richard R. Fagen and William S. Tuohy. (Stanford, California: Stanford University Press, 1972. Pp. viii, 209. \$8.50.)

Utilizing the perspective of a single community (Jalapa, the state capital of Veracruz), Fagen and Tuohy have provided a fascinating, well-written account of politics in Mexico. Aware of the deficiencies of many earlier community studies within and outside the United States, the authors have focused their efforts not so much on the issue of who governs or who holds power as on identifying the sources of power and the impact of the nation's governmental system on the residents of a strategically located community. Jalapa is strategic in the sense that as a state capital in the core area of Mexico, it lies at the nexus between national and regional politics in a centralized, hierarchically structured polity. Acknowledging that there is little local autonomy in Mexico, the authors define the scope of their study as including any political activity, whatever its origin, that takes place within the community. Citing previous national and local studies, they embark on an analysis of Jalapan politics as an example of what they consider the norm to be at the subnational level in Mexico, that of a "politically well-managed community" in which the forces of stability rather than those of conflict predominate.

Drawing on their own data and those of other studies outside the confines of Jalapa, Fagen and Tuohy establish that this is a politics characterized by centralized decision making, highly stratified patterns of participation, and considerable inequities in governmental responsiveness to demands and in the distribution of benefits to the citizenry. Because present in-

equalities are so great, and class-based political movements so weak, they seek to explain why there is extensive public support in Jalapa for the governmental system and acceptance of existing social conditions.

Their research findings suggest six reasons: First, access to the policy process is strictly controlled at all levels. Second, limited resources, low status, and restricted leadership opportunities in municipalities contribute to the exodus of local talent to state and federal levels. Third, the channels through which people can articulate their grievances are severely limited. Where public officials demonstrate a response to local issues it is a consequence of their concern with civil tranquility and these men's fears of losing their image as capable managers with hierarchical superiors. Fourth, the political culture shared by the haves as well as have-nots requires acceptance of the status quo as a necessary condition. Fifth, a random sample of political attitudes toward governance demonstrates that while there is near consensus among all groups on the uselessness of voting in local elections and the elitist character of decision making, most citizens feel that they can expect equal treatment by police and governmental officials. Sixth, Jalapa's informal system of governance is the consequence of "an accretion of past practices, now rationalized by bureaucratic methods, supported by those who are most advantaged, suffered by those who are least advantaged, and eased by a national economy that has been more successful than many in generating opportunities and—in their absence—hope" (pp. 130–31).

The outcome of this political system, the authors tell us, is one of dynamic stability, in which postrevolutionary governmental arrangements have shown a marked capability to absorb external or internal challenges to authority. This political order which maximizes the forces of stability and minimizes those of conflict they explain as the consequence of authoritarian practices tempered by flexibility and responsiveness to potentially disruptive issues, wide-scale public acceptance and legitimation of existing authority relations, depoliticization of the community, and generalized apathy.

One of the more interesting insights into Mexican politics to be gained from this book, and one that also has cross-national utility, is the authors' explanation of the prevailing deference toward authority and acceptance of the status quo: for them it is to be understood in terms of the continuance, regardless of the Revolution, of the tradition of corporatist

thinking with its emphasis on "recognition and acceptance of one's station [as] the key to the good society" (p. 40). Later Fagen and Tuohy develop this point further by referring to the "still powerful tradition of Hispanic bureaucratic centralism" that pervades Mexican governmental organizations (p. 137).

In short, as a case study this book combines a precise methodology, conceptual clarity, and imaginative thinking in analyzing and evaluating the material collected.

Regardless of these merits, however, there is a problem of generalization which such a single, discrete study of the unique cannot resolve. I do not raise this point in the abstract, for if anyone writing about subnational politics in Mexico in particular and in Latin America at large has the skills, the resources, and the ability to engage in comparative analysis, it is Professor Fagen. Limiting this observation just to the studies for which he is responsible in one way or another and which the authors cite at random—Antonio Ugalde's analysis of Ensenada, Baja California (*Conflict and Consensus in a Mexican City* [University of New Mexico, 1970]) and Jorge Capriata's "The Political Culture of Marginal Elites: A Case Study of Regionalism in Mexican Politics," a comparative study of two cities in Chiapas—one is inclined to ask how do those cases compare with the findings reported in Jalapa? How do they differ? Can generalizations made here be converted into propositions regarding subnational politics in Mexico? If so, why? If not, why not? If the latter is the case then can one validly generalize about national-state-community linkages from a single instance? Unfortunately these questions are not considered in what is otherwise a fine case study.

LAWRENCE S. GRAHAM

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A History of the Chinese Communist Party, 1921–1949. By Jacques Guillerma. Translated by Anne Destenay. (New York: Random House, 1972. [First published in France as *Histoire du parti communiste chinois, 1921–1949*, by Payot, Paris, 1968.] Pp. xviii, 477. \$12.95.)

Although the Chinese Communist Party (CCP) came to power some 24 years ago, general and comprehensive accounts of the Party's eventful history from 1921 to 1949 are only beginning to appear in print in the West. Guillerma's is one of the more recent arrivals.

Unlike earlier monographs, this study places the birth and growth of the Chinese Commu-

nist movement in broad historical context and yet never loses its focus on the CCP itself. The book devotes a good deal of space to such sociopolitical issues as the CCP's changing relations with the Kuomintang (KMT), the overall Chinese political scene during the Sino-Japanese war, the interaction between domestic politics and the international scene and the shifting world balance of power bearing on the outcome of the CCP's struggle for ascendancy.

The author is fully aware of the constraints placed on the study by the scarcity of reliable information because of the lack of free access to the archives in Peking and in Taipei, and the distorted nature of much of the available official and autobiographical materials. But this disadvantage, faced by all contemporary writers on CCP history, is in part offset by the author's personal experience in China, where he served as a French diplomat in the 1930s and 1940s. Guillermaz conveys a sense of reality not shared by many other armchair historians.

Although the author writes with a historian's pen, the material he presents is of value to political scientists who want a composite picture of what brought the Communist Party to power in China. His accounts of how the CCP administered the regions under its control, first during the Kiangsi period, then during the Sino-Japanese war, and later during the full-fledged civil war, present some coherent patterns anticipating the future administration of the People's Republic beyond 1949.

Guillermaz is quite right in stating that Mao Tse-tung's importance in the CCP during its initial years was not as great as the contemporary official history would have us believe (p. 57, n. 1). But his discussion of Mao during 1921-27 is too brief and scattered, except in one section (pp. 115-117), and no systematic account of Mao's role is given until almost the second half of the volume (after p. 242). Unless Mao's early emphasis on the peasantry and his disagreements with the Comintern-dominated CCP Central Committee are emphatically presented, the ordinary reader would not fully comprehend the reasons why the initial Comintern policy of urban proletarian uprisings in China failed so tragically. The author has glossed over the deep-rooted mistrust between Mao and Moscow dating from very early days. On the other hand, he has taken too literally Mao's 1949 statement about "leaning toward" the Soviet Union (p. 438) without examining the circumstances prompting that unusual statement. But Guillermaz shows good judgment in suggesting that Mao was pragmatic

enough to adapt his views to changing times and circumstances, as in the scaling down of brutality used against the landlords and rich peasants during the Yen-an period (p. 431).

Guillermaz's analyses of the basic issues are, with few exceptions, generally sound. One such exception is his argument that the awakening of the proletariat was a third factor—along with nationalism (May 4th Movement) and the messianic message of the Bolshevik Revolution—contributing to the origin and development of Chinese Marxism. He cites statistics to show the size and urban concentration of the Chinese proletariat around 1920, on the eve of the founding of the CCP. One can find grounds to quarrel with Guillermaz on this point: Not only was the 1.9 million Chinese proletariat three times smaller than the Russian proletariat during the comparable stage; but the Chinese peasant-worker ratio (with the peasant households amounting to 96 per cent of the total population, as Guillermaz noted on p. 9), in contrast to the 2:1 ratio in Russia, rendered meaningless the author's claim about the role of the Chinese proletariat.

Some minor errors or inaccuracies may be noted. Yeh Ch'ing (Jen Cho-hsuan) did not "disappear" as the author claims (p. 62), but joined the KMT after he had miraculously escaped death from the shots of a KMT firing squad in an execution of captured CCP leaders. He moved to Taiwan with Chiang's government in 1949, and became a leading anti-Marxist theoretician. The nominal head of the Party after the Tsun-yi Conference in 1935, which confirmed Mao's *de facto* leadership, was Chang Wen-t'ien, not Ch'in Pang-hsien as the author suggests (p. 257). Ch'in had been the Party's Chairman until then. General Marshall's mission to China should be described as mediation and conciliation, instead of "arbitration" and "reconciliation" as given in the book (p. 384). The date of the purge of Kao Kang, the Manchurian Party boss, is given as 1953 (p. 268), when in fact it was 1954. The Party leader who greeted Mao's arrival in Yen-an was Liu Tzu-tan, not Liu Chih-tan, although the same mistake is made in several other English-language books.

Guillermaz gives the following as the principal "causes" of the Communist victory over the KMT (pp. 442-448 *passim*): (a) centralization of control; (b) better organization; (c) centrality of purpose provided by the Party's ideology; (d) the quality of the cadres; (e) the Party's missionary role; (f) the mistakes of the KMT, and (g) the Sino-Japanese war, which

diverted the attention and sapped the strength of the KMT government and gave the CCP a respite and an opportunity to expand. This diagnosis is quite legitimate; my only question concerns his point regarding CCP's "centralization." Most scholars familiar with the CCP's and the KMT's working styles would agree that the CCP, especially under Mao's command, stressed *decentralized* operations and that Mao's strategy of decentralization enabled his guerrilla and regular army units to attain a high degree of mobility, with which the much more centralized KMT army was incapable of coping. Guillerma could have meant "unity" within the CCP top leadership between 1936 and 1949, free of the factional strife that beset the KMT. If so, I would agree.

In summary, this is a handy, one-volume comprehensive history of the CCP from its inception to its rise to power, interlaced with many useful maps and photographs, some of which appear in book form for the first time.

JAMES C. HSIUNG

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Urban Dynamics in Black Africa: An Interdisciplinary Approach. By William J. Hanna and Judith L. Hanna. (Chicago and New York: Aldine, Atherton, Inc., 1971. Pp. 390. \$9.75.)

There have been two major approaches to social research in African urban areas. The traditional approach was concerned primarily to understand the mechanisms by which rural Africans adjusted to the imposition of colonial cities. Contemporary research, by contrast, tends to be less concerned with adjustment as such, focusing rather on the distribution of power, goods and services in the process of development. The implied purpose of much of the more recent research is to provide for a more equitable and efficient distribution of scarce resources over the whole urban community. The Hannas' book, a review and synthesis of the extensive secondary literature on African cities, emphasizes the first approach. It is divided into two sections: a chapter-by-chapter discussion of the main themes of the literature, and a long bibliography. As a general introduction to African urban conditions, this book should be welcomed both by Africanists and those interested in urbanization in the Third World.

Generalization about African cities is a risky business. Their great variety and number; descriptive material that is often superficial, outdated, and unsystematic; and the very great

changes that have been taking place since independence—all serve to undermine even the most carefully qualified assessments. The Hannas treat this problem gingerly by suggesting that, although they intend to generalize, the statements they make must be viewed as a series of working hypotheses. Early chapters present material on the importance of African cities and their patterns of growth. The authors then move into the literature on rural-urban migration and the whole question of the degree to which African townsmen are "committed" to urban life. This leads logically to a discussion of social structure and, in particular, the importance of ethnicity as a situational variable influencing a wide range of urban choices.

While ethnic structures (neighborhoods, associations and friendship networks) represent an early phase in the adaptation of migrants to urban life, a later phase is characterized by the greater importance of what the Hannas call "quasi-classes." These they define as "influential population aggregates" which are at least "semiautonomous units." Examples of urban quasi-classes are the governing elite, modernists, businessmen, and wage-earners, each of which is described at some length. Unfortunately for the clarity of the argument, these quasi-classes are not mutually exclusive categories (is this why they are only *quasi* groupings?), nor is the political expression of their interests always free of "ethnic" considerations. The importance of these categories in urban politics is in any case downgraded by the authors themselves when they argue, in Chapter 8, that the recent development of class politics is still subordinate to or even submerged under ethnic politics. In fact, some of the best analysis in the book focuses on the different political forms taken by urban ethnicity and the different kinds of conflicts that result. Of particular importance is the situation in which a local ethnic group competes in various political arenas with a "stranger" group for control of a town. Almost invariably the "stranger" group is educationally and economically better off, though numerically inferior to the local group. In this form of competition class and ethnicity, sometimes even religion, merge and overlap so much that they are not easily separable by simplistic models of structural conflict.

While it is difficult enough to describe and analyze the social and economic basis of political conflict in African cities, it is an even more elusive exercise to grapple with the range of forms taken by African urban politics. This is

particularly the case when the unit under discussion—the urban political system—is not clearly differentiated from either the national political system on the one hand, or the myriad of structures that link urban with rural areas on the other. To deal with these apparent ambiguities, the Hannas present a model of an urban political system containing three different subsystems—ethnic, locality, and interlevel—and subdivide each according to whether relations flow through a formal network, an informal network, or a political party network. The utility of this model for comparative purposes will have to be tested with more data than are presented in this book.

Two limitations of the study deserve comment. First, the authors rely too heavily on theoretical propositions from non-African literature, many of which are plausible, but have no demonstrated relevance to modern African cities. For example, an extended discussion in Chapter 9 of the role that “key townsmen” play in integrating the three urban subsystems makes important citations of, *inter alia*, *The American Voter*, a study by Eisenstadt in 1952 of the leaders of immigrant groups in Israel, an article by Lucian Pye written in 1958, Festinger’s work on cognitive dissonance, and a general discussion on industrialization and society by Hoselitz and Moore. How key townsmen *might* function in modern Africa is one thing; how they in fact *do* function to integrate urban subsystems is a more interesting, but still unanswered question.

A second limitation is the book’s relative silence on what we might call “the dynamics of urban policy.” Although a useful chapter is devoted to urban problems such as unemployment, bad and expensive housing, educational deprivation, poor health and sanitation facilities, and “poverty pockets,” no attempt is made to provide systematic explanations for their increasing incidence, or to discuss the range of possible solutions presently being explored by African governments. For example, to what extent can the present income structure be modified to reduce the elite/mass gap in African cities? How can the balance be redressed between bloated, often parasitic capital cities and stagnating rural areas? What kinds of housing policies would bring the benefits of urban planning to the lowest income groups? How can urban political systems be restructured so that a broader mass of citizens can participate in their own local governmental institutions? These are profoundly political questions in essence, although they are often technical in form. But

their answers depend on an approach to urban development that both incorporates and goes beyond the concerns of this book.

RICHARD STREN

*Ministry of Lands, Housing and
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Turning Point in China: An Essay on the Cultural Revolution. By William Hinton. (New York: Monthly Review Press, 1972. Pp. 112. \$5.95, cloth; \$1.75, paper.)

Hinton’s book is essentially a philosophic essay, rather than a political analysis of the Cultural Revolution in China (1965–1969). Hinton, one of the first American Quaker representatives in China, postulates that the Cultural Revolution in China “has not been a struggle over power for power’s sake,” but rather “a struggle between individuals representing conflicting class interests” (pp. 16–17).

Examples cited by Hinton to illustrate his thesis include: (1) the development of “bourgeois elitism and profit motive” in China in the middle ’sixties; (2) the impact of “revisionism” in Soviet Union upon Chinese society; (3) the war in Indo-China; and (4) the “inability of the Chinese Communist Party [under Liu Shao-chi] to lead the country forward while split into two irreconcilable factions” (p. 54).

Although he revised and completed the text while in China during the summer and fall of 1971, Hinton describes events in China in a way that is rather confusing and difficult to follow. A considerable part of his description seems badly dated and gives a misleading impression of how China is run.

Moreover, Hinton’s oversimplification throughout the book seems to betray a certain political bias, e.g., “In China the working class is beating back the bourgeoisie. In the world revolution of today China leads the way” (p. 112).

In spite of many loopholes, the book is still worth reading. Hinton’s essay on the Cultural Revolution in China does offer a third dimension of missing perspectives in our own analysis of Chinese politics.

WINBERG CHAI

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Modernization without Development: Thailand as an Asian Case Study. By Norman Jacobs. (New York: Praeger Publishers, 1971. Pp. 421. \$20.00.)

This exasperating book portrays Thai society and seeks to explain the propensity of that soci-

ety—and presumably others like it—for modernization and development.

According to Professor Jacobs, *Modernization*, is the “maximization of the potential of the society within the limits set by the goals and the fundamental structure (or forms) of the society.” *Development* is something more—the maximization of social potential “regardless of any limits currently set by the goals or fundamental structure of the society” (p. 9). The author tells us further that “. . . qualitative change [is required] in all the crucial focuses of the social order, if development is to be achieved in any one crucial focus of that social order” (p. 12). Thus, it is possible to modernize without “developing,” and Thailand is offered as an example.

Patrimonialism explains this lack of development. *Patrimonial* is defined in terms of the distinguishing properties of seven fundamental societal institutions—authority, economy, occupation, stratification, kinship and descent, religion, and societal arrangements for maintaining and adapting the social order (p. 13). Patrimonial authority is largely a function of personal status plus control over prebends that can go with such status. This authority sustains the moral quality of the system. Therefore the patrimonial economy can legitimately be manipulated by political authority. The essential occupations are farming and statecraft—but not in that order. Kinship and descent arrangements work against the accumulation of wealth; religion legitimates the social order and roles within it; and the statecraft-practicing moral-intellectual elite has a monopoly control over order and change. So goes the definition, or the model, as it is called by the author (p. 5).

The bulk of the book describes Thailand in terms of these seven fundamental “institutions” and exemplifies one of the methodological problems of social scientists: If you posit a comprehensive model, and apply it to a set of phenomena, then, by golly, the phenomena tend to fit the model—and appear to validate it. It is therefore not exactly surprising to find that Thailand is the epitome of patrimonialism.

Some of the most interesting material in this volume is found in three chapters (9, 10, and 11) dealing with religion. They contribute to the portrait of a monumentally coherent social system, distinguished by a superb capacity for persistence and expansive adaptation. Yet the fundamental normative properties of this social system thwart “development.” Thai society fails to maximize its potential. It does not “manifest an open-ended commitment to productive change, no matter what the consequences might

be on existing goals or existing ways of doing things” (p. 9), alas!

This book starts more arguments than it resolves. It is rife with sweeping generalizations and oversimplifications. For example, we learn that Thai peasants are neither economically passive nor indifferent. They hedge against danger; they resist unproven opportunities. In matters of credit and marketing they tend to be exploited. They depend upon informal organization to meet extrafamilial labor needs. But the Thai rural sphere is inordinately more complex than the author's generalizations indicate.

To sum up, there is the making of something interesting and useful here, but it is both overdone and underdone. The patrimonial perspective may indeed do double service as model and as explanation. But on the one hand it does not adequately handle certain salient phenomena—e.g., the “rural sphere” and the economic system. On the other, it doth profess too much—in offering a single, fundamental, coherent thematic explanation of the “modernizing” and “developmental” propensities of something as complex as Thai society.

WILLIAM J. SIFFIN

Indiana University

Revolution in Perspective: Essays on the Hungarian Soviet Republic. Edited by Andrew C. Janos and William B. Slottman. (Berkeley: University of California Press, 1971. Pp. 185. \$10.00.)

The result of a symposium commemorating the 50th anniversary of the Bela Kun regime, *Revolution in Perspective* is a specialist's book conceived along relatively limited lines. The editors specifically make no “claim for completeness or for providing a comprehensive reevaluation of the regime” but rather seek to investigate several of the more controversial or less-illuminated aspects of the short-lived regime. While one might have preferred the inclusion of other topics (such as the response of Hungarian workers to the regime or, more important, the place and impact of the brief Soviet period on subsequent Hungarian history), one cannot help welcoming this addition—modest as it is—to the appallingly small stock of serious work on Hungarian communism.

By far the best written and most scrupulously documented as well as most persuasive essays of the study are Professor Janos's pieces—one on Hungarian politics and society prior to the Soviet Republic, and one on the agrarian question in 1918–19. Working from the hypothesis of the growing gap between the politics of development and the politics of mobili-

zation in a modernizing society, Janos argues for a new look at turn of the century Hungarian society and particularly at the ruling elite in that period. Dismantling the traditional view of an *ancien regime* led by the landed aristocracy, Janos traces the creation and expansion of a new ruling elite, the gentry-dominated bureaucracy, which in time operates (and enters political alliances) according to its own vested interests (which "took precedence over the particular interests of economic classes" [p. 20]). Thus there are examples of significant splits between the landed aristocracy and the ruling bureaucracy, particularly on the issue of economic progress, as well as evidence of changes from traditional methods of rule (though not necessarily from traditional attitudes towards the ruled). Indeed, the eventual undoing of the new ruling class was the growing political awareness and expectations of the working class and peasantry, which eventually created insuperable economic as well as political dilemmas for the ruling gentry. The latter tended to alienate both the landed aristocracy and the growing bourgeoisie in efforts to solve these problems (primarily through economic reward and status rather than popular political participation). The gentry itself, however, became diluted or "corrupted" as it sought to absorb the ascending national minorities or potential middle class. It is not entirely clear why, as Janos maintains, the gentry was willing to sacrifice its own to accommodate talented German, Slovak, and Rumanian youth—on the demand of assimilation—while, according to Janos, it perceived the same absorption of already assimilated Jews as a threat to gentry rule of the bureaucracy.

In his second essay Professor Janos does not dispute but rather supplements the traditional view of Hungarian society, this time demonstrating that the well-known peasant opposition to Bela Kun's policies contained a vocal Left as well as the more generally emphasized Right. This "second agrarian opposition," composed of radical peasant socialists, maintained a large degree of independence, opposing the urban, industrial Socialists on numerous issues, condemning in particular the "new bureaucracy," dominated, they complained, by Jews.

Another essay which seeks to counter many of the accepted views on the competing forces within the new regime is that of Peter Kenez, who unfortunately does not present his case as convincingly as Janos does. While his point may be well taken, i.e., that the divisions within the ruling Hungarian Socialist-Communist Workers Party occurred along issue cleavages

rather than along the lines of former party affiliation (Socialist or Communist), he oversimplifies when he states that there was no difference between Socialists and Communists as such in the postmerger period. He could perhaps more convincingly prove the final unity of the two groups on the key issues of agrarian and foreign policies, but he ignores the differences in attitudes and disputes which went on in practically every "ministry," albeit often because of the views of the Left Communists but no less real because of this.

Professor Keith Hitchins expertly analyzes the perhaps tragic dilemma of the Rumanian Socialists of Hungary—a dilemma similar to all the minorities of the country—and the failure of the Russian-trained universalist Kun regime to provide a satisfactory nationalities policy. Given, however, the editors' stated intention of providing two essays on "external" aspects of the regime, the discussion of Rumania itself and the Socialists within Rumania should have been treated in a separate essay. In view of the critical role played by Rumania in the destruction of the Kun regime, such an essay would have helped place events in their international context. Professor Slottman's entertaining and authoritative essay on the reaction of the Austrian Geistesaristokraten to the Hungarian Revolution of 1919, on the other hand, would seem irrelevant. Whatever the justification for examining the reaction of this group rather than that of leftist circles or the ruling circles in Austria, the result would tend to bear out the author's own suggestion that "perhaps such an inquiry is too blatantly the product of a curiosity that should be restrained" (p. 149). For all that Professor Slottman's fine scholarship can uncover is three people (Stolper, Musil, Kraus) who have anything in some way important to say about the events. Indeed much of this essay seeks to explain the "non-response," which itself might have been of value if it had had any real significance for Austrian society at the time.

The book concludes with an all too brief essay by Richard Loewenthal analyzing the place of the Hungarian Soviet in International Communism. Loewenthal points out that the international movement learned the wrong lessons from Kun's failure, preferring to trace the cause to the lack of a Leninist party rather than to specific errors of the Kun regime or to the basic fact that "the conditions for a purely Communist revolution had not existed in Hungary" (p. 178). While one might argue Loewenthal's contention that before 1919 Lenin did not consider his conception of a party as a

model for others (what of the disputes with Rosa Luxemburg or Lenin's *Imperialism: The Highest Stage of Capitalism?*), Loewenthal accurately points to the impact of the Kun failure on the future policy of the Comintern in its emphasis upon organization and Leninist parties.

GALIA GOLAN

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Run it Down the Flagpole: Britain in the Sixties. By Bernard Levin. (New York: Atheneum, 1971. Pp. 451. \$8.95.)

Among the thousand and one developments that Bernard Levin discerns in Britain in the 'sixties is the decline of the written word "almost as if newspapers' managements and editors were determined to prove Marshall McLuhan right: by the end of the decade the number of journalists whose chief drawing power lay in their prose style had fallen catastrophically" (pp. 342-343). In that case Levin himself is, to use a favorite geological simile, "one of those granite survivals from the Jurassic Age which are occasionally to be found sticking up out of the more recent deposits" (p. 302) in the Holocene press of modern London. For his chief drawing power undoubtedly is his mastery of the written language. Clause upon clause, interwoven with subclauses, bracketed asides, and strings of well-chosen adjectives are used to construct sentences of great length and complexity but great charm and easy flow. Irony, wit, and the imaginative combination of the apparently dissimilar lace the mixture. Readers of Levin's columns in the London *Times* (he has quit the *Daily Mail* since the publisher's blurb for this book was written, to the enrichment of the Top People of Britain) should need no further recommendation than his authorship; for the rest let me state that, of all the books I have reviewed, this is the one which I probably enjoyed most reading.

What does it offer the political scientist? One might first say what it does not provide. It does not constitute a reliable, balanced record of the decade. There are odd slips of fact—thus Lord Robens is referred to as chairman of the National Coal Board since 1962 (p. 148): he actually took up his appointment on 31 January 1961. Rather more, there is the bending of fact to suit the twist of Levin's argument or the flow of his story. Thus, Mr. Wedgwood-Benn's two by-elections in Bristol South-East (referred to as Bristol South-West) took place on 4 May 1961 and 20 August 1963; but the tale of Benn's campaign runs better with the latter described as the second by-election in the constituency "in just over a year" (p. 183). Or, in a

later by-election, the needs of the drama of the folk heroine of the Northern Irish civil rights movement, Bernadette Devlin, dictate the odds over which she triumphed: a "constituency which had previously returned Unionist members with comfortable majorities" (p. 263). In fact, this area has had a Republican majority ever since partition, and Unionist members had only been elected when the Republican vote was split or when part of it abstained because the Republican candidate was too extreme. But it spoils the story to point out that a Joan of Arc was bound to win anyway.

As for balance, Bernard Levin has his own idiosyncratic gallery of rogues. Prominent in it are all men of the law; the pompous hypocrisy which often affects judges and counsel is unmercifully sought out and laid bare. The unfortunate Mr. Henry Brooke, British Home Secretary for two years in the early 'sixties—who had the ill-luck to hold an unpopular office when the satire boom got under way in Britain but whose chief offence is less what he did than being so bad at explaining why he had done what he had done—is flayed alive.

Yet in his treatment of Henry Brooke, as in many other instances, Levin is fulfilling a valuable role: that of portraying the feel of the time, the indefinable mood of the moment. Henry Brooke did become, while holding office, a symbolic hate figure. *Run it Down the Flagpole* captures many such passing intangibles and, for one who lived through the decade in Britain, brings back many memories with a flavor of actuality. Not only of the changing mood of politics: Levin ranges from the burning of cattle in the great foot-and-mouth epidemic (likened to an ancient ritual burnt offering) to the Moors Murders (which includes a characteristic interpolation expressing Levin's dislike of Maoist China), with considerable attention to the changing morals of the decade and the more ridiculous attempts of the Law to regulate that process. An unforgettable chapter gives his appreciation of what he regards as the three great experiences of London operagoers. This volume is a masterly parade of insight, appreciation, experience, and prejudice covering the range of man's activities in Britain during the decade.

MICHAEL STEED

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Village and Bureaucracy in Southern Sung China. By Brian E. McKnight. (Chicago: The University of Chicago Press, 1972. Pp. xi, 219. \$9.00.)

In Western sinological scholarship the poli-

tics and institutions of the Southern Sung period (1127–1279 A.D.) have until very recent years come in for little attention compared to many earlier studies of the Northern Sung (960–1126). Professor McKnight's thorough and lucid monograph treating a highly complex subject very creditably reflects this burgeoning of scholarly endeavor on the Southern Sung. The author also blazes a relatively unexplored trail by penetrating to the most elemental strata of Sung governmental organization that historical resources permit—the village-level subbureaucracy composed of clerks and village officers.

It is the village officers conscripted on a rotating basis from the rural elite, defined as the top four tax classifications of households, that are the focus of this study. While in later dynasties the power and functions of rural control were distributed among several groups, during the Southern Sung, village officers (and in particular the very crucial Superior Guard Leader) were alone responsible for tax collection, the maintenance of local order, the compilation of land and population registers, and other miscellaneous services. Indeed, these village officers, both wielding and yielding to political authority, were the lynchpins in the government's control of society.

Although the author claims only to make a descriptive analysis of the village service system—a goal which he admirably fulfills—he reaches conclusions and suggests generalizations about the Sung state and society that will interest readers of this journal. By way of technically describing the various procedures and criteria for the selection of village service personnel, the central chapters of the book delve into the administrative structure of rural society. The author emphasizes that government's conscious elimination of the rural elite's privileges, allowing them to be exempt from or irregularly to dodge village services and other obligations, made the Southern Sung rural society more equal and homogeneous, less rigidly hierarchical than previously. Wealth became the primary criterion for determining both the distribution of service responsibilities and local influence, so society became more open and fluid, just as the path into officialdom began to widen in the Northern Sung. The long-range potential effects of this change in the rural social order, largely brought about by the growth of a money economy, were completely disrupted by the Mongol takeover, however. Chapter 8, the penultimate chapter of the volume, examines several solutions to the most vexing problem of the village service system—the distribution of

its financial burden among the people—and offers insights into the process of administrative innovation in traditional China. Initiation for reform, we are told, originated with the rural elite themselves who, with subsequent government encouragement, established in some regions public estates from which revenue was derived to pay those performing village services.

One minor criticism of this book: There are several typographical and Japanese romanization errors in the bibliography. Finally, without intending to diminish the considerable value of this work, I would hope that now after constructing the institutional and historical framework of rural society, the author will in the future permit himself to explore Sung society in more theoretical and even speculative terms and to relate his keen preliminary observations on the topic more fully to economic developments.

EDMUND H. WORTHY

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Russian Peasant Organization before Collectivization: A Study of Commune and Gathering 1925–1930. By D. J. Male. (Cambridge: Cambridge University Press, 1971. Pp. 253. \$12.50.)

American specialists on the Soviet Union owe a considerable debt of gratitude to their British colleagues for a noteworthy series of systematic studies of the Soviet Union in the 'twenties. This group of scholars, many of whom are associated with the University of Birmingham, have undertaken the task of filling in the gaps in our knowledge of this vital period, when so many of the basic decisions which ultimately shaped the Soviet system were being formulated. The book under review is an important contribution to this series. It focuses on the traditional peasant commune (*mir*, or *obshchina*), which remained the basic institution of peasant land management in most of the Russian-inhabited territories of the USSR before collectivization.

The author's main thesis is that the structural and behavioral patterns of the commune, Western speculation notwithstanding, in no way facilitated the introduction of the collective farm. Indeed, the commune, as a more or less natural peasant institution—as evidenced by its vigorous spontaneous revival after the Revolution—represented a serious obstacle to Bolshevik efforts to penetrate the village. When the collectivization drive began, the agents of the regime found it necessary to circumvent the commune

or to smash it, since they could not use it for their purposes. Male suggests that very few Bolshevik leaders had any illusions about the commune. Most of them deplored its technological backwardness and its tendencies to preserve traditional social patterns. A few, following Bukharin's Stolypin-like injunction to "wager on the strong," looked with favor on the establishment of independent farmsteads. But the majority found this ideologically unpalatable. In any case, for most of the period, Party leaders recognized their inability to influence village developments. Legislative efforts to increase the power of the rural soviets vis-à-vis the communes were stillborn until the eve of collectivization, as were efforts to enforce improvements in cultivation practices. The harder the regime tried to break the power of traditional village authorities, the more the peasants banded together in their communes. Party influence in the village was minimal. The position of the local soviets was not much better. Dependent on the peasants for much of their financing, the soviets often became tributaries of the communes they were supposed to control.

The author's principal contribution consists in (1) a painstaking comparative analysis of physical characteristics of communes in different regions of the RSFSR—drawn mainly from statistical handbooks and published secondary accounts of the period; (2) an exegesis of basic legislation on land tenure and the formal powers of various local political institutions; (3) a comparative study of budget data of local soviets and communes. The book contains more than fifty tables, several useful organization charts, four maps, three appendices, and a moderately extensive bibliography. Specialists will find much here that is of interest.

Yet the presentation of the materials, the very wealth of detail on certain questions, is sometimes rather disconcerting. The data are not always effectively marshalled in support of the basic themes. One has the impression at times that the argument is merely a vehicle for the author to present his hard-earned data. This impression is reinforced by Male's frequent disclaimers that the data available don't really tell us what we need to know or seem to be seeing. Thus, his very detailed treatment of regional physical variations among communes is accompanied by frequent caveats against hasty generalizations about what the commune in fact was as an institution (p. 53). This is rather a modest reward for wading through so much material.

Similarly, the value of the detailed discus-

sions of legislation is somewhat reduced by Male's admission that most of the important provisions remained a dead letter during the period (p. 109). Again, the actual importance of the matter under discussion does not seem to warrant such thorough treatment.

At the same time, it seems to me that Male needlessly restricts the scope of his study. He purposely chooses to deal with some important issues in a cursory way. Thus, he passes over the questions of the penetration of machinery and modern techniques into the village and the development of a market orientation much too facilely. While this is not true of his treatment of social stratification, he tends to rely rather uncritically on Soviet assessments of intravillage relationships. An analysis of family size as a factor in household socioeconomic status would have helped to clarify some of his own questions concerning the standard Soviet assessments.

There are also a few minor points of interpretation that may disturb the reader. It is certainly at least misleading to assert, for example, that Lenin "did not oppose the Stolypin reform" (p. 158), although it is true, as Male points out, that he had little use for the commune. And there are by now many Western specialists who would quarrel with Male's assumption that "collectivization was a decision made . . . mainly on economic grounds" (p. 212), unless he tells us what he means by "economic."

Naturally, there is little use in criticizing an author for not writing the book the reviewer thinks he should have written. Male is certainly to be praised for the high standards he observes in the interpretation of his data. We should be grateful for the wealth of material he has provided and the insights he brings to the complex problems of the Russian village on the eve of collectivization.

ROBERT F. MILLER

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Sous-Développement et Révolution en Amérique Latine. By Ruy Mauro Marini. (Paris: François Maspero, 1972. Pp. 199. F 14.80.)

Marini's book is mistitled. It deals almost exclusively with Brazil, more specifically with the events culminating in the 1964 coup d'état and its aftermath to 1969. Only in the short introduction and conclusion is there an attempt to link these events with those occurring elsewhere in Latin America. These "underdeveloped" countries are seen as pawns at the mercy of international market manipulations over which they have no control. Since these manip-

ulations are reinforced by the actions of the local bourgeoisie, who in turn benefit from them, the proletariat finds a double barrier to achieving its just rights: In the local arena the bourgeoisie is content to keep in power governments that maintain or even expand its privileged status, while, for a price, nationalism is also sacrificed to the highest international bidder.

Marini is personally acquainted with and closely linked with the Revolutionary Left in Brazil. But such familiarity is insufficient, for the author engages in vast simplifications and his analysis does not go much beyond a chronology of events. There is little grasp of the Brazilian and Latin American background; moreover, the analysis is tainted with Marxist clichés that do little to explain the Brazilian situation and even less to place it in the broader framework of Latin American development. As an example, the author affirms that "the most original Latin American contribution to the world proletarian struggle [is] its international character" (p. 27). This said, he goes on for two long chapters (most of the book) examining capitalism and revolution in Brazil, without demonstrating if/how the Brazilian proletariat may function as an actor in the liberation struggle that presumably other proletariats are waging elsewhere.

Similarly, Marini is all too easily convinced of the revolutionary spirit of peasant leagues, urban syndicates, and even military elements. He fails to explain Che Guevara's difficulties in finding much support for his ideology in the Bolivian *campo*—difficulties that probably would be just as pronounced, if not more so, in the Brazilian interior. Further, ideological blinders prevent Marini from considering the fact that in Brazil, though Marxist ideology did have the support of a few elements in society, the great majority among the less privileged groups seem to have been moved more by personal motives, by nationalistic considerations, by populist-paternalist appeals, and by the lack of clear leadership from the soon-to-be-deposed President Goulart. In addition, the author fails to account for the role of the middle class in aspiring to and imitating upper-class values rather than in serving as a vehicle of revolutionary fervor to lead the lesser privileged elements of society.

In the concluding chapter Marini is optimistic in predicting that the Revolutionary Left will eventually succeed in Brazil and in the rest of Latin America. But again this optimism is not buttressed by facts; the author's ideological faith appears to enable him to dismiss fact and

to forget historical and sociological analysis. The final paragraphs of the books are another testimony to this type of interpretation. Here the author envisages the heirs of Che Guevara as leading and winning the struggle to "expropriate the expropriators," just as they had been doing "in Bolivia, in Chile, . . . and Cuba" (p. 188). By the time the book had been translated and published in France, Bolivia itself was again under a new rightist regime, and neither Chile nor Cuba are quite the models of success that Marini envisages.

Marini's interpretation thus both gains as well as suffers from his personal commitment to the revolutionary cause. It gains by giving the reader an intimate and personal account of the role of the Revolutionary Left in Brazil. By the same token, the personal perspective has prevented the author from transcending his own biases, from extending and deepening his analysis, and especially from dealing with issues posed by other interpretations of the same events. Finally, by explicitly accepting Cuba as the sole model to be followed, Marini fails to explore the possibility of viewing contemporary Brazil in its *own* terms and as another alternative model of economic development. These ideological biases and failures thus limit the value of this book for the serious student of Latin American and, most specifically, of contemporary Brazilian development.

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Struktura i funkcje partii chlopskiej na przykladzie ZSL (Structure and Functions of a Peasant Party: A Case Study of the Polish United Peasants Party). By Zenon Mikolajczyk and Emil Patryn. (Warsaw: Ludowa spoldzielnia wydawnicza, 1968. Pp. 331. 18 zlotys [approx. \$.85], paper.)

System Partyjny PRL (Poland's Political Party System). By Michal Sadowski. (Warsaw: Ksiazka i Wiedza, 1971. Pp. 280. 22 zlotys [approx. \$1.00], paper.)

Studies in the Polish Political System. Edited and translated by Jerzy J. Wiatr. (Wroclaw: The Polish Academy of Sciences' Press, 1967. Pp. 242. 35 zlotys [approx. \$1.60], paper.)

Eastern Europe's gradual post-World War II recovery has been accompanied by the re-emergence of the powerful scholarly traditions in the social sciences which prevailed in a number of these societies prior to Communist rule. In Poland, this trend has meant a new interest in

the work and methods employed by prewar sociologists such as Czarnowski, Chalasinski, Grabski, and Znaniecki. Notable empirical efforts, even in the new and sensitive field of political sociology, have followed in recent years. In this latter area, despite significant political and ideological restraints on research, much of the work done by Poles shows an awareness of and interest in methods and substantive issues taken up in the West. As a result there exists a real possibility of communication and comparative research between American and Polish social scientists.

The Wiatr-edited work, the only one of the three reviewed that appears in English, is obviously of most direct use to American political scientists. This book, composed of ten articles which appear to have been published earlier in Polish journals, confronts a number of subjects central to American empirical and theoretical research. Divided into three sections, the articles cover aspects of "social change and social structure," "political parties and interest groups," and "local politics." Of interest in Part One are Ostrowski and Przeworski's factor analysis of mobilization and modernization processes affecting the Polish countryside, and Wesolowski's detailed discussion of diminishing class differences in postwar Poland. The second section of the book is mainly interesting for its description of Poland's curious electoral system. Also included is a Wiatr article which tries to define Poland's "hegemonic" party system (the dominant Communist Party along with two minor, but "co-ruling," non-competitive parties) in terms of a broad typology of comparative party systems. In the last section, Herz and Jasinska take up the question of interest-group conflict in local affairs, and another Jasinska article analyzes differing political roles adopted by local officials.

A major weakness in the book, probably the result of shortening longer Polish pieces in this translation, is the frequently inadequate descriptions of method and data—this is especially true of the Ostrowski-Przeworski and Jasinska articles. Nevertheless, as a whole, the book offers much to those interested in broadening their courses in comparative political systems by including material on a Communist state.

Mikolajczyk and Patry's excellent effort is a case study of the program, organization, membership, and functions of the United Peasants Party, one (along with the much smaller Democratic Party) of the two minor political formations which continue to exist in present-day Poland. The UPP is a party with 400,000 mem-

bers, compared to the Communist Polish United Workers Party's 2,100,000. An immense amount of well-organized information about this party is presented by the authors, who give the reader a picture of internal peasant party life unavailable until recently for the PUWP. Based to a large extent on surveys as well as statistical data, the book investigates why people join the UPP, how members differ from nonmember peasants, and what role is actually played by the UPP in Polish political affairs.

In contrast to Mikolajczyk and Patry's modest case study is Sadowski's work, a revised and expanded second edition of his *Przemiany społeczne a system partyjny PRL (Social Changes and Poland's Party System, 1969)*. This book, clearly written in the tense political atmosphere of the last Gomulka years, is in reality several rather unconnected works in one. Probably of least interest is the author's lengthy defense of the Communist Party's "leading role" in the three-party system and his harsh attacks on the work of Wiatr and the émigré sociologist Zygmunt Bauman for their inadequate comparative typologies of political party systems. Sadowski also criticizes those sociologists concerned with interest groups. Of much greater value is the author's effort to compare the social bases, occupational structures, and educational levels of the memberships of the three Polish parties over time. Basing his conclusions on these interesting data, Sadowski finds that the Communist Party must be understood to be the single legitimate national political representative, since it completely monopolizes politically involved persons seen to be leaders in society, i.e., the educated, the professionally trained, the urban, the young. In this analysis, the two minor parties are characterized as mere representatives of specific "friendly interests"—the farmers and privately employed craftsmen—which have no possible pretensions to national leadership. That such a view coincides with long-time Communist Party policy cannot however erase the fact that in this work, for the first time, detailed comparative data on the Polish parties are presented in a conceptually interesting fashion by a Polish scholar.

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Dilemmas of Growth in Prewar Japan. Edited by James W. Morley. (Princeton: Princeton University Press, 1972. Pp. 527. \$12.50.)

"What went wrong?" with the Japanese polity in the two decades before Pearl Harbor:

this question set the tone for the first American efforts to understand modern Japanese history and now returns for re-examination in this sixth and last volume of the *Studies in the Modernization of Japan* series.

It is instructive that this volume was not originally planned when the Conference on Modern Japan was launched in 1959. The five volumes then envisioned divided modern Japan into disciplines—history, economics, sociology, political science, and arts and letters—under an overall focus on Japan's "success" at modernization. This was a natural emphasis for the generation of scholars who were in most cases born of the wartime need for Japan specialists and had an active role in the Occupation reforms. By 1958, when this project was conceived, it seemed that Japan had indeed been reformed, and the 1930s receded in importance as a period in which Japan was victimized by a flawed (and ultimately reformable) political system. Recent research on the Meiji Restoration had encouraged a high evaluation of nineteenth century Japan, and the natural tendency was to judge Japan's modernization on a secular scale as a tale of success.

But the disaster of the war was too large and too close to be reduced to an interlude, and the old nagging question of "What went wrong?" could not be avoided for long. So this sixth volume was added midway through the series, and contains thirteen papers presented at the seminar held in Puerto Rico in January 1968. The authors are predominantly historians and predominantly non-Japanese (nine out of fourteen in both cases), so that the overall tone shows no great departure from the bulk of existing scholarly literature in English on modern Japan. James Morley in his introduction provides a thorough and competent review of the historiography (both in Japanese and English) of Japanese militarism, proving if nothing else that historical interpretation of that era has from the start been deeply mired in contemporary politics, particularly on the Japanese side.

Papers by James Crowley and Akira Iriye deal with Japanese decision makers' distorted images of the rest of the world in the 1930s, in the former case vis-à-vis China and in the latter, America. One is left persuaded that the Japanese were indeed astonishingly parochial in their views, although one would wish explanation of why the Meiji Japanese seemed so much more cosmopolitan: a study of the educational system on this score would seem in order. One would also wish for a more generous elaboration on Iriye's suggestion that the Japanese images were distorted partly because they tried

to make sense out of a world which was itself in crisis and made no sense. What, for example, of the revisionist argument that it was American rather than Japanese imperialism which paved the road to Pearl Harbor? This view is of course a political booby trap, being equally popular among the American New Left and the Japanese New Right, but it deserves more attention in this volume.

A similar sense of Japan's confusion in functioning within the contemporary world order (or disorder) is conveyed in Hugh Patrick's lucid analysis of "The Economic Muddle of the 1920's." Assuming but not treating systematically the causal relationship between the economic problems of the period 1930–32 and the rise of militarism, Patrick deals largely with the monetary and fiscal system, offering the suggestion that a single policy change might have made a great difference: the return to the gold standard should have been effected earlier (around 1922 rather than 1930) and at a devalued rate (rather than pre-World War I parity). Such a concrete assertion of how things might have gone right (or at least less wrong) is refreshing in this generally overcautious volume.

George Beckmann and Peter Duus deal with left-wing dissenters who perceived that something *was* going wrong but failed to get their message across—the Japanese Communist Party in one case and professor/politician Oyama Ikuo in the other. One is left with a shrug of the shoulders: The left was too preoccupied with Western political dogma to understand the political realities in Japan, which swept by them without a passing glance. Of far greater interest is Tetsuo Najita's brilliant paper on the politician Nakano Seigō as a representative of an indigenous radical tradition springing from Ōyōmei Confucianism. While leaving unanswered the question of how widely this ideology pervaded the modern political elite, Najita's essay is highly provocative and should prove seminal in the history of modern Japanese political thought.

Only two of the papers deal with the curiously neglected problem of what the conventional men of political and economic power in Japan were doing in the 1930s. Robert Spaulding's essay on the bureaucracy and Arthur Tiedemann's on big business serve to scratch the surface but leave one tantalized for more extended and systematic treatment.

Finally, two provocative papers treat the broad social forces underlying the political developments of the 1930s. Hayashi Kentarō effectively compares the modern political development of Japan and Germany in what should

serve as a model for future comparative efforts. By avoiding an approach which begins with the sticky problem of defining "fascism," Hayashi comes up with lucid new insights into the nature of Japanese militarism in contrast to Nazism. In particular, he suggests that Japanese militarism represented a reassertion of traditionalist conservatism over the brief threat of the radical right, whereas in Germany Hitler was enabled to use the strength of a similar radical rightwing movement to come to power. A rather similar conclusion is reached by Ronald Dore and Ōuchi Tsutomu in their analysis of the role of the peasantry in "what went wrong." Following a cocky but persuasive rebuttal, line by line, of Barrington Moore's arguments on the Japanese case in *Social Origins of Dictatorship and Democracy*, the authors offer an alternative thesis: The Japanese village neither caused nor supported "fascism," it simply offered no resistance to it, being a stronghold of the "collectivist ethic." This still leaves unanswered the question of which classes *did* support fascism, and leaves open the possibility of a modification of Moore's thesis, namely, that enlightened agrarian reform might have made of the peasantry an active force *against* militarism.

The concluding essay by Edwin Reischauer, "What Went Wrong?," makes a dogged effort to pull together some of the interpretive threads presented in this volume, but in his effort to be fair to the diversity of suggestions made, he leaves the entire problem murkier than ever. Read in conjunction with Morley's introduction, this conclusion would suggest that we have moved from consistent if simplistic interpretations to such a tolerance of complexity that we end up with no interpretation at all, merely a list of "factors." Synthesis remains for the future, but many new avenues of approach are provided by these essays.

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The Modernization Imperative and Indian Planning. By Baldev Raj Nayar. (Delhi: Vikas, 1972. Pp. 246. Rs. 30.)

Professor Nayar of McGill University calls this book a product of interdisciplinary research, and sets out to examine the political economy of Indian planning. He attempts to analyze the political factors behind Indian economic policies by tracing the origins and political motivations of the Mahalanobis model, which formed the basis of the Second Five-

Year Plan and Indian development strategy in the 'fifties.

The first chapter argues that in developing countries it is imperative for the governments to formulate programs and policies with the objective of accelerating economic development. Without such efforts, he suggests, any government would find it very difficult to survive. The success of such development programs would depend upon the type of political framework that exists in any particular country. Compatibility of economic policies and the political environment in which they operate, are analyzed in terms of a taxonomic model which distinguishes between the mobilization, the reconciliation, the bureaucratic, and the theocratic systems.

In the next chapter, Nayar tries to test his "model" by analyzing the compatibility of the Indian political system and the economic policies of the government. He begins with a fairly conventional discourse on the theories of balanced and unbalanced growth. It is shown that the First Plan, with its limited aims of providing the economy with a basic infrastructure, was quite consistent with the conciliatory political system in India. The Second Plan, with its emphasis on heavy industry and long-term growth, was more ambitious. It is argued that the Mahalanobis model was akin to the Soviet model. Its so-called "socialist" objectives were reinforced by the views of the political elite and the foreign economists (e.g., Lange, Bettelheim) who advised the planners during that period.

The third chapter contains the crux of the author's thesis. Economists generally specify growth and raising the level of consumption per head as the objectives of development. Improving the level of living is the basic motivation behind development programs formulated in developing countries. The author dissents from this view. He asserts that plans for economic development are rooted in the aims of political sovereignty, military security, and economic independence. Japan, China, and the USSR are cited as examples of countries in which a major motive force behind development programs was the quest for national security. Military strength and political and economic independence are seen as interdependent. The political and economic factors underlying the basic philosophy of the Second Plan are treated as a case in point. To substantiate his argument, Nayar quotes extensively from Mahalanobis and Nehru. Through their writings and speeches it is shown that industrialization is

imperative for economic and political independence and national strength (pp. 109–134). Many of Nehru's speeches and ideas on planning go back to the pre-independence period. From this he concludes that the strategy of heavy industrialization of the Second Plan was rooted in much earlier nationalist political thinking. It was then that the imperative of modernization was born.

The last chapter reviews India's economic performance during 1950–1967, and finds that it compares favorably with the economic stagnation in the first half of the century but not so well with the contemporary growth in other developing countries. The strategy adopted in the Second and Third Plans did provide the country with a substantial infrastructure and industrial base, but at a cost. Most of the expansion took place in the private sector, contrary to the "socialistic pattern." The neglect of agriculture led to a serious crisis in the mid-'sixties. This was accompanied by inflation and unproductive defense expenditure. In terms of social welfare, we are told, matters became much worse, with increasing unemployment and widening inequalities. The dependence on foreign aid did nothing to improve the situation. In contrast to China's growth, India's economic performance appears relatively poor, but this is ascribed to the difference in political systems. (Recent evidence does not bear out the thesis that Indian growth has been less than Chinese.)

It is difficult to accept the author's main thesis. To say that the quest for military security and national political and economic power is *one* of the motivating forces behind development efforts is plausible; but to say that it is *the* raison d'être is gross exaggeration. It is curious to note how often political scientists, setting out with the intention to treat a subject in an interdisciplinary manner and to regard society as a *system*, end up asserting the dominance of politics. In most less developed countries, particularly during the latter half of the twentieth century, raising the standards of living of the mass of the people at subsistence level, is an important propellant in development programs. Indeed, as Nayar himself argues at the beginning of the book, the survival of governments depends upon such an effort.

In his attempt to prove the point, he has drawn on quotations from Nehru's speeches before and immediately after independence. Clearly, it is not difficult to find the desire for political independence and national power in these—it was a part of the nationalist movement. Attempting to reach self-sustaining

growth through import-substituting industrialization was at least partially a reaction to the colonial past with its emphasis on plantation crops for export. It would have been interesting to examine to what extent the alleged trade-off between growth and equality was really a sacrifice of *both* economic growth *and* social equality for the military-industrial complex. But to identify the desire for economic independence with a quest for political and military power is to carry the argument to an extreme. The strategy of heavy industrialization was also prompted by economic factors. The slogan of self-sufficiency stemmed (at least partly) from the foreign exchange constraint on development and the export pessimism of the planners at that time. Economists may have overemphasized these factors, but one would have expected an interdisciplinary study to put them in perspective and to bring out the conflicts, not to dismiss them altogether.

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A Theory of Ideology: The Tanzanian Example. By John R. Nellis. (Nairobi, Kenya: Oxford University Press, 1972. Pp. xii, 217. Shs. 25/—.)

In *Opinions and Personality*, M. Brewster Smith and his colleagues asked, "Of what use to a man are his opinions?" Like other analysts of political ideas, John R. Nellis attempts to answer this question. As a political scientist, rather than a social psychologist, Nellis assesses the usefulness of ideology not only to an individual, President Julius Nyerere of Tanzania, but also to the Tanzanian political regime. Although explicitly rejecting both a functional and an individualistic social psychological perspective, he implicitly does adopt a functional framework. For him, the key dependent variable is system persistence, with the focus on the utility of ideology to the political elite and the governing structures, especially the Tanganyika African National Union and the planning mechanisms. In furthering the persistence of the regime, Nyerere's political beliefs serve to justify the present regime activity, to explain the meaning of events to the Tanzanian citizens, to promise future payoffs, to guide the process of decision making, and to rally mass support behind the elite and their development projects for the building of African socialism within Tanzania. According to Nellis, the ideology of Nyerere performs these functions by serving as a verbal currency, a standard of "deferred payment," in effect a functional substitute for money.

Of what use to a social scientist is this political economy model of ideology? In Nellis's view, a useful model of ideology, although necessarily oversimplified, should postulate assumptions that at least approximate empirical conditions. He wants his model to help explain political events within the independent African territories. His model leans heavily on classical economic assumptions originally devised by Adam Smith. In this ideal type, man is motivated by feelings of envy to seek greater income and status. The variables of supply and demand best explain the behavior of elites and masses. At least in the short run, the citizens' demands for concrete services cannot be met by the limited supply of resources, particularly in a poor country like Tanzania. The elite therefore offer symbolic promises of future payoff as a form of supply. Of course, in the long run, the gap between promise and performance will become accentuated, thus leading to frustration and eventual overthrow of the ruling personnel. Taking a pessimistic view of the prospects for political stability within the new African states, Nellis seems to deny the long-range utility of ideology for system persistence. Instead, he sees income production, organizational efficiency, and the manipulation of information as the most effective ways to maintain the leaders in office.

Of what use to a social scientist is this book? As the title implies, the book consists of two parts: a long articulation of the functions of ideology and an application of the model to the Tanzanian case. The first part contains more strengths than the second. Nellis sketches an original, imaginative account of the functional importance of ideology and its effects on public policies. He skillfully states his major assumptions, clearly justifies their meaning, outlines the deductive implications, and then criticizes their empirical validity within Africa. But the attempt to apply these assumptions to the Tanzanian situation meets with less success. Any ideology, including African socialism, has meaning only within a particular historical context. The same set of ideas has different consequences under diverse cultural and structural conditions. Nellis introduces little information about the precolonial cultural values or structural patterns of diverse Tanzanian groups. He also gives only limited attention to the postindependence power structures. Basing his evidence on fieldwork done in 1965-1966, he relates ideology to policies and policy consequences—especially to village settlement agricultural projects and to the Arusha plan—but not to the process of decision making. We can-

not really estimate the effectiveness of ideology as a guide to decision making, because the description of economic planning remains separate from the treatment of ideology. Furthermore, in the absence of depth interviewing or survey studies, it is difficult to assess the meaning of Nyerere's ideas to the peasants and political elites as well as to ascertain precisely the extent to which his ideology explains the political meaning of events or rallies support behind the regime. Finally, despite the perceptive self-criticisms of his model presented at the end of the book, some of Nellis's assumptions derived from classical English economists may run counter to the African context. The skeptical reader will wonder about the applicability of political economic notions originally devised in an emerging industrial, private-capitalist society like England to explain conditions within agrarian, noncapitalist African territories.

In sum, this book, one to provoke thought about the importance of ideology in the developing nations, probably has greater utility as an explanatory model in general than as an aid to explaining specific political performance in Tanzania.

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Cuba: The Measure of a Revolution. By Lowry Nelson. (Minneapolis: University of Minnesota Press, 1972. Pp. 242. \$10.00.)

In this critical study of the Cuban Revolution, Nelson attempts a systematic appraisal of the Castro regime's performance over a twelve-year period by comparing revolutionary Cuba with the Cuba of old. He has done so largely on the basis of available statistical data on economic production, national income, employment, education, public health, and other similar indicators of development for the pre- and post-1959 periods, and on the basis of the qualitative aspects of economic, social, and political change since 1959. The principal strength of this study for the Cuban specialist in particular thus lies in the compilation of often highly revealing data, and in the comparisons drawn with respect to two distinct periods in Cuba's contemporary development.

As a rural sociologist who studied Cuban society first-hand in 1946, Nelson is able to illuminate many social and economic aspects of prerevolutionary Cuba later distorted by both the Castro regime and its detractors alike. In early chapters, therefore, he examines the island's levels of economic and societal development before the Revolution, and the state of Cuban agriculture prior to 1959. The picture

that clearly emerges—both descriptively and statistically—is of a society that was making advances on several fronts. Hence, Cuba showed encouraging signs in terms of modest industrialization, greater agricultural production and diversification, rising per capita income, and broader educational, health, and welfare benefits.

The balance of the book is devoted to a critical examination of the Castro regime's record in promoting economic development, social change, and political restructuring, with statistical evidence being employed where possible for comparisons with prerevolutionary Cuba. Nelson concedes that the Castro government has shown much greater integrity than its notoriously corrupt predecessors, and that it has produced some notable social achievements—especially in promoting public education and reducing prostitution, and possibly in promoting public health and some forms of cultural change. Nevertheless, he maintains that the costs of Cuba's revolutionary experience have been exceedingly high in terms of human suffering and political repression. Moreover, he finds that the regime's economic performance has been nothing less than dismal when measured against prerevolutionary benchmarks, let alone when measured against the regime's own ambitious economic goals. Thus, he concludes that under Castro "the consequences to Cuba has been an almost unmitigated economic disaster. . . . [Because] aside from fishing, there was not a single item in the range of agricultural and livestock products which after twelve years equalled or surpassed the per capita production of 1958" (pp. 196–197).

Whether Cuba's revolution can in fact be "measured" solely according to Nelson's criteria is another matter, however. To begin with, there are obvious methodological problems that involve the reliability, comparability, and significance of statistical data drawn from both periods in which methods of accounting and reporting differ greatly. Furthermore, a twelve-year time-frame may indeed contain a record of "unmitigated economic disaster," but revolutions after all are not made solely for economic reasons, and initial economic downturns are certainly predictable outcomes of revolutionary upheaval. Finally, what one attempts to "measure" may itself be influenced by one's understanding of the genesis and development of the Cuban Revolution. For Nelson, the "old Cuba" was clearly enjoying some economic and societal improvements prior to 1959. Given this perspective, not only was there little objective necessity for the radicalized revolution that

followed, but also that revolution became highly dysfunctional when measured against particular indices of past development. Consequently, there is a built-in bias in what Nelson has chosen to emphasize and measure. The results of his analysis are further skewed, in turn, because of his pronounced tendency to understate the political, generational, and nationalist tensions as major determinants in the Cuban phenomenon. These aspects, in short, are not fully accounted for in his appraisal.

The first two chapters dealing with the development and consolidation of the revolutionary process, for example, barely touch on the discredited image of Cuba's old political class owing to its identification with corruption and *entreguismo* (national sellout). As a result, there is little analysis of why Castro's generation of 1953 looked upon itself as the only redemptive force in Cuban political life—at war with not only the Batista dictatorship but also the older political generation. Moreover, Nelson underplays the effect of frustrated Cuban nationalism, the legacy of U.S. interventions and economic-political penetration, and Washington's later policy toward Castro in influencing the course of revolutionary development. Thus, hardly any significance is attached to the 1933 Revolution (pp. 4–5)—specifically, the role of the 1930 Generation, the role of Washington in bringing down the revolutionary government, and the repercussions that the failure of both the 1930 Generation and the 1933 Revolution had on Castro's later behavior. Similarly, Washington's ambiguous if not hostile posture toward the Castro regime in 1959 is depicted as "hardly . . . a decisive factor" in shaping Cuban policy, even though it "reenforced the attitudes which . . . were generated during a half century of experience" (p. 24). In brief, Nelson builds his case against the Castro regime by de-emphasizing precisely those factors in Cuba's pre- and post-1959 situation which evidently elude his understanding or empathy.

As a historical account, Nelson's study is additionally flawed by several factual errors: Cuba's War of Independence (instead of "rebellion") began in 1895 and not in 1896, while U.S. intervention itself prevented the Cuban struggle from being allegedly "successful" (p. 4). Batista's army hardly fits the criteria of a "professionally trained and led army" (p. 14). To claim that "such wholesale and vengeful killing as occurred in Cuba [in 1959] was without parallel in the history of Latin America" (p. 19) simply ignores the Mexican Revolution. Castro did not personally head the contingent that arrested Major Huber Matos (p.

21), nor was the latter arrested in May 1959 (p. 25); and Felipe Pazos of the National Bank rather than Manuel Fernández resigned from the Cabinet in November 1959 (p. 27). Finally, the Soviet trade agreement of 1960 provided for the purchase of 4 million tons of sugar over a 4 year period rather than 5 million tons over a five-year period (p. 27), while the Second Agrarian Reform was in October 1963 and not in October 1959 (p. 73).

EDWARD GONZALEZ

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Yugoslavia. By Stevan K. Pavlowitch. (New York: Praeger Publishers, 1971. Pp. 416. \$11.00.)

Realizing the difficulties of writing a compact work on South Slavs, Stevan K. Pavlowitch, with a rare combination of erudition and modesty, has condensed past and current political, economic, social, and religious developments in Yugoslavia into a comprehensive book that will be very useful to all those who are searching for facts rather than fancy, and who are interested in reality rather than sensationalism. Using the method of "geometrical progression through history," he puts the developments in the area into a historical perspective.

The author of the book says that he has been, on occasions, irritated by "the fashionable vulgarizers, ignorant myth-makers, pontificating would-be experts, pompous parrots, and all those who dash off a new book simply to exploit the market, after a visit here, an interview there, and with only a shaky knowledge of the languages involved" (p. 21). He cites several examples of distortions and outright falsifications of historical events in the country which, because of the developments there in the 1940s, have captured the attention of a great many publicists ever since.

Pavlowitch does not indulge in polemics, but he presents the developments in the area inhabited by South Slavs in a straightforward manner, destroying along the way many myths and legends—some manufactured in Yugoslavia, some in the West. Condensing the long history of South Slavs from its beginnings to World War II into some seventy-five pages, he examines the factors which brought the present-day political system in Yugoslavia into existence. With brevity and clarity of analysis, Pavlowitch discusses the various phenomena which are usually described as Stalinism-Titoism, Titoism, monocratism, and polyethnism.

In view of the unavailability of documents still classified by the Soviet and Yugoslav governments, the whole story of the much-exam-

ined Tito-Stalin break in 1948 will not be known for some time. Being aware of the lack of hard evidence, Pavlowitch resorts merely to pointing out the verifiable facts and leaves the detailed and more meaningful interpretations of those events to future historians who may be able to document the motives of the main actors in the Balkan drama; he follows a similar pattern when he describes the Tito-Soviet cooperation in the 1950s and 1960s. His analysis of Tito's dilemma—adherence to Marxism-Leninism versus desire for independence—comes out very clearly as does the issue of the Tito cult promoted inside the country by the Party and outside of it by some intellectuals in the West. He believes that the survival of the regime created largely by Josip Broz-Tito and his co-workers will depend, to a large extent, on the survival of the cult of Tito.

The book provides the reader with comprehensive information on, and analyses of, many relevant issues such as the traditions in both the Catholic and the Orthodox Churches which have strengthened the centrifugal tendencies in Croatian and Serbian nationalisms; the unemployment problem that forced the regime to permit a million Yugoslavs to earn a living abroad by the end of 1970; the effects of tourism which earns foreign currency for the state and, at the same time, exposes the population to foreign influences; the effects of ideology on the older and younger generations; Tito's censorship of the "counter-revolutionary attempt" in literature and arts; the reasons for the adoption of federal division in state administration; the development of Yugoslavia into a land of some fifteen different nationalities; and the orientation of the generation that will attempt to assert itself when Tito dies. Pavlowitch believes that while the generational conflict in the 1940s played into the hands of Tito more than it worked against him, the present generational conflict, twenty years later, is more anti-Party in its effects; and the political trials in Yugoslavia in the 1970s seem to bear out this contention. Indeed, the Tito regime has openly acknowledged the domestic opposition generated by some intellectuals and writers, by emigrés and churches, by the conflicts of generations following the traditional Balkan pattern, and by the unsolved problem of intermingled nationalities in the multinational state.

The work of Pavlowitch brings into focus the unsettled problem of the several national identities, the negative aspects of nationalisms, interethnic antagonisms and their effect on the ruling party. The rising nationalisms—particularly of the Croats and the Serbs—and the tra-

ditional conflict between them have led to speculations that the country will fall apart after Tito dies, but Tito has denied that this would happen. What kind of change will take place in the country following Tito's death is anybody's guess. As Pavlowitch sees it, after Tito's departure the Soviet Union will not allow the situation to deteriorate to its own disadvantage.

This is a scholarly, sober and well-researched book written in a forthright style; and those students of the area, and of comparative politics and international relations who recognize the link between the past, present and future will appreciate this excellent historical reference source.

JOSEF KALVODA

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Nuclear Politics: The British Experience with an Independent Strategic Force, 1939-1970.

By Andrew J. Pierre. (London and New York: Oxford University Press, 1972. Pp. 378. \$18.75.)

Nuclear Politics is a good account of the evolution of the British nuclear force and doctrine. Better focused than William Snyder's *The Politics of British Defense Policy, 1954-1962* (Columbus, Ohio: Ohio State University Press, 1964) and more thorough (although less analytical) than Richard Rosecrance's *Defense of the Realm* (New York: Columbia University Press, 1968), it can be profitably read either as an introduction to the subject or as a memory refresher and should serve as the standard treatment for some time to come.

Since it covers thirty years, the book cannot cover in detail any of the interesting events. And because the author chooses to stick close to the history of the period, he leaves to others the job of formulating and testing propositions with interesting theoretical implications. If the book has a theme, it is the familiar one that "The British nuclear force has been as much a product of politics, economics, military, scientific, and bureaucratic interests, pressures, and perspectives as it has been of strategic analysis and theoretical logic" (pp. 311-12). The role of domestic politics, including the sometimes divergent pressures of public opinion and party opinion, is well documented. Several weapons systems would have been canceled earlier, with less cost and less harm to the defense establishment, had the government not developed a political stake in them (p. 310). Public support for an "independent" nuclear force has remained fairly steady and fairly high, but a greater constraint on the decision makers has been the views of party activists—a strong

group of nationalists in the Conservative party and a group of unilateral nuclear disarmers in the Labour party. Both wanted Britain to play a role independent of America. While neither wing triumphed, each had influence. The former strengthened the hand of Conservative governments in negotiating with the U.S., inhibited the consideration of alternatives to the British force (e.g., merger with a French or with a wider European force), and led Conservative governments to exaggerate the degree to which their deterrent was in fact independent, thereby hindering realistic analysis of Britain's position. The unilateralists temporarily succeeded in putting the Labour party on record as renouncing nuclear weapons and pushed Wilson into the fuzzy pledge that he would "renegotiate" the Nassau agreements if the Labour party gained power in 1964.

Policy was debated within a setting of three basic dilemmas that neither party chose to confront directly. In 1948 Churchill described Britain as "at the very point of junction" of three "majestic . . . interlinked circles"—the Commonwealth, the English-speaking world, and "United Europe." The problem, of course, is that activities in these arenas, while bringing prestige and other benefits to Britain, often conflicted with each other, especially when there were strong demands for using scarce resources for domestic pursuits. Similar dilemmas of choice operated in the trade-offs between nuclear and conventional forces. Although the Conservatives stressed the importance of an independent nuclear force they would not spend the sums necessary to procure it. And while the Labour party talked of the role of conventional armies in world politics it would not pay the high domestic price of re-instituting the draft. The initial attempt to be strong in both kinds of forces led to weakness on both fronts. Thus Pierre points out that the stress on an independent nuclear deterrent which appeared in 1954 and culminated in the 1957 White Paper preceded by several years the development of a viable nuclear force. The third dilemma was posed by the fact that although Britain sought both independence from the U.S. and influence over it, the paths to these two goals often diverged.

These considerations, which run through Pierre's book, undercut the quotation from Zuckerman in the frontispiece: "The decisions which we make today in the fields of science and technology determine the tactics, then the strategy, and finally the politics of tomorrow." At almost every point—from the decision to make the bomb, to the decision to cooperate

with the U.S. insofar as the U.S. would allow it, to develop an independent capability when the McMahon Act cut off information, and to maintain a nuclear force of limited military utility—politics came first. Of course there were unintended technological consequences. Perhaps the most important one was that, as Pierre points out, by trying to match the superpowers in too many areas of military technology Britain spread her resources too thin, failed badly (e.g., the TSR-2 aircraft, the Blue Streak missile), and gave up. Had she been willing to do less, or move more slowly and thereby take advantage of the American experiences, she could have ended up with a cheaper and more independent force, as France did. But even here while technology partly explains the cost of trying to be a major power, politics, especially domestic politics, explains the choices that were made—often inadvertently.

And, in the last analysis, the defense policy produced by the political pressures may not have been so bad after all. The "erratic pattern of weapons acquisition" (pp. 320–21) certainly wasted money, but the foreign policy consequences were limited. A truly independent nuclear force would have been useful mainly as a symbol (which Britain needed less than France) or as a deterrent against a Russian nuclear attack directed at Britain alone (a negligible danger). A large conventional army might have been more valuable, both in gaining support from other European states as a reciprocity for a greater British contribution to NATO and in protecting British interests in the Third World, but it is questionable whether the gains would have been worth the price of continued conscription. The more significant price may have been domestic. Some inconsistency and vacillation was an almost inevitable consequence of the painful loss of power in the world. But as a result of domestic politics and the absence of courageous and creative leadership, these disconcerting weaknesses were magnified, thus helping to undermine British self-esteem and confidence.

ROBERT JERVIS

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Conservation in the Soviet Union. By Philip R. Pryde. (London and New York: Cambridge at the University Press, 1972. Pp. 301. \$14.50.)

Professor Pryde has done a first-rate job of researching, organizing, and writing a cogent account of the most important aspects of conservation in the Soviet Union. For so brief a

book, the coverage is indeed comprehensive, and yet about as detailed as it can be, given the limited amount of factual material that is available to Western writers. Only occasionally might the author have been able to update his information a few years, and those omissions are probably explained by the lag between the writing and the publication of the book. The writing is superb, smooth and organized so as to support the development of basic concepts that lead logically from one thought into another.

The selection of topics reflects the desire to present a complete picture of conservation in the Soviet Union, ranging all the way from basic Soviet precepts to actual practices and problems in some of the more significant areas of the environment. After a brief introduction which provides insight into Soviet rationale regarding conservation, the author provides a fairly comprehensive chapter which documents the development of the conservation movement in Tsarist Russia and the Soviet Union. This is supported by lengthy appendices in the back of the book which include, among other things, excerpts from the 1960 Law on the Conservation of Nature in the RSFSR; recommended outlines for courses in conservation in higher educational institutions in the Soviet Union; the text of the 1968 USSR Land Legislation Act; the 1921 Decree of the Council of People's Commissars on the Preservation of Natural Monuments, Gardens, and Parks; the Text of Water Legislation Principles, and so forth.

These accounts of principles and historical trends are followed by chapters on conservation of Soviet land and soil resources, the *zapovedniki* (nature preserves) in the Soviet Union, management of fisheries and wildlife, the extraction and conservation of timber and mineral resources, water resource utilization and conservation, and environmental pollution and environmental quality in the Soviet Union. These chapters are sprinkled with case studies of some of the better-known conservation problems in the country, such as the Caspian Sea problem and the Lake Baykal controversy, and are further supported by additional appended materials on endangered wildlife species, statistics on rivers and other water resources, and so forth.

The last chapter summarizes attitudes, problems and trends in conservation in the Soviet Union, and happily includes brief accounts of Soviet theory and practice toward population without which a study of conservation could not be complete.

Thirty-one pages of bibliography attest to the thoroughness and balance of the author's research. In addition, the many homely insights reveal a first-hand intimacy with the Soviet scene.

In short, I have read the book from cover to cover and have found nothing particularly wrong with it. This is my highest form of praise. The book is a timely piece that serves as a nice companion volume to Marshall I. Goldman, *The Spoils of Progress: Environmental Pollution in the Soviet Union* (Cambridge, Mass. and London: the M.I.T. Press, 1972, pp. 372). The two books cover some of the same ground regarding Soviet attitudes toward conservation and historical background of the development of the movement in the country. But Goldman's book is more narrowly directed toward pollution problems, whereas Pryde attempts to span the broader field of conservation in general. And, of course, Pryde's book is written from the geographical point of view, while Goldman's book is more an economic treatise. The two books together provide an excellent conceptional and factual account of environmental problems in the Soviet Union and governmental reactions to them.

PAUL E. LYDOLPH

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Dictatorship and Armed Struggle in Brazil. By João Quartim. Translated by David Fernbach. (New York: Monthly Review Press, 1971. Pp. 250. \$6.95, cloth; \$3.45, paper.)

Quartim sets three related goals in this volume: (1) to describe in Marxist terms Brazil's class structure in the present period of dependent capitalist development; (2) to analyze and draw lessons from the experience of the armed revolutionary movement of the late 1960s; and (3) relying on the foregoing analyses, to propose a series of tactics best suited to overthrowing the military government and ultimately establishing a socialist regime. As a polemic based on "intimate knowledge" (cover flap) of the armed opposition movement, it succeeds far better in the latter tasks than in the first.

Of the two major tendencies in the contemporary Brazilian revolutionary opposition movement, described here as the militarist and the Leninist, Quartim casts his lot with the latter and castigates the militarists for an adventurism destructive to the entire movement. He criticizes them for rejecting traditional Marxist-Leninist methods of building a mass movement and for reducing organizational structure solely to the armed groups advocated by Régis Debray in *Revolution in the Revolution?* Detail-

ing several operations undertaken by militarist groups which brought disastrous results for the revolutionary opposition, he accuses their leaders of "forgetting that the annihilation of the enemy's armed forces required the preservation of [their] own" (p. 212), and he warns that such tactics "may end with the death of an entire generation of revolutionaries" (p. 237). The author also describes how some armed groups, their priorities distorted by their initial success in the preparatory, urban stage of guerrilla warfare, failed to move on to the next, rural stage. Yet only in the countryside can armed opposition be feasibly coordinated with organization-building among the people.

In seeking to refine the Leninist position, Quartim prescribes: (1) a national revolutionary party to coordinate the clandestine as well as legal operations while pursuing the revolutionary interests of the working class, and (2) a front to enlist multi-class support, based on a minimum program demanding an end to the dictatorship, expulsion of imperialism, liquidation of monopoly capital, agrarian reform, and workers' control of production. To be implemented successfully, such a strategy calls for attracting the urban and rural masses into dynamic organizations by championing their deeply felt grievances against employers, landlords, and the state.

Regarding Quartim's first task, the lengthy historical introduction and hasty analyses of Brazil's socioeconomic structure offer provocative hypotheses but are replete with factual error. He seldom documents his sources, and his statistics of automobile, tractor, and wheat production and of land tenure patterns simply do not jibe with those in Brazil's *Anuário Estatístico*, in some instances through obviously faulty transcription and at other times because he has relied on erroneous secondary sources. Other examples abound.

The historical section reveals a serious misunderstanding of the theory and practice of Brazilian corporatism. Corporative theory was not a "borrowed" accretion to Brazilian jurisprudence. Rather it has constituted a major current of Brazilian political thought in this century. And though the corporative institutions have indeed worked (and still do) to prevent the autonomous organization of the working class, co-optation is a more important mechanism to achieve this than the government imposition of union heads which Quartim alleges. The genius of the corporative system established by Vargas in the 1930s lay in its creating conditions whereby docile and corrupt

union bosses emerged from the working class itself.

When the author observes that Vargas's Estado Nôvo failed to survive the end of the Second World War, he overlooks the fact that its basic corporative institutions remained in the new liberal democratic state (1946-1964), enabling political elites to continue controlling the working class. Given the continuity of these corporative institutions, I cannot agree with Quartim that the coup of 1964 meant "the creation of an entirely new state" (p. 53). Quite the contrary, it represents a concentrated effort to make the corporative institutions of the Estado Nôvo work more effectively for Brazil's dominant classes.

Despite his weakness in documentation, the author does advance an interesting hypothesis about the "pact of dependence" between the elites of Brazilian and foreign capital. Countering the argument that since 1964 the military government has dealt a fatal blow to the Brazilian bourgeoisie, he claims that these capitalists, as junior partners of international monopoly capital, expanded their access to markets abroad. For their part in the bargain, giant multi-national firms consolidated their control over heavy industry, and Brazilian state capital received vast foreign credits to expand its activities in the economic infrastructure. Hence the continuing utility of class analysis and dependency theory, for the increasing dominance of foreign capital has lent new vitality to the Brazilian ruling class instead of destroying it.

To conclude, Quartim's slipshod research does not diminish the interest of his hypotheses about recent socioeconomic changes in Brazil. More important, his constructive criticism of the armed opposition movement, backed up by fine descriptive analysis, offers a far better plan for ultimate triumph than Debray does.

KENNETH PAUL ERICKSON

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War Comes to Long An: Revolutionary Conflict in a Vietnamese Province. By Jeffrey Race. (Berkeley: University of California Press, 1972. Pp. xxiii, 299. \$11.95.)

Lest one think that everything about Vietnam has been said, along comes a major work on the origins and development of the Vietnamese Communist revolution that provides exciting new material for the historian, the student of revolution, and the policy analyst.

Race's book, which examines the conditions of revolutionary war in one Mekong River

delta province, offers first of all an original and convincing explanation of the NLF's evolution. Race disputes both the argument that the NLF was always Hanoi's tool and that it developed entirely independently of northern direction. His careful sifting of revolutionary documents and close familiarity with local politics lead him to suggest: that after the 1954 Geneva Conference, the NLF anticipated coming to power when Diem's regime collapsed; that it moved fitfully from political to armed struggle as Diem's suppression campaign took effect; that Hanoi's hesitancy to support armed revolution was a source of great consternation to southern cadres; and that Hanoi finally changed its general directives to the southern movement only when, in 1959, it became apparent that armed revolution was the only recourse to the NLF's disintegration. Thus, the north *did* provide its southern compatriots with strategic guidance, and *did* direct after Geneva that southerners bury their arms for the anticipated day of takeover. But the north consistently, in the latter half of the 1950s, had to respond to the activities of a purely southern movement that was contending with an oppressive government carrying out a reactionary land-[back]-to-the-landlord program. The NLF was independent of external support, though not of external guidance.

War Comes to Long An is equally impressive in its contrasting evaluation of the sources of NLF success and government failure in village leadership. The NLF did well in Long An because it developed and implemented a comprehensive program of social change that directly appealed to peasant interests. Its multidimensional revolutionary strategy included a stronger and more locally relevant organization and an attractive program that emphasized the redistribution of wealth and status. Race's analysis is a useful antidote to those rather superficial models of revolution that exaggerate the importance of "insurgent" organization.

By the same token, his book is useful in providing analytic tools for critically assessing American counterinsurgency tactics, such as the Phoenix operation, which stressed violent methods of uprooting the so-called Viet Cong infrastructure. The basic failure of such programs lay in the American and South Vietnamese inability to comprehend the NLF's social revolutionary nature. Race makes a solid case that no government strategy based on new tactics, new weapons, or even a new distribution of resources could win. Only a strategy that si-

multaneously instituted radical social policies and refrained from responding with violence to the opponent's successes could hope to compete with the NLF.

The author leaves it to others to extrapolate from the Long An experience to the rest of Vietnam, from there to the rest of Southeast Asia, and possibly even to revolution and intervention throughout the Third World. But the implications are strong enough: Long An is revolution and counterrevolution in microcosm, wherever the forces of rapid change confront entrenched power and privilege backed by foreign guns.

MELVIN GURTOV

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The Sword and the Scepter: The Problem of Militarism in Germany. By Gerhard Ritter. Translated by Heinz Norden. (Miami: University of Miami Press, 1969): Vol. I: *The Prussian Tradition, 1740-1890* (338 pp. \$15.00). Vol. II: *The European Powers and the Wilhelminian Empire, 1890-1914* (328 pp. \$15.00).

It may, at first blush, seem odd to recommend that these two volumes be read in conjunction with David Halberstam's *The Best and the Brightest* or other literature on the Vietnam problem, but the suggestion is not illogical. Gerhard Ritter's magisterial study was undertaken in the midst of the Second World War in order to help answer a question that was troubling many of his countrymen, namely, "How did the German people, for centuries among the most peaceful nations of the west, come to be the terror of Europe and the world, acclaiming a violent adventurer who will live on in history as the destroyer of the old European order?" This translation of the first two volumes of his work appears at a time when many Americans are asking similar questions about their own country.

Ritter's answer, most historians would agree, was only a partial one (he was a historian of the old school, with little interest in the sociological and psychological aspects of his subject), but for that very reason, it is all the more relevant to our own problems. His book was called *Staatskunst und Kriegshandwerk*, which, properly translated, means, not "The Sword and the Scepter" (a fatuous title) but "The Art of Statesmanship and the Craft of War." His thesis was that throughout the history of the great states of the West, there has always been tension between the two—an un-

easy relationship that is the consequence of the conflicting views of their practitioners. The statesman, if he is true to his calling, appreciates the uses, indeed, the indispensability, of military power but insists that it must be an instrument of policy. The soldier, while paying lip service to this principle, will in matters of practice resent its application. In time of war, the statesman's job is "to curb its internal dynamics, to shorten it, to limit its aims . . . to keep war instincts . . . from getting the better of calm political reason" (I, 9). But it is particularly in wartime that the soldier is most resistant to restraints. He is always "against neglecting any occasion to destroy the enemy forces," whereas "the statesman may fear destruction of permanent values, undesirable political side-effects, the stiffening of the enemy's resolve against peace" (*Ibid.*). Of the two, it is generally the soldier who has the greater self-confidence, because his view is narrower (Ritter would have been interested in Halberstam's statement about U.S. commanders in Vietnam, namely, that "their particular certitude made them more powerful players than men raising doubts"); and the statesman, therefore, is always in danger of being overborne by the brasshats and of submitting to arguments of military expediency.

In German history this problem was endemic and ultimately disastrous, and, before he died, Ritter had finished four large volumes on the evolution and resolution of the civil-military conflict. Of these, the first two, now ably translated by Heinz Norden, are the most satisfactory, for the later volumes, which deal with the First World War, are filled with polemical sallies against the Fritz Fischer school of history, and the author sometimes loses his sense of perspective. In contrast, those dealing with the eighteenth and nineteenth centuries and the origins of the First World War are both more objective and more readable, free from the mountains of detail that Ritter piled up in later volumes to protect himself against counter-assaults from the Fischerites.

In the first volume, for instance, after a discussion of the power politics of Frederick the Great and the development of the techniques of limited war in the eighteenth century, Ritter describes the revolution in warfare that took place in the Napoleonic period and the work of the great theorist who first recognized its significance. It was Clausewitz who had the wit to see that, with the coming of the mass army and the new technology, war would constantly tend

to assume its absolute forms, and it was he who handed down, less as a definition than as a goal, the dictum that "war was [i.e., should be] the continuation of state policy by other means." Clausewitz was no mere theorist. He had crossed Europe with the Russian armies that pursued Napoleon from Moscow, and at the end of that journey, as a soldier in Paris in 1814 and 1815, he had had ample opportunity to watch the Prussian commanders Blücher and Gneisenau attempting to take policy into their own hands. That civil-military conflict was the forerunner of the many that followed, particularly those between Bismarck and the Prussian military during the war years 1864, 1866 and 1870-71, which are discussed with authority and rich detail in the second half of this volume.

In Bismarck's career, Ritter holds, "the phenomenon of true statesmanship may be studied . . . to better effect, than in almost any other figure in our history" (I, 11), for in the Chancellor's person national self-assertion and a sense of European responsibility were combined, and while he respected military power, he used it with restraint and for precise political objectives. To him, as to Clausewitz, war had its own grammar, but not its own logic; the latter was supplied by the statesman. Such views did not ingratiate Bismarck with Moltke and his "demi-gods," and the Chancellor's whole career was marked by military attempts to throw off his restrictions and, increasingly, to encroach upon his sphere of competence. These he was able to defeat, but his successes in this respect were the last for the civilian principle.

How true this was Ritter demonstrates in his second volume, which begins with a brilliant section describing the army's role in politics in France, Britain, and Russia in the years 1890-1914 (and showing that none of these countries was free of military encroachment upon decision making). It continues with a discussion of the factors in Germany that made this more dangerous there than in other countries—such things as the progressive militarization of the thinking of the middle class, the dominance of Social Darwinism, and the lack of civilian political talent after Bismarck's fall. This brings the author to the culminating sections in these volumes, those on Tirpitz and the growth of the battle fleet and on Schlieffen and the ascendancy of the General Staff. Here he provides brilliant descriptions of the way in which, in moments of perplexity and danger when the international order begins to crumble and crisis is the order of the day, the terrible simplifiers in

uniform can take over and, with their breezy self-assurance, commit the nominal leaders of the country to courses pregnant with disaster.

GORDON A. CRAIG

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Political Parties and Pressure Groups in Britain.

By Geoffrey K. Roberts. (New York: St. Martin's Press, 1972. Pp. ix, 203. \$8.95.)

Dr. Roberts, who lectures in political science at the University of Technology, Loughborough, England, has written an introduction to the subject of political parties and pressure-groups, "those influential organizations which do so much to shape the decisions which the more formal of our [British] political institutions make for us" (p. ix, introduction). In the first of his nine chapters he sets the scene with a virtual justification for presenting the remaining eight. It is that the individual citizen, feeling he has "less influence on the shaping and direction of public affairs, than was possessed by his nineteenth-century counterpart" regains ground through these intermediate groups, which "filter and focus the multiplicity of demands that the citizens have on government . . ." (p. 7). In so doing the groups do not subvert democracy, they complement it.

Distinguishing in the conventional way between parties and pressure groups, Dr. Roberts then devotes three chapters to the former, one specifically to the latter (which are equated with lobbies and interest groups). Chapter 2 is given over to the history of (English) parties, pre-1832 (pp. 14 through 18), post-1832 (pp. 18-28). Chapter 3 opens with a specification of the functions of parties that follows traditional lines. The chapter is rounded out with a sketch of the anatomy of British party organizations and party programs. Chapter 4 deals with leaders, generally meaning front-benchers in Parliament, and followers. Having touched on the qualities required of leaders, Dr. Roberts proceeds to ask: Who are they? How are they selected? Followers are divided into activists, supporters, and sympathizers. The relationship between leaders and followers is then explored. Predictably, Michels is brought on stage, and his hypothesis is discussed along four dimensions; "the appointment and change of leaders, the distribution of power within the party, the way in which decisions are made, and the sources and control of finance" (p. 69). The broad conclusion is that "in all three British parties, effective control is, and to a large extent inevitably must be, concentrated in a few hands—generally the Leader, the chief

members of the parliamentary party, and a small number of top executives in the central bureaucracy . . ." (p. 77).

Turning to the other species of intermediate groups, the author distinguishes between lobbies (specialized in one political role) and pressure groups, conceived as having other functions as well. Their history is then sketched (pp. 81 through 89), after which Dr. Roberts has something to say about their objectives and rather more about their methods and their organization. The conclusion of this part of the book is that (in Britain) there "is a kind of 'countervailing power' in the exercise of political, as well as economic, influence" (p. 106). Accordingly, pressure groups are judged "politically beneficial" as well as, by virtue of stimulating public debate, "an educative force."

With Chapter 6 (Groups and Policy), wider considerations are introduced. A "ladder of values" is identified. Its bottom rung is made up of ideology (a set of basic values), from which policies extend, and, higher still, political decisions. Party is related to the ladder of values through the 1966 Election manifestoes, from which the relationship between ideology, policy, and program is extracted. This is followed by a brief attempt to illustrate the dynamics of the relationship in practice. Pressure groups come on this scene as vehicles for public opinion on particular issues (apparently to be equated with policies on the ladder of values), although they also express ideologies.

If ideology provides "the fuel for the vehicle of their political behavior," the parties and pressure groups do not run free, i.e., the vehicle does not lack "brakes." Such "constraints" are the subject of Chapter 7. These arise principally from within the parties or pressure groups, the political system, another of the subsystems, such as the economic; or from another State. In Chapter 8 the discussion becomes comparative—an essentially structural-functional treatment based on a somewhat idiosyncratic classification of political systems. The concluding chapter pulls the threads together in terms of "the plural society," where "no exclusive source of basic values" is recognized and free competition is invoked for reaching acceptable compromises. This competition is represented mainly as occurring between parties, but "often" as encapsulating the claims of pressure groups, which also, however, go their own way. It follows that within the political system, there is a place for conflict as well as consensus, and even consensus ought not to be so interpreted as to overlook the claims of the *unor-*

ganized. Finally, Dr. Roberts asks, "Can the plural society which Britain has become, with its paraphernalia of political parties and interests and pressure groups, be called a democracy?" (p. 190). Answer: "political parties and pressure groups perform many functions which help to carry out" the "requirements of democracy" (p. 191).

The book is what it purports to be—an introduction. It could be safely put in the hands of freshmen and sophomores for courses on British or comparative politics. But American instructors would have to be prepared to identify more fully a number of the bit players (e.g., Desmond Donnelly, Woodrow Wyatt) who walk on this political stage, and to explain some of the brief historical references (e.g., Catholic Emancipation, the 1832 Reform Act). Instructors will not need any encouragement to question some of Dr. Roberts's judgments. One cannot, for example, *deduce* "countervailing power" from the mere existence of "natural opposites" or even from the free play of debate in which "both sides" are presented (pp. 106–7). Nor is the Schattschneider criticism weighed in British terms—that in the pluralist heaven, the chorus sings with a strong upper-class accent. All things considered, however, this is a useful book for the younger student.

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Brazil in the Sixties. Edited by Riordan Roett. (Nashville, Tenn.: Vanderbilt University Press, 1972. Pp. xvii, 424. \$15.00.)

Modern Brazil: New Patterns and Development. Edited by John Saunders. (Gainesville: University of Florida Press, 1971. Pp. x, 350. \$12.50.)

Brazil: People and Institutions. By T. Lynn Smith. Fourth Edition. (Baton Rouge: Louisiana State University Press, 1972. Pp. xx, 778. \$15.00.)

The early 1970s have proved to be banner years for the publication in this country of reasonably rigorous social science research concerning Brazil. Always one of the world's largest countries, this emerging "Colossus of the South" has very recently entered the select company of nations with more than 100 million inhabitants and the equally restricted circle of those industrial powers boasting a GNP of over \$50 billion. In this context, comprehensive assessments of the socioeconomic, demo-

graphic, and institutional aspects of contemporary Brazil are particularly timely, even if they are based upon data and research of the 1960s. When such studies represent fairly sophisticated analyses of highly qualified scholars, as in the volumes under review, political scientists ought to pay serious attention to the insights they may provide regarding the process of modernization and development in a country edging close to attaining continental paramountcy.

Only one of these three broad-ranging works purports to treat political matters directly and at any length. Roughly the first quarter of the symposium edited by Roett includes a workmanlike review of political developments from 1946 through 1969 by the editor, now an Associate Professor of Political Science at Vanderbilt University, and an uneven summary of Brazilian-United States relations by Peter Bell, who during most of the 1960s was a representative of the Ford Foundation in Brazil. Sandwiched between is by far the most intellectually satisfying portion of these three books, at least from a disciplinary and conceptual point of view. Douglas Chalmers, Associate Professor of Political Science at Columbia's Institute of Latin American Studies, has provided us with an insightful essay on "Political Groups and Authority," appropriately subtitled "Some Continuities in a Decade of Confusion and Change" (pp. 51-76). Focusing upon the "lack of an acceptable conception of the basic character of the Brazilian political system" Professor Chalmers explores the "enduring political structure" underlying that country's formal institutions. In stressing the importance and persistence of essentially elitist "vertical" relationships formed around authority and domination as part of a "changing, dynamic, and distinctive" political style, he rejects the pervasive notion that they are transient characteristics of a traditional or transitional stage in the development process. Quite validly he questions "whether class or any other basic cleavage accurately describes the differences between Brazilian power contenders of any period" (p. 65). Underscoring the "hyper-fluidity" of political support at the national level, Chalmers suggests that in the Brazilian case the conventional dichotomy between the "técnico" and the politician is increasingly meaningless as the new politician-technician becomes the crucial type of political actor. He is thus able to conclude that there are no grounds for assuming that Brazil will or must establish a pluralistic, liberal-demo-

cratic form of government in order to become more developed. If democratic values of responsiveness to citizen demands, egalitarianism, and provisions for participation in politics are to be achieved, they will not come through copying older democratic institutions appropriate to another sort of politics in another age. They will have to emerge out of efforts to shape new types of vertical groups and new relationships between such groups and the increasingly technical political process (p. 76).

By contrast with the Roett symposium, that edited by University of Florida sociologist John Saunders includes only a very formalistic and traditional paper on "The Political and Legal Framework of Brazilian Life" by Anyda Marchant which emphasizes the legal façade over the political realities. The first twenty pages of Senhora Marchant's essay do, however, furnish a brief sketch of Brazilian political history useful for orienting the uninformed reader. For those who need such orientation, this essay should be read as background to the Roett volume. Indeed, the same holds true for Donald Dyer's succinct discussion of geographical factors (pp. 29-50) and J. V. Freitas Marcondes's paper (pp. 133-55) treating "The Evolution of Labor Legislation in Brazil," since he deals with this topic in the broader context of the evolution of the Brazilian governmental system from the mid-sixteenth century to the present.

If the Roett book is the stronger on political topics, Saunders excels in the analysis of societal factors. The editor's own essay on "The Modernization of Brazilian Society" is a useful overview of the subject, and its discussion of social differentiation and cultural integration prior to 1964 adds depth to the other book's interpretation of political trends. To a considerable extent this could also be said of two other sociological papers in the Saunders volume: Sugiyama Iutaka's "The Changing Bases of Social Class in Brazil" and José Arthur Rios's examination of "The Growth of Cities and Urban Development." Cultural matters of possible political import are covered by Earl W. Thomas in "Protest in the Novel and Theater" in Roett and "The Evolution of Brazilian Literature" in Saunders. The latter provides a background survey, while the former is of greater use to the political scientist concerned with the present situation. Its companion essay on the short story by Alexandrino Severino falls between Thomas's two chapters in terms of its relevance to the student of Brazil's politics.

Economic themes loom large in both the

Roett and Saunders books. In the former, Werner Baer and Isaac Kerstenetzky furnish an account of the Brazilian macroeconomic scene depicting the 1960s as a decade of adjustment to the structural changes of the 1950s. Their projections for the 1970s, however, err substantially on the side of caution, with the per capita GNP predicted for 1980 in fact likely to be reached by 1975 instead. In the Saunders symposium, Eric Baklanoff covers much of the same ground in greater historical context, but fails to make any prognostications for the 1970s. Although in both collections William H. Nicholls deals with the agricultural sector, his more current analysis and policy prescriptions appear in the Roett-edited volume. Highway expansion is singled out as the major catalyst of change in rural areas by this long-time observer of the Brazilian scene. The Roett work also includes a useful essay on the economic functions of the public sector by Andrea Maneschi and a detailed technical study of tax credit impact on industrialization in the Northeast, while the Saunders volume includes a general historical sketch of industrialization prior to 1964.

Each of these books devotes some attention to the rapidly changing field of education, although neither brings the reader fully abreast of the present situation, particularly in the field of higher education. The late Anisio Teixeira analyzes "The Changing Role of Education in Brazilian Society" for the Florida symposium and Douglas Graham surveys "The Growth, Change, and Reform of Higher Education in Brazil" for its Vanderbilt counterpart. Complementary much more than overlapping, because the philosophical Brazilian educator's deeper time perspective provides an excellent backdrop for the recent data amassed by the technically oriented young U.S. economist, these essays are of substantial importance to anyone hoping to understand Brazilian modernization.

T. Lynn Smith, Florida's distinguished Graduate Research Professor of Sociology, contributes to the Saunders study a brief essay on "The People of Brazil and Their Characteristics." He has subsequently published a fourth edition of his classic, *Brazil: People and Institutions* (which first appeared in 1946). Rather than a complete revision, this is a reprinting of the 1963 edition with the addition of a lengthy supplement covering the more recent trends and data (pp. 627-730). But given the author's more than thirty years' systematic quest for understanding of the processes at work in Brazil, these meaty chapters are a must

for any scholar concerned with the societal roots of Brazil's political forces. For while the data may be available elsewhere, Professor Smith is specially gifted in their use and making wise inferences from them. Thus his meticulous demographic analyses indicate that with an end to large-scale immigration and the falling of birth rates among the middle class, the long observable racial "bleaching" will be reversed during the 1970s; that while real differences between urban and rural life are being reduced, the perceived deprivation on the part of the rural population is still on the rise; and that the Trans-Amazonian highway "could mean fully as much for the agricultural, industrial, and commercial development of the northern part of South America as the Aswan Dam will for northern Egypt" (p. 651).

Great improvements in farming methods, promising beginnings in planned colonization, and the manifold impact of rapid sustained urbanization are among the themes explored by Professor Smith. He argues persuasively that many of the societal changes observable in contemporary Brazil parallel those in the United States during the 1920s. His conclusions on changing class structure, family life, and community practices have implications for the political scientist greater perhaps than do the frequently decreed modifications in governmental institutions. With respect to politics proper, Professor Smith makes but a few random observations in keeping with his general thesis that so much has occurred so rapidly that "it probably would have been almost impossible for anyone, even a specialist working full time on the study of Brazilian political affairs to have kept fully informed about these happenings" (p. 726).

In sum, these three works do not provide a balanced and comprehensive treatment of Brazil's political panorama. They do furnish much of the understanding of Brazilian society and the industrialization process essential to any adequate comprehension of that country's political affairs. A just sufficient summary of political trends is furnished for those readers concerned with locating Brazil in relation to the developed and developing nations. The Brazil of 1973, owing in large part to economic growth rates averaging nearly 10 per cent annually for the past five years, has already moved beyond the limits suggested in many of these essays. In the political realm, however, it must still cope with the problems so concisely posed by Chalmers. Otherwise, the net yield of

these three studies for the student of politics is substantial, albeit largely indirect.

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Youth and Authority in France: A Study of Secondary Schools. By William R. Schonfeld. (Beverly Hills, Calif.: Sage Publications, Inc., 1971. Pp. 80. \$2.80.)

This short monograph, published as a Sage Professional Paper, represents a potentially significant contribution to the literature dealing with political development in France. The author has two goals: (1) to examine empirically the authority relationships in French secondary schools; (2) to consider the political implications of the style of authority, in particular, as it affects political development.

Professor Schonfeld reports that authority relationships in French secondary schools follow a developmental model alternating, in the lower grades, between two patterns labeled the *authority-laden syndrome* and the *chahut* (classroom insubordination) *syndrome*. The first is characterized by a high degree of compliance by the students to the directives of teachers, the second represents a status of rebellion against particular teachers. The apparent factor deciding which pattern will occur is labeled the degree of *Caesaristic legitimacy*.

The socialization process over the secondary-school cycle results in two types of authority relationships. In the first place, the early cycle of alternating patterns may persist if the teacher is not perceived as facilitating the achievement of expected goals which include learning and passing exams. In the second place, Schonfeld suggests that when goals are met, authority relationships follow what he labels the *assumed-coverage* cycle, that is to say, a pattern of authority characterized by the students' internalization of authoritarian directives. In reality, however, such directives are assumed since they are not actually issued by teachers.

The significance of the monograph lies in the author's attempt to relate his findings to the political system, in particular, to the pattern of political change in France. Schonfeld suggests that political authority in the French system may follow the pattern outlined in secondary schools. Thus he argues that the usual form of government in France is of the *assumed-coverage* type under which citizens comply with a set of assumed directives which are internalized rather than issued by the authorities. This form of government will persist as long as three

requisites are met: (1) The level of economic output sustains a reasonable standard of living; (2) internal stability exists; (3) foreign military involvement does not threaten the internal stability of the system. However, Schonfeld argues that when these requisites are no longer met, "there will be a movement away from *assumed-coverage* government toward an *authority-laden* regime in which the political system impinges much more directly on the citizens" (p. 70). It is during the transitional phase that the author suggests that *chahutage* (read political instability) will occur.

The author's conceptualization owes much to Harry Eckstein, in particular, the latter's emphasis on the political significance of the linkage between political and social authority patterns. Schonfeld also draws on Crozier's *Bureaucratic Phenomenon*, while pointing out the limited political implications of that work. Though the author does not refer to Stanley Hoffmann, his model reminds one of Hoffmann's definition of homeorhetic change outlined in his essay on "Heroic Leadership: The Case of Modern France" in *Political Leadership in Industrialized Societies*, edited by Lewis Edinger (New York: John Wiley and Sons, Inc. 1967).

It is too early to assess Schonfeld's contribution since the present monograph represents only the first step in a larger project in which the author intends to examine the pattern of authority in other sub-systems. Only then will the model be fully articulated and available for general testing. One can only hope that the final product will not be very far off.

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The Long Revolution. By Edgar Snow. (New York: Random House, 1972. Pp. 269. \$6.95.)

Red China Today. By Edgar Snow. (New York: Random House, 1970. Pp. 749. \$20.00.)

It is surprising that even Edgar Snow could have misunderstood Chairman Mao. After a conversation with Snow on December 10, 1970, the Chairman courteously escorted his long-time American sympathizer to the door. Mao said that he was not a complex man but really a very simple one. He was, Mao said, only a lone monk walking the world with a leaky umbrella.

Here, the Chairman apparently was using a Hunanese slang expression, "*ho-shang ta-san*" (a monk walking under an umbrella), which

was misunderstood by Snow and possibly his interpreter too. It means "*wu-fa wu-l'ien*": "no law (same sound as "hair" in Chinese) and no sky." In other words, Mao said that he was a man unrestrained by any regulation or law!

Snow also erred more than once when he injected Chinese words into his writing (pp. 24, 180 and 219), which illustrate the pitfalls common to many students who want to show off English. However, his posthumous publication, *The Long Revolution*, not only confirms some of the well-known facts on the Cultural Revolution, but also adds immeasurably to our knowledge of the episode.

It is now confirmed by Mao that for years, Teng Hsiao-p'ing, Secretary General of the Party, did not report to the Chairman. Mao complained that Teng was hard of hearing but at meetings would sit far from him, and that in 1965 Mao could not get the Party-controlled press in Peking to publish an article criticizing an historical play. The article—revised 11 times, we are told, and seen by both Mao and his wife, Chiang Ch'ing—was finally published in a Shanghai newspaper in November 1965. It marked the beginning of the Cultural Revolution.

There are a number of crucial questions that one might wish to have the privilege of asking the Chairman. For example, when did he finally decide that his successor, Liu Shao-ch'i, had to go? We are now told that the moment of decision came in January 1965 when Mao put before a meeting of the Political Bureau a program for the coming Cultural Revolution. Right at that meeting Liu strenuously opposed the program's first point, which was an outgrowth of the Socialist Education Movement. The first point denounced and demanded the removal of "those persons in authority within the Party taking the capitalist road." But it was after the publication in Shanghai of the above-mentioned article that events unfolded rapidly.

According to Mao's comments to Snow, there were two fundamental issues underlying the Cultural Revolution. In addition to "revisionism," a closely linked issue was posed by Liu, who had sought a compromise in the Sino-Soviet impasse when the United States escalated the Vietnam war. Liu wanted to send a Chinese delegation to the Soviet Twenty-third Party Congress to reactivate the Sino-Soviet alliance. Mao resolutely refused to be drawn into a position of dependence, as in Korea, and a possible double cross.

The possibility of a double cross is an important factor to keep in mind when one attempts

to explain why, when China finally sought a detente with one of the two superpowers, it was not the Soviet Union but the United States with which Mao preferred to negotiate. Besides, rapprochement with the Soviet Union could not solve the problem of Taiwan, which—contrary to the views of some prominent American scholars several years ago—had always been the most important issue in Sino-American relations.

It is debatable whether the political differences between Mao and Liu over these two fundamental issues were irreconcilable. Was the Cultural Revolution a power struggle? Snow answered his own question: "Subjective factors cannot be entirely separated from objective reality" (p. 20). Admittedly, the story of the Cultural Revolution is full of important gaps, omissions, and modifying facts. Besides, as Snow stated, we have not heard from the defeated.

Now it was said that Mao and Liu had always represented "two lines" from the beginning of the Chinese Communist movement. Snow's account of Liu seems to be quite fair under the circumstances. Snow remarked that before 1962 Liu had never overtly opposed Mao personally, and that Liu did not seem to have made any planned, serious attempt to meet Mao's challenge in an all-out struggle or even fully realized, before August 1966, that he himself was the No. 1 target of the Cultural Revolution. How could Liu, Snow asked, without specific directives, know that he was supposed to abolish himself and the Party apparatus he headed? He could only act as he always did—use Party cadres to carry out Party policies. Who could have foreseen the Red Guards? All his previous experience with Mao's rectification campaigns had failed to prepare him for anything so drastic as the breakup of the Party machine itself by outside forces. That was where he made mistakes in line and orientation at the beginning of the Cultural Revolution. Perhaps he was no less astonished than the rest of the country at Mao's daring intent.

Mao said that there were two things of which he highly disapproved during the Cultural Revolution. One was lying and the other was the maltreatment of Party members removed from power. But he did not elaborate on either one of these points. The Cultural Revolution was made possible because of the enormous prestige of the Chairman and the support of the People's Liberation Army. Curiously enough, the book contains little information about Lin Piao, and there is a complete blank

on the role of Premier Chou En-lai in the various stages of the Cultural Revolution. However, visitors do not ask embarrassing questions. The world was speculating about the fate of Lin Piao after the airplane accident in Mongolia in September 1971. But when I visited China shortly after the incident, his name was not mentioned once by anyone during my entire trip. Apparently, Snow did not ask his hosts the whereabouts of Liu and the other ousted top leaders.

Snow told Mao that he had once attended a conference in which the professors debated whether Mao had or had not made any original contributions to Marxism. He asked one professor at the close of the conference whether it would make any difference in their controversy if it could be shown that Mao himself had never claimed to have made any creative contribution. The professor impatiently replied, "No, indeed. That would be quite beside the point." After hearing this story, Mao laughed and said that more than two thousand years ago a Chinese philosopher had written an essay. A hundred schools of thought then arose to dispute its meaning! Snow must have got some personal satisfaction from this comment.

The Long Revolution contains eight chapters and four appendices. It deals with various subjects, including the commune system, medical care, population control, the Cultural Revolution, the army, and the Sino-American rapprochement. The most valuable parts of *The Long Revolution* are the records of Snow's conversations with Mao and two lengthy dialogues with Chou during Snow's last visit to China, August 1970–February 1971. Some of the material, however, including the discussions of Sino-American relations and the first concrete figures on China's industrial and agricultural output made available in Peking for nearly a decade, had been published previously in newspapers and magazines. Other valuable data in the book include material on the rebuilding of the Party and state superstructure after the Cultural Revolution. For example, the Party membership was suspended during the Cultural Revolution but less than one per cent of the Party membership had actually been expelled. By November 1970, something like 95 per cent of the former Party members had been reinstated. Reinstated did not necessarily mean reassigned; many awaited "liberation" following completion of "struggle-criticism-transformation," the three-stage formula for redemption. As for the fragmentary reports on China contained in the book, the United States has been

flooded with similar eyewitness reports since 1971.

When *The Other Side of the River* was published in 1962, it was a major contribution to the understanding of China as Snow found it during his visit in 1960. *Red China Today* is advertised as a revised and updated, enlarged edition of the 1962 book. As a matter of fact, with the exception of the addition of several footnotes, the deletion of several chapters and several very minor revisions, the two editions are identical.

Edgar Snow returned from his last trip to China in February 1971. He died on February 15, 1972, thus ending the unique career of an American journalist who first interviewed Mao in 1936 and brought to the world a sympathetic and influential account of the Chinese Communist movement. He will be remembered by his friends in China.

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Mao's Revolution and the Chinese Political Culture. By Richard H. Solomon. (Berkeley and Los Angeles: University of California Press, 1971. Pp. 604. \$16.75, cloth; \$5.95, paper.)

This book is the latest in a series of landmark studies of Chinese politics that attempt to integrate the confusing and difficult-to-obtain data emerging from the Mainland with a theoretical orientation drawn from one or another of the social sciences. It is legitimately to be compared with such earlier works as Franz Schurmann's *Ideology and Organization in Communist China*, A. Doak Barnett's *Cadres, Bureaucracy, and Political Power in China*, historical works by Stuart Schram and Jacques Guillelmaz, and the Social Science Research Council volumes edited by Barnett, John M. H. Lindbeck, and Robert Scalapino. Like the Schurmann volume, Solomon's work takes a well-developed theoretical approach—his choice is political culture—as the means to order the details of Chinese politics. It is, of course, desirable that scholars continue to try to close the gap, long extant, between the particular study of Chinese politics and differential theoretical and methodological viewpoints on how most accurately and parsimoniously to investigate politics in general. That Solomon's effort is, on balance, less than completely successful, should not be interpreted as signaling the futility of such enterprises, nor does it mean that both political science and Chinese

political studies are not better off because of his work.

The work is divided into four parts, the first two of which investigate the details of traditional political culture. The main idea is that the socialization process in traditional and transitional China created a clear pattern of attitudes toward authority—whether familial, political or administrative—that stressed conflict avoidance and accommodation, anxiety when confronted with authority (and hence the avoidance of politics), and the fear of personal betrayal and of social confusion. Thus, concludes the author, Chinese politics and family life is set, at its base, against political change whenever there is the danger of disorder; indeed, against any challenge to authority that involves even the threat of violence. Chinese politics was therefore profoundly conservative: the individual learned to repress deep within his psyche tendencies to right wrongs, to forego any desire to strive for social-economic-political justice, and not to become involved in political movements. It follows that the Chinese historically was much more willing to tolerate political repression and arbitrary government than, say, Western political man, who was subject to an entirely different set of environmental forces.

Having asserted this, Solomon then tries to explain how it is that China underwent a severe revolutionary catharsis, is governed by one of the world's most stringently militant Marxist political parties, and yet does not sink back into the more natural traditional attitudes toward politics. His answer is that a single man, Mao Tse-tung, managed to overcome the socialization process that so molded all other Chinese; and, aided by, learning from, and using several of the developments of Chinese history (warlordism of the 1920s, the Japanese invasion of the 30s, and the increasingly miserable socioeconomic conditions of his compatriots) fashioned a party that gradually led an otherwise unwilling people to overthrow the old system of political culture. The confusion (*luan*) created by these historical circumstances enabled Mao and his associates to enlist large numbers of people in an effort to refashion Chinese society completely, a task made possible by the very fact that the Chinese political psyche, once freed of the limitations imposed by the old political system, tended to go to the opposite extreme and approve of and actively assist in the violent transfer of state power. Thus, the key to the Chinese revolution, as Solomon sees it, is the dialectical relationship be-

tween the leader, Mao (who learned how to "turn on" the Chinese people by making essentially the same analysis in the field as Solomon made in the social science laboratory) and all other Chinese (who retained, and still retain, all the nonrevolutionary biases of their traditional political culture).

The last two parts of the book comprise a long historical illustration of this thesis. The history of the Chinese revolution is nothing more than that of the relationship between the single Leader and the masses of the led, inside and outside the Party. The Leader's discovery of the Truth (not of Marxism, but of the nature of Chinese political culture) had set *him* free and *his* life has since been dedicated to informing his brethren of this discovery and then to forcing them to change for the better whether they like it or not. The led have been dragged along, as it were, by the force of the Leader's personality, and show constant signs of falling back (indeed, wishing to fall back) into the rather more comfortable and conventional ways of yesterday.

So the history of the Chinese revolution is the history of Mao's differential success in creating a new political culture to replace the old. That is what the Cultural Revolution was all about, in Solomon's eyes: it was the third of Mao's great attempts to mold the Chinese people in his own image, to kick them into shape (the first two being the long revolutionary road to power culminating in 1949, and the Great Leap Forward in 1957-59). The trouble is that in the long run, the struggle against the traditional culture is an unequal one. Most recent observers wonder if the Cultural Revolution, for all its *sturm und drang*, did anything but confirm that the old ways not only have not been eradicated, but that they may indeed be more powerful than ever. Liu Shao-ch'i, who sought to use traditional Chinese political culture to modernize China, may well yet have the last laugh, and Mao, who still seeks to revolutionize China despite, and to some extent against, the people's wishes, seems to have lost his battle.

This idea adds an important element to our understanding of modern China. The question remains, however, whether it is anything more than heuristic, for there are major methodological problems with Solomon's analysis. For instance, his reasoning is heavily circular in his derivation of the central features of Chinese political culture. First he surveys the prior literature on Chinese characteristics and rejects this as unscientific, then does attitude testing on a

group of Chinese in Hong Kong and Taiwan, and concludes that the political attitude structure so revealed must reflect reality since it is congruent with the testimony of the traditional writers. Moreover, his sample is woefully small (less than one hundred to represent the 700–800 million Chinese), male only, and instead of being randomly chosen, is deliberately skewed to reflect regional diversity and uniform age distribution. The statistical variance in interviewer response is extreme. Throughout, there is heavy reliance on the doubtful analogy between some alleged Chinese characteristics, such as emphasis on orality and food, and political characteristics, such as “eating bitterness.” More important, it is not clear whether the characteristics elicited are those centrally important to Chinese political culture. In the end, the reader is left asking what Chinese political culture is: how does one know it if it is put before him (i.e., how does one sort out political culture characteristics from the other attributes of the Chinese people?). Indeed, it is not even clear that the author has not induced the heuristic, bare-bones theory out of the facts of Chinese political history (a legitimate enterprise in itself) but then *a posteriori* grafted onto it pseudoscientific (in this case) measurement techniques to serve as weighty “proof.” Finally, Solomon fails to explain adequately how it happened that Mao alone escaped the fate of other Chinese people and, operating as much outside and upon Chinese political culture as within it, emerged as a Savior revered by the masses instead of being rejected by them as a crank. Despite the author’s good intentions, doubt wells up in the reader’s mind, and his natural conclusion is to question whether the political culture approach is the proper shortcut to a sophisticated understanding of latter-day China.

The book falls apart in the second half, as the framework of the first proves not only inadequate but inapplicable. There is just too much that needs explanation, or rather, everything that has happened to the Chinese Communist Party since 1928 and to China as a whole since 1949 is forced within the bounds of the theory. Reality is too complex for the political culture approach (as it is for all other approaches—which is perhaps the way it should be), and the author finds himself casting about for a more viable framework, hopping from political theory to political history to invocation of some of the words of the political culture approach to, in the end, straight Pekinological esoterica and the uncovering of abstruse facts. If the political culturalist feels the necessity to take upon his

own shoulders the explanation of everything that happens in a given time-span, and the result is the mish-mash of Parts III and IV, not many will be encouraged to follow in Solomon’s footsteps.

The second half of the book can, to be sure, be used as a reference volume on what has happened in China since 1949. Solomon’s reconstruction of the Great Leap Forward period, its aftermath, the events leading to the Cultural Revolution, and major aspects of the most recent era of Chinese history, is the first detailed attempt to revise and improve our understanding of post-1949 China, using the Red Guard and other Cultural Revolution documents. It is difficult, however, to plow through 366 pages of facts that do not, despite the author’s assertion, serve as illustrative proof of the theoretical conclusions of the first half of the volume, and are not tied together by any other sort of intellectual glue. The book should, indeed, be treated as two separate works. If that had been done in the first place, theory and fact could have been separated, or only relevant facts noted, and the overall evaluation of Solomon’s effort would possibly have been much higher. Lest this review end on perhaps too negative a note, I should assert that rather than to advance by slow, timid, and painful increments, it is often better to aim high and fall short. Students of contemporary Chinese politics are indebted to Dr. Solomon for his efforts.

THOMAS W. ROBINSON

Council on Foreign Relations

The Managerial Class of India. By M. Subramaniam (New Delhi: All India Management Association, 1971. Pp. 91. Rs. 10.00.)

Social Background of India’s Administrators. By M. Subramaniam. (New Delhi: Publications Division, Ministry of Information & Broadcasting, Government of India, 1971. Pp. 180. Rs. 12.00; £1.25; \$3.60.)

The author of these monographs on the social composition of India’s governmental and managerial elites brings impressive qualifications to his task. Professor Subramaniam, trained in public administration in India and Australia, has conducted several similar studies in both countries and was twice prize-winner in the Haldane Essay Competition of the Royal Institute of Public Administration. The first of the books here reviewed results from a study done while teaching recruits to the higher civil service at the National Academy of Administration. The author thus had access to a large body of official data about his subjects as well

as personal contact with many of them. While still at the National Academy, Professor Subramaniam felt that studies of other middle-class groups should be undertaken for comparative purposes. *The Managerial Class of India*, encouraged and sponsored by the All India Management Association, results from a project in which managers of five "highly management-conscious firms" were compared with those of firms drawn as a random sample from a list of 200 top firms published by the Federation of Indian Chambers of Commerce and Industry.

In both studies a large mass of data was assembled from application and recommendation forms, questionnaires, and interviews. These were coded and processed by sorter and computer; tables were then constructed to show the interrelation of social characteristics in the populations concerned. In the volume on civil servants the subjects are mainly direct recruits through competitive examinations, from 1947 to 1963, of the Indian Foreign Service, Indian Police Service, and Indian Administrative Services. In the first two groups the total populations (144 and 772, respectively) were studied; in the last-mentioned, 45 per cent of the total. The rationale for emphasizing direct recruits is that they comprise the majority of the foreign and central services, and there is generally a trend toward direct recruitment by examination and progressive retirement of mature-age recruits in the Indian civil services. In the case of the central services—departments administering matters under the federal government, as distinct from the Indian Administrative Service which administers matters under state jurisdiction—a different method of selecting the study population was employed: the total numbers of the two more prestigious and the two less prestigious services were surveyed. In the book on managers, more than 1500 individuals supplied data via detailed questionnaires circulated by the chief personnel officer of each firm. While there was inevitably some variation in various firms' definition of the term "manager" and in percentage of response, these factors do not seem to be of sufficient magnitude to call into question the conclusions drawn.

In the first book the author addresses himself to the question of the representativeness of Indian bureaucracy. To the surprise of no one at all familiar with Indian society, bureaucracy turns out to be very unrepresentative indeed. How important is this? Professor Subramaniam argues that representativeness per se is less important than responsiveness to public needs, and that the latter is influenced, at least in other countries, by sharing of "common values

which are part of the nation's socio-political consensus" and the exposure of the bureaucracy "to all influences regardless of their class origins" (*Social Background of India's Administrators*, p. 6). He also stresses the importance, in a developing society, of efficiency, and contends that this is best achieved by rigorous selection through tests of ability:

For India clearly, the rigorous selection of an efficient bureaucracy should be placed far above a concern with its *immediate* representativeness. That will come about gradually and faster than suggested by hasty critics through the widening of educational opportunities for all and the provision of intensive education for the under-represented. Meanwhile, suitable training methods may be tried to make it more responsive (*Ibid.*, pp. 6-7).

The unrepresentativeness of public bureaucracy in India is a function to a great extent of the very small middle class—only 10 per cent of the population, as compared to 60 per cent in the U.S. The middle class dominates the civil service in both countries; in India its degree of over-representation in the civil service is more than 9 times its population percentage; in the U.S., only 1.5. The author suggests, for India, that "the natural growth of the middle class in the wake of economic development is bound to correct its over-representation" (*Ibid.*, p. 128). He rejects reforms which would immediately establish proportionate representation for all classes on the ground that the very low educational standards it would entail would be seriously damaging. Prof. Subramaniam thinks that the inevitably slow process of widening educational opportunities up to university level, which would first help the lower middle class to qualify but would be rather unlikely to greatly assist the agricultural and working classes, would be of some limited value. He raises the question whether, through sophisticated research, it may be possible to deepen the understanding and sympathies of civil servants for other classes—which, in his view, already exists to an extent greater than is commonly assumed. What is implied is some sort of in-service political socialization to counteract the tendency for middle-class bureaucrats to harden in their class-based attitudes and to emulate those of their superiors as their careers lengthen.

The title of the concluding chapter of *Social Background of India's Administrators*, "A Middle Class Monopoly—So What?," is an apt one which accords well with the author's closing remarks: "We may not have the best or the most representative civil service in the world, but any drastic change in the modes of recruitment

would produce more harm than good. There is much to be said for a policy of controlled experiment, careful research and gradual change" (pp. 130-131). Ideally, this may be so; yet this reviewer cannot but wonder, in view of clear signs of serious economic deterioration in India within the last year or so and the impact of this decline upon the outlooks of all classes, the bureaucracy included, whether time is on the side of gradual improvement.

The findings of *The Managerial Class in India* show important similarities and differences only of degree:

... both higher civil servants and managers are drawn more heavily from the coastal Presidencies and the corresponding linguistic groups than from the heartland of India. ... Both include a large proportion of mobile inter-state affiliates the proportion being higher for managers. Both are drawn heavily from the middle class but over the last decade the lower middle class component has increased steadily in the civil services, partly naturally and partly through governmental policy, while there is no similar tendency among the managers. ... The main point of difference between managers and higher civil servants seems to be the higher proportion of science and engineering graduates among the former (*Ibid.*, p. 36).

Indian managers show a high degree of professionalization; for this fact the author tries to sketch an historical explanation in a purely hypothetical way, since no definitive history of Indian business and commercial development exists to provide adequate data.

Both of these books are extremely useful in providing a clear and empirically adequate sociological profile of two important segments of India's political and economic structure. If they do not offer any ready solutions to problems, at least they enable the interested reader to perceive the problems in sharper outline.

CHARLES S. BRANT

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Catholicism and Political Development in Latin America. By Frederick C. Turner. (Chapel Hill, N.C.: University of North Carolina Press, 1971. Pp. 272. \$8.75.)

While this book occasionally dips into the past, and more frequently looks outside Latin America to Europe for the philosophical roots of the positions it describes, Turner's is essentially a snapshot. It portrays the highly divergent views held today by Latin American Catholics, clerical and lay, as they face issues critical to the continent as a whole and to themselves as Catholics and to the Catholic Church in particular.

Turner has worked immensely hard and systematically going through a large and scattered literature, published and unpublished, as well as conducting scores of interviews. More important, he describes all points of view sensitively and, on the whole, fairly as he reworks his raw data. The "left," "center," and "right" within the Catholic world begin with very different assumptions about the nature of social change and development, and about the proper place of the Church and religion in man's life. Turner brings out these differences in basic assumptions before describing the differences over specific issues to which they lead: whether the church should or should not support radical reforms; the precise role it should play even if it does in principle support them; the stand it should take *vis-à-vis* non-Catholic radicalism, violence, and Marxism; the solutions advocated for the manifold problems internal to Catholicism itself; its views on the population problem; attitudes toward the U.S. and so forth.

The author appears to me to be fair to all camps even though intellectually, and to some extent even emotionally, he seems to be somewhat more allergic to the extreme left than to the right. Indeed, he is not very sympathetic even to those "next" to the extremists (persons such as Ivan Illich and Dom Helder Camera). He clearly finds it easier to see saving graces in the passionate anti-communism of Archbishop Rossell Arellano of Guatemala than in the idealism of Camilo Torres, which ended so tragically. The book ends with a breath-takingly explicit plea that the U.S. government support moderate progressives such as Chile's Christian Democrats with all the means at its disposal—but perhaps through multinational agencies in order not to compromise the receiver! I doubt that policy recommendations of this kind are an appropriate end to an otherwise analytical piece of work.

Nevertheless, and paradoxical though it may sound, this is essentially a fair, well-researched, and sensitive portrayal, whose major blemish lies in the rather loose organization of its six chapters and some unnecessary repetition of material. The chapter on "Catholic Revolutionaries," for example, deals at much greater length than the title would suggest with the causes of the increasing popularity of evangelical sects in Latin America, and the reaction of all parts of the Church to it. The same chapter also goes at length into the growing shortage of priests: like other problems, a worthwhile one to discuss, but not while evaluating the position of Catholic revolutionaries. Similarly, in the chapter on "Tradition and Reaction," Turner deals at great

length with the relationship of the Church to Peron: a story that deserves to be told but which does not bear a relationship to the title of the chapter except at one or two points. Thus, several chapters lose focus either through tackling problems which are of importance, but fall outside their scope; or by presenting historical accounts of important events, only some of which relate to the main theme of the chapter.

But perhaps the ferment and partial confusion of these chapters reflect their subjects more accurately than would an aseptic and overorganized analysis. For the reality described in the book is determined, at least in part, by certain key figures whose unique styles, histories and positions have had much to do with the particular impact of each. And even apart from any "great man" interpretation of events such as those in Guatemala, the position of no group is sufficiently coherent to justify, or possibly even enable the drawing up of, a schematic outline. This opaqueness is one very important characteristic of the position of Latin American catholicism (of whatever orientation), and the author brings it out, both intentionally and unintentionally, through his failure to organize his material sufficiently. Indeed, apart from confusion, there is one more characteristic that might have been highlighted: the existence of many a void. Many laymen, priests and even prelates continue to take no specific position on various critical issues because they are unaware of them as being critical, or are unable to integrate them into traditional modes of thought.

HENRY A. LANDSBERGER

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Essence of Decision: Explaining the Cuban Missile Crisis. By Graham T. Allison. (Boston: Little, Brown and Company, 1971. Pp. 338. \$4.50 paper.)

This study was summarized at the American Political Science Convention in September 1968, and in the lead article of this *Review* in September 1969 under the title of "Conceptual Models and the Cuban Missile Crisis." In brief, the book sets forth three decision-making models which are applied to the Cuban Missile Crisis: Model I, The Rational Actor; Model II, The Organizational Process; and Model III, Governmental Politics. The three analytic chapters on these models are interspersed with three substantive chapters on the details of the crisis itself, and may be read separately for the sake of historical continuity. Since the author states that all the material used in this study is in the public domain, and since there are sev-

eral different interpretations of the crisis itself (A. Horelick and M. Rush, *Strategic Power and Soviet Foreign Policy*, 1965; E. Abel, *The Missile Crisis*, 1966; R. F. Kennedy, *Thirteen Days*, 1969; N. S. Khrushchev, *Khrushchev Remembers*, 1970, *inter alia*), the focus of this review will be primarily on the three conceptual models.

Some years ago, Richard A. Brody defended his model of "international reaction" (O. R. Holsti, R. A. Brody, and R. C. North, "Measuring Affect and Action in the International Reaction Models: Empirical Materials from the 1962 Cuban Crisis," *Journal of Peace Research*, Vol. I, [1964]) in a rather disarming fashion. He stated in effect that "while modeling has no particular virtue, it also has no particular vice." The main working hypothesis of Graham Allison's study might be stated; "That while modeling does have some particular virtues, it also has some particular vices." That is, the main proposition set forth in Allison's study is that; "Professional analysts of foreign affairs (as well as ordinary laymen) think about problems of foreign and military policy in terms of largely implicit conceptual models that have significant consequences for the content of their thought" (p. 4). Allison sets for himself the task of making these models *explicit* and then delineates two subordinate propositions which state the intellectual framework of the study: "Most analysts explain (and predict) the behavior of national governments in terms of one conceptual model here entitled Rational Actor or 'Classical' Model . . .," and "Two alternative conceptual models, here labeled an Organizational Process Model . . . and a Governmental (Bureaucratic) Politics Model . . . , provide a base for improved explanations and predictions" (pp. 4-5). As working hypotheses, these two subordinate propositions form the backbone of the study.

In treating the Rational Actor Model, the study points up the fundamental intellectual, psychological or analytical weakness upon which the model is premised: rational political man. However, it is doubtful if any of the foreign policy analysts who use this general model, do so on the assumption of rational man and/or rational behavior, since neither rationality nor rational behavior can be proved *a priori*. The determination of whether or not a decision/policy/program is rational is largely made *ex post facto* in relation to the objective(s) it was supposed to achieve or the goal(s) it was supposed to try and fulfill. That is, if the decision produced an outcome that was satisfactory or successful in terms of the

objective(s)/goal(s) it was supposed to achieve, then it might be considered "rational." For example, if the primary objective of the Kennedy Administration in the Cuban crisis was to get the "offensive weapons" out of Cuba, then the decision/policy/program arrived at was "rational" since the desired result was achieved, although it may not have been done in an optimal manner. However, even such a definition of rationality in terms of some previously stated objective is insufficient and inadequate unless it is placed within the larger national and international context and unless the costs involved in achieving the objective are carefully assessed. That is, if the Khrushchev Administration had not agreed to withdraw the offensive weapons on Sunday, October 28, and if the Kennedy Administration had felt compelled to fulfill its "explicit threat of air strike or invasion on Tuesday," October 30 (pp. 65 and 247—a new and possibly contentious point), then it might well be asked whether the decision/policy/program was rational in terms of the costs involved. On moral grounds, it is interesting to note that Robert Kennedy had intended to write about the "basic ethical question involved: what, if any, circumstance or justification gives this government or any government the moral right to bring its people and possibly all people under the shadow of nuclear destruction?" (*Thirteen Days*, p. 128).

The greatest value of the Allison study seems to be in the explication and development of two alternate models. The substance and style of the study in dealing with these two models is far more detailed, thorough and convincing than is the treatment of the first model. These chapters raise these two approaches or models to a new level of sophistication and analysis applicable to questions of foreign policy and national security. The first of these two models is what Allison terms the "Organizational Process" model. It draws heavily upon Herbert Simon's *Administrative Behavior* (1957), James March and Simon's *Organizations* (1958) and March's *Handbook of Organizations* (1965). The need for such a model is quite clear, because as Allison notes: "Few specialists in international politics have studied organization theory. It is only recently that organization theorists have come to study organizations as decision-makers; behavioral studies of foreign policy organizations from the decision-making perspective have not yet been produced" (p. 69). While this observation may not be entirely accurate (Henry A. Kissinger, *American Foreign Policy*, 1969, I. M. Destler, *Presidents, Bureaucrats and Foreign Policy*, 1972, and David H.

Davis, *How the Bureaucracy Makes Foreign Policy*, 1972), the point is well taken. The second of these models deals with "Governmental Politics" and draws heavily on Richard Neustadt's *Presidential Power* (1960), Roger Hilsman's *To Move a Nation* (1967) and Charles E. Lindblom's *The Policy Making Process* (1968). It emphasizes the importance of obtaining internal bureaucratic consensus on ends and means in order to achieve the desired outcomes. Almost inevitably, so far reaching a consensus has been difficult because, as the author notes, attributing the phrase to Don K. Price: "Where you stand depends on where you sit" (p. 176). The inferences and implications of "Price's Law" for decision makers are most interesting, whether they are defined as the President, Chiefs, Staffers, or Indians.

Allison concludes that each conceptual model greatly influences the assumptions, the tools of analysis and the results produced. This seems only logical since different conceptual frameworks elucidate particular aspects of a question, problem or issue, which can then be synthesized to explain so complex a phenomenon as the Cuban crisis. In the field of international politics/foreign policy, there are easily two dozen or more identifiable analytic approaches (e.g., James Rosenau, *International Politics and Foreign Policy*, 1969; James E. Dougherty and Robert L. Pfaltzgraff, *Contending Theories of International Relations*, 1971), upon which the researcher can draw to help analyze, evaluate, and interpret the various phenomena of international relations, crisis management, and conflict resolution. If the same analytic rigor exercised by Allison were also utilized in applying some of these other approaches to the Cuban crisis and other international phenomena, we would add greatly to our insight into such phenomena and to our understanding of the validity of the various methodologies used.

DAVID LLOYD LARSON

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China and Japan at War, 1937-1945: The Politics of Collaboration. By John Hunter Boyle. (Stanford: Stanford University Press, 1972. Pp. ix, 430. \$16.50.)

John Hunter Boyle has written a fascinating account of the efforts of a few Japanese and Chinese to substitute collaboration for war to enable their peoples to stand together against communism and Western imperialism. His book is easily the best study of the Chinese interest in collaboration and of the thought and

operational problems of the Japanese who reached out to them. Based on an impressive array of Japanese and to a lesser extent Chinese sources—and interviews with participants—Boyle's analysis unravels a host of tangles, especially of those threads leading from Chinese "puppets" back to Chiang Kai-shek and from Japanese negotiators to their nominal superiors in Tokyo. His conclusions are not likely to be modified—unless publication of his work jars loose new documentation from Chiang's personal files.

After considering the general problems of studying so-called puppet governments, Professor Boyle focuses on the Wang Ching-wei regime. He examines Wang's relationship to Chiang Kai-shek, and presents overwhelming evidence that Chiang knew and approved of Wang's initial efforts toward peace with Japan. He also explores Wang's personal motives for negotiating with and eventually coming to terms with the Japanese and does justice to the complex mix of patriotism, ideology, and ambition involved.

More intriguing is the story of the Japanese negotiators from whose perspective Boyle can see most clearly. Central to their efforts was the pan-Asian thought of Ishiware Kanji, the brilliant militarist forced into retirement because of his opposition to the Army's—and the Japanese government's—imperialism in China. Although himself an architect of Japan's seizure of Manchuria, Ishiware came to believe that the evolution of Chinese nationalism, combined with the growing threat of Soviet communism, required Japan to go off on another tack. He insisted that Japan be generous with China, respect China's national aspirations, work shoulder to shoulder with China against the Soviet Union—rather than drive China into alliance with the Russians and be faced with a two-front war on the Asian mainland. The men who negotiated with Wang Ching-wei shared Ishiware's vision. They understood and respected Chinese nationalism and sought to work with Wang not because they wanted a puppet but because they hoped to build a bridge to the Chinese Nationalists. But the efforts of these men, of Kagesa Sadaaki, Inukai Ken, and Imai Takeo, failed to produce the generous settlement all involved believed necessary because the Japanese negotiators could not obtain support for their program from the elements dominant in the Japanese Army or from the prime minister.

Once again we are granted insight into the incredible divisions that existed within the Japanese government, the ability of various com-

manders and their units to act independently of civilian or military authority in Tokyo. In China, army officers with their own ideas of what Japanese policy in China should be created their own puppet regimes, ignoring orders to the contrary. A host of Japanese agencies functioned in China, each intent on creating its own empire. For the most part there was only one shared element in Japanese planning and operations in China: an incredible ignorance of Chinese nationalism and a refusal to heed those few Japanese, like Ishiware and Ishii Itaro, who understood that China in the 1930s was not the old China of easily manipulated warlords. Such ignorance of another people's society, such operational independence on the part of military commanders would have been inconceivable to Americans—before Vietnam.

The interpretation of the New Order for East Asia that prevailed in the Japanese Army and was shared by Konoe left little leeway for Chinese who wanted to work with Japan. North China would have to be separated from any "national" Chinese government—the Army in North China would not surrender its fief. Inner Mongolia and Manchuria would cease to be Chinese territory. China's resources were to be "shared" with Japan on an obviously exploitative basis. On all these questions the Army commanders and agency heads involved were adamant, unwilling to consider more generous terms for peace even as the tide of war shifted in 1944. Boyle's analysis of the concessions Konoe could offer might be useful to students of Japanese-American relations who have wondered if the United States missed an opportunity to reach an accommodation with Japan in 1941.

Perhaps the most important of the many insights granted by a reading of this volume comes from the evidence that Japanese policy toward China and the rest of the world did not result from a rational assessment of the national interest conducted by the prime minister or responsible officials. On the contrary it is obvious that "Japanese policy" was in reality an aggregate of decisions made by various officers in the field and by various agencies at home over which the center, whether civilian or military, exercised little influence. This pattern of policy making will surprise few political scientists (and perhaps not many historians), and it is disappointing, therefore, to have Boyle nonetheless make frequent reference to "Japanese policy" or "Japan's purpose" as if the Japanese decision-making body was a monolithic force. Despite this caveat, the book is a most valuable contribution to the understanding of Japanese

thought and policy during the 1930s and 1940s—and to the history of China and Japan as well.

WARREN I. COHEN

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World Without Borders. By Lester R. Brown. (New York: Random House, 1972. Pp. 395. \$8.95.)

Provocative and readable, this book would be a good addition to the reading list of any introductory course in International Relations. Mr. Brown's analysis ranges over a wide variety of issues: environmental crisis, rich-poor gap, unemployment, urbanization, hunger, education, population, East-West relationships, economic interdependence, communication, transportation, multinational corporations, supranational institutions, and the quality of life.

The book's extensive scope contributes, of course, to some degree of superficiality. And there are occasionally irritating repetitions. But the work's primary value is its dynamic treatment of current and projected trends in international affairs for general readers—and especially for undergraduate students.

One should judge *World Without Borders* in the context of the present transitional period in American foreign policy. The book certainly illuminates the extent of foreign policy choices and calls into question the nature of American involvement in international affairs.

The two central issues considered in *World Without Borders* are social justice and the quality of life. Social justice is a perplexing problem for Americans, I would suggest, since it has been only a peripheral topic in our political history. Brown—a Senior Fellow with the Overseas Development Council—does a good job of conceptualizing the rich-poor, or North-South, gap in the world, but he doesn't really demonstrate his contentions about the "great stress on the international political fabric" which the growing economic disparities will cause. As Brown argues, technologically advanced societies are highly vulnerable to sabotage and disruption of energy supplies and raw materials. But social and political unrest in the poor nations will damage their own future more than it will harm the living standards of the rich countries.

Distributive justice requires more than humanitarian sympathies or assertions about future North-South conflicts. It necessitates a higher level of political leadership within the technologically advanced societies. One approach, recently suggested by Frederick Nossal,

is an international development tax. In a sense, there are already international taxes—paid by members of the World Bank and the United Nations. Perhaps the United States and other industrialized nations could return to the "luxury tax" system to finance global development programs.

Finally, Brown discusses the concept of the quality of life—which has become a fashionable political issue in recent years. "Reduction in poverty on a global scale," he says, "can be viewed either as a narrowing of the gap between rich and poor nations, or in terms of providing a minimum level of living for all people. Over the shorter term, the latter is a more realistic goal both because it is more easily attainable and because it focuses more directly on the critical human needs" (p. 322). In essence, the author urges a global minimum living standard—expressed in social rather than economic terms.

Regarding the rich nations, Brown argues that per capita income is becoming less satisfactory as an indicator of well-being, since the decline per person of natural amenities tends to offset growth in income. In America, the quality of life is a potential political bombshell; only the preliminary aspects of this political problem have appeared up to now.

Although some political scientists will be disappointed with the political analysis in *World Without Borders*, it is an impressive treatment of the threats to the well-being of mankind.

ROBERT H. PUCKETT

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The UN and the Palestine Refugees: A Study in Nonterritorial Administration. By Edward H. Buehrig. (Bloomington, Ind.: Indiana University Press, 1971. Pp. xvi, 215. \$8.50.)

In view of the mountains of words that have accumulated on the many aspects of the "Palestine Question," as it originally was, it is curious that Professor Buehrig's should be the first full-length study of UNRWA (UN Relief and Works Agency for Palestine Refugees in the Near East). For one thing, it is an institution quite different in several respects even from other operating welfare agencies of the UN system constituted under Article 22 of the Charter as subsidiary organs of the General Assembly—for examples, UNHCR (High Commissioner for Refugees) and UNICEF (the Children's Fund). This alone would make it worth the primarily legal and administrative study that Buehrig has written. One reason such a study has not appeared before now is because it could

hardly have been written from published records alone; the author did considerable field-work and had the cooperation of officials experienced in the actual operations of the Agency to help him put flesh on the bare bones of formal documentation.

In addition, UNRWA is one of the oldest continuing UN operational bodies, which justified an historical review after nearly a quarter-century. The study is a useful reminder of how and why it came into existence and what changes it has undergone in that period. This is all the more important since UNRWA is an embarrassing reminder of a basic element in the broader problems of Arab-Israeli relations and those of the major powers in the region—an element that governments and people are prone to push under the rug of more urgent political controversies, while withholding essential resources if the Agency is to carry out the responsibilities so regularly given it by a distant and often indifferent Assembly.

The author emphasizes the political as well as humanitarian origins of UNRWA and frequently notes underlying political causes of the legal and administrative difficulties discussed. The most basic of these was the mandate in Paragraph 11 of the 1948 Assembly Resolution 194(III), which authorized repatriation of those refugees who desired it and compensation for those who chose resettlement. This provision conflicted with the perceived requirements for a viable Jewish state; and of course there has yet to be a reconciliation through some compromise peace settlement to replace the 1949 Armistice Settlements. Buehrig ends his study with a wholly political conclusion that reverts to the origin of the problem:

As it did in the partition resolution of November 1947, the General Assembly could once again declare its support of a Palestinian Arab State. In such a context, the vain promise of repatriation could be abandoned. Furthermore, UNRWA would gain a new rationale. Instead of the invidious task of seeking through individual welfare to mollify a people for loss of a homeland, the Agency's acknowledged role would be to sustain the emergence of the Palestinians as an independent national community. . . . The United Nations could then relate to the Palestinians—for whose plight it cannot escape a measure of responsibility—as a source of financial and technical assistance. While the many repercussions of such a move by the General Assembly are not easily predictable, the net result might be better rather than worse (p. 189).

Although it is unfair to criticize an author for not writing a book he never undertook, it

must be said that it is also unfair to politically minded readers to put forth such a proposal without any discussion of its profound political implications, not even of those "repercussions" that might be fairly "easily predictable." This is especially so since, given the political content already included in the study, it is difficult to see how an Assembly declaration in the 1970s could be much more effective than the earlier proposal for the partition of Palestine.

Meanwhile, we can be grateful for this valuable analysis of the constitutional origins, the legal and operational problems of this unique agency. UNRWA was established by the General Assembly as a result of the unexpected turn of events in 1948 after Britain terminated its mandate over Palestine. It was given two objectives: the urgent one of immediate relief for hundreds of thousands of Arab refugees displaced by the war; and the longer-term one of creating employment as a means of assisting permanent resettlement of the refugees in neighboring countries. Buehrig describes how its activities in the first field evolved gradually into its current programs of health and welfare, while it was completely frustrated in its latter objective. He analyzes its constant struggle for autonomy and the problems of control growing out of its three-way relationship with its parent Organization, its host countries, and its contributing governments. Its mandated economic approach, in short, failed to resolve a problem bitterly political both in origin and in its continuing character.

After 1965, as Buehrig shows, UNRWA consequently turned from the "attempt to integrate displaced Palestinians into the surrounding Arabic society through economic programs," to the "cultivation of individual capabilities through basic education and vocational, technical, and professional training" (p. 147). One of his most interesting themes is that this gradual shift in emphasis eventually resulted in unexpectedly fostering the nationalist movement among the Palestinians, who were originally a politically and socially fragmented mass. UNRWA now, he concludes, "raises the question of whether humanitarian regard for the needs of refugees has been changed into subsidy of a political movement" (p. 189).

The answers to this and other questions raised by the UNRWA experience are not as simple as the political polemics on both sides would have us believe; but this study assists in clarifying them in many ways.

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Gehlen, Spy of the Century. By E. H. Cookridge. (New York: Random House, 1972. Pp. 402. \$10.00.)

The Game of the Foxes: The Untold Story of German Espionage in the United States and Great Britain During World War II. By Ladislav Farago. (New York: David McKay, 1972. Pp. 696. \$11.95.)

Project Paperclip: German Scientists and the Cold War. By Clarence G. Lasby. (New York: Atheneum, 1971. Pp. 338. \$8.95.)

The Double Cross System in the War of 1939 to 1945. By J. C. Masterman. (New Haven: Yale University Press, 1972. Pp. 203. \$6.95.)

OSS: The Secret History of America's First Central Intelligence Agency. By R. Harris Smith. (Berkeley: University of California Press, 1972. Pp. 458. \$10.95.)

No more fearsome research frontier exists than the secret and covert foreign operations of governments in war and peace. Fearsome, because hard evidence is elusive. The subject is important because enormous resources in war and peace have been allocated to secret warfare and to ferreting out secret information; and information is a crucial variable in decision making. During World War II thousands of individuals labored in the nether world of "strategic services." When the Truman doctrine was proclaimed in 1947, permanent secret strategic service and intelligence agencies were determined as essential to United States security. Since then, tens of thousands of persons have been engaged in a variety of secret activities costing billions of dollars. Most of this activity has occurred beyond public scrutiny. Indeed much of it has gone forward beyond the span of control of executive, let alone legislative authority.

This raises questions: What has been the nature and consequence of these secret governmental activities? What has been the basic setting for policy, organization, and responsible control? What have been and are the costs and the benefits? Recent books, listed above, shed some backlight on these questions.

J. C. Masterman's *The Double Cross System* is the most significant and unique of these works. Significant because it deals authoritatively with an important secret operation during World War II; and unique because it is a genuine secret document. Sir John, a British historian and Oxford don, wrote it in the summer of 1945 as a top secret report. It is an ac-

count of how British Intelligence, through an operation called "Double Cross," gained control of the entire German network of spies in Great Britain. The German agents were "turned around" by their British captors and used for counter-espionage and to deceive their former bosses. Deception about the time and location of the Normandy invasion in June 1944 was probably essential to success on D-Day.

Publication was ultimately urged upon the British government by the author because of its potential propaganda value. Many had come to doubt the efficiency of the Secret Services or its efficacy as a foreign policy instrument. Masterman became disturbed by such low public esteem and determined to gain governmental permission to publish because, as he writes elsewhere, "might not the relating of past successes do something to maintain belief in the efficiency of the service?"

But the government insisted upon censoring certain passages before allowing publication. This raises the question of whether the book, given an indeterminate amount of censoring, and given Masterman's primary motive in his ten-year struggle to get permission to publish, is not in some ways self-serving. Whether it is self-serving or not, it does paint the picture of a remarkable success. Masterman concludes by finding that "espionage in an enemy country is doomed to failure because the dice are hopelessly loaded against the spy" (p. 187). Furthermore, "espionage in wartime is difficult and usually unprofitable; counter-espionage is comparatively easy and may yield satisfactory results" (p. 188). He argues, however, that peacetime espionage is "easy and profitable." These are moot points. This work will be of use not only to espionage buffs, but to students of bureaucratic and international politics.

To OSS, R. Harris Smith has assigned an extravagant subtitle, *The Secret History of America's First Central Intelligence Agency*. Mr. Smith is a political scientist who served briefly in the Central Intelligence Agency. His book does not live up to its subtitle's promise. On a subject that has evaded most serious writers, Mr. Smith has produced a work that adds to our knowledge about a major World War II enterprise: the Office of Strategic Services. Denied OSS archives that remain padlocked under the custody of the OSS offspring, the C.I.A., Mr. Smith's "secrets" come primarily from contacts with 176 alumni of OSS. Even if the reliability of their memories after a quarter of a century is questionable, the author has tapped a valuable historical resource. Relying on their

memories after 25 years, and on a thorough search of thin bibliographical resources, he recreates some major OSS episodes in major war theaters. These range from the complex maneuvers in North Africa after the fall of France in 1940 to the tragicomic and confused American activities in Indo-China towards the war's end. Smith concludes by comparing the OSS with its successor agency, the CIA, which he calls "the mirror image of OSS." The author gives too much uncritical credit to OSS, without ever making sufficiently clear what were its contributions to victory. His characterization of CIA as a "liberal" force since World War II is inadequately developed. What we have, in effect, is some additional, maybe important pieces of a jigsaw puzzle, but the whole picture remains unassembled.

Project Paperclip: German Scientists and the Cold War, by historian Clarence G. Lasby, is an account of a little-known American operation after World War II in which the United States government "imported" more than 600 alien scientists and technologists for research and development in the atmosphere of Cold-War competition with the Russians. When the Third Reich collapsed, French, British, Russian and American intelligence teams organized competitive secret programs to recruit German scientists with special skills, particularly in military weaponry, especially rocketry. Most branches of the U.S. government, civilian and military, were involved. These bizarre programs came to be assigned the code name "Project Paperclip." Few Americans were aware of the coordinated, deliberate effort to compete for this Cold War human booty—what someone has called "intellectual reparations." Professor Lasby, of the University of Texas, has carefully interviewed more than 200 participants. Unlike Smith, he had access to government documents never before exploited. He sheds light on organization behavior as well as international politics, particularly Cold War origins. This is a model study of its sort, written with clarity and economy, and telling an unknown story.

E. H. Cookridge is a popular writer on espionage. His *Gehlen, Spy of the Century* is one of several recent books dealing with the uncomfortable fact that the United States relied heavily for many crucial years during the Cold War on the West German intelligence system organized and led by one of Hitler's chief intelligence leaders. Until recently, little was known about this invisible but powerful figure (who was Hitler's man for espionage and counterespionage in the Soviet Union). At war's end,

General Reinhard Gehlen was hired and subsidized first by the U. S. Army and then by the C.I.A. with hundreds of millions of dollars. In 1956, the Gehlen organization became the West German *Federal Intelligence Service* (B.N.D.). In this book we learn that the American intelligence establishment, without hesitation, was willing to support lavishly Herr Gehlen, who, according to Cookridge, "served Hitler with unswerving loyalty until the bitter end . . ." (p. 7).

The book displays the apparatus of scholarship, but it is surprisingly uncritical. Gehlen is portrayed as something of a hero but such a view probably will not survive a more critical examination of his overall role in exacerbating the Cold War. Perhaps Gehlen's most significant input in this regard was that he supplied seventy per cent of NATO's intelligence on Soviet and Eastern European affairs, according to Cookridge. This book has value in what it reveals, more implicitly than systematically, about intelligence policy, organization, doctrines, and use. But the reader is left to make his own judgment of the efficacy of Gehlen's massive apparatus.

A similar deficiency characterizes Ladislav Farago's *Game of the Foxes*. Farago, another writer of popular works, has produced a mammoth spy "bedside reader," of uneven historical value. Its principal claim to originality is its foundation on German secret intelligence archives, hitherto unused. These Farago has supplemented by dint of a prodigious amount of legwork, with interviews and a variety of unexploited archives. He has produced fifty-three vignettes; some of them have little to do with the *Abwehr*, and no analytical structure supports them. What did all of this secret activity add up to? In a few introductory paragraphs the author hints that it added up to very little, and he hints that it should persuade us to put little reliance on secret services. Again, the reader is left to make his own interpretation.

In surveying my five-foot-shelf of books on this subject, I accept these particular works as additional pieces of an important jigsaw puzzle—a puzzle still far from being assembled. These books contain hints at answers, albeit incomplete, to the questions posed initially in this review. Organizational behavior of secret intelligence organizations appears to transcend national boundaries; their behavior is shaped in the context of bureaucratic politics; much of their activity is useless; and yet evaluated information ("intelligence") is sometimes a pivotal element in decision making.

The term "strategic intelligence," however,

remains a compound of definitional and analytic confusion. It is used loosely to cover a variety of disparate meanings and functions, ranging from information gathering and analysis to espionage, sabotage, covert and clandestine political action, and to counterintelligence and paramilitary operations. These books provide only raw material for a better understanding of this secret side of government. They make little contribution to definitional or conceptual clarity on the subject.

HARRY HOWE RANSOM

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The French Presence in Black Africa. By Edward M. Corbett. (Washington, D.C.: Black Orpheus Press, 1972. Pp. xi, 209. \$12.50.)

At a time when French-speaking Africans are getting less and less Francophile, and when French aid to Africa is drawing increasing fire from metropolitan observers and African recipients, Professor Corbett's book is both timely and instructive. Rarely has French dominance in Black Africa been subjected to more intense scrutiny—and more devastating criticisms. More concisely and systematically than anyone else so far the author shows how little impact the formal granting of independence to France's African colonies has had on their subsequent relationships with the former imperial power; how, through various bilateral aid agreements, the recipients have placed themselves within the magnetic field of French military, political, economic and cultural influences; and how, through what he terms "the French mind-set of the African political and military establishments" (p. 1), these establishments have unwittingly facilitated France's cultural pre-eminence in the area.

In his earlier and excellent work on French Canadian nationalism (*Quebec Confronts Canada*, Johns Hopkins Press, 1967) the author dealt extensively with the plight of the Francophone minority in the face of a dominant "alien" culture. The shoe is now on the other foot. After pleading the cause of *Francophonie* in North America, the author takes up the cudgels against its European advocates and "native" hirelings in Black Africa. Ultimately, nearly every facet of the French presence in Africa is brought under the microscope and critically examined.

The chief merit of Corbett's *The French Presence in Black Africa* lies neither in the originality of his thesis nor in the nature of the evidence brought to bear upon his analysis. In one form or another the "French connection" to Black Africa has been a central theme in the

works of Samir Amin, Theresa Hayter, and Ti-bor Mende. Indeed, for anyone acquainted with Mende's most recent work (*De l'Aide à la Recolonisation: Les Leçons d'un Echec* [Paris, 1972]), the volume under review has relatively little to offer substantively. The value of the book lies elsewhere. Besides making available a body of data which is often too dispersed (or as yet untranslated) to be readily accessible, the theme around which it is constructed is one which has not received as much attention as it deserves from English-speaking social scientists. By drawing attention to the external roots of the dependency relationship which currently ties much of French-speaking Africa to metropolitan France, Corbett suggests some new and significant perspectives from which to look at problems of change and development.

Unfortunately, the author does little more than hint at some of the dimensions of analysis which need to be explored to do full justice to his topic. While there is some advantage in dealing serially with the various facets of the French presence in Black Africa (political, military, economic, cultural, etc.) one often wonders how exactly each relates to the others. What, for example, are the links between, on the one hand, French expatriate enterprises and aid agencies and on the other, the more explicitly political aspects of the French presence? What are the relationships between metropolitan pressure groups and the various aid agencies involved in *Cooperation*? What is the nature of the connection between the loosely-knit, camarilla-like organization built around Jacques Foccart (officially known as the General Secretariat for Community, African and Malagasy Affairs) and the several private or semi-public corporations operating in Africa?

To these all-important questions Corbett's book only offers the vaguest answers. In part this is because the questions to which the author addresses himself are never formulated in these terms, but one also suspects that the sheer inadequacy of his data (most of it drawn from official sources) is a sufficient reason for not raising such questions. In this respect one can only lament the very superficial treatment accorded to the various aid and development agencies, public and private, currently operating on behalf of the Secretariat of State for Cooperation. How these agencies came into being, how their personnel is being recruited, what their policy-orientations and ideological postulates are, and how all this in turn is likely to affect processes of development—these are matters that still remain to be investigated. Equally distressing is the absence of any serious

effort to investigate the social bases and ramifications of the top-heavy bureaucratic machinery built around the Secretariat of State for Cooperation. The suggestive findings made by William Cohen in his *Rulers of Empire* (Stanford University Press, 1971), especially as regards the interconnections between the Cooperation establishment and former colonial civil servants, invites a more systematic, in-depth investigation of the kind which one might have expected from a book such as Corbett's.

Perhaps the most disappointing aspect of Corbett's treatment is his neglect of what Suzanne Bodenheimer in a most stimulating discussion calls the "infrastructure of dependency" (*Politics and Society*, I, 3 [May 1971], 327-357). Although due attention is paid to the attitudinal variables which have influenced and continue to influence the receptivity of African elites to France's tutelage, little effort is made to relate these variables to socio-economic forces. To what extent is the concurrence of interests between African elites and their metropolitan counterparts rooted in class affinities? How dependent politically and otherwise are African entrepreneurs and bureaucrats on French financial, economic, and social resources? To what extent is the dominant position assumed by French economic interests in Africa the key to an understanding of France's political ascendancy in the area? Although these questions go well beyond the geographical scope of Corbett's study, their theoretical relevance to the problems discussed by the author is obvious.

Compounding these substantive shortcomings are a number of questionable statements and inaccuracies. For example: "The implications of growth are foreign to the concept [sic] African societies have of the world" (p. 24); "prostitution was unknown in precolonial Africa" (p. 26); "the conversion to the single-party system that is now general in former Black African territories is undoubtedly more directly attributable to the primitive political environment in which these governments operate than to a French model" (p. 50). In the light of these assertions one must be grateful to the author for not dwelling at further length on the African dimension of the French presence.

Given the aim which the author set for himself ("to undertake an analysis of sufficient detail to provide the basis for an appreciation of the . . . processes through which France exerts predominant influence in the new republics south of the Sahara," p. ix), it is perhaps unfair to take him to task for not doing more than he intended. It would be equally unfair not to rec-

ognize the merits of his work. For anyone interested in the impact of French policies in Black Africa, Corbett's book provides essential background information. If for no other reason it must have its place in any library dealing with French-speaking Africa.

RENE LEMARCHAND

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The Concept of Custom in International Law.

By Anthony A. D'Amato. (Ithaca and London: Cornell University Press, 1971. Pp. xvi, 286. \$9.50.)

According to the traditional view, international customary law is established by the repeated practice of many states if such practice is motivated or accompanied by a conviction that it is required by international law. In other words, the two elements are usage and *opinio juris sive necessitatis*. This technique presents numerous difficulties, especially in a rapidly changing world. How can the law develop if the procedure is rigidly applied? How can changes be employed, determined, and explained without violating existing law? How many, and which, states are required to engage in any given practice, and how can the existence of *opinio juris* be ascertained? Can one or a handful of states exercise a veto, and, if so, can this be done by silence, statements or acts? Can a rule retain its validity if it is challenged by some and defended by others? Is there a distinction between a state's words and deeds? And what is the situation if there is a discrepancy between the two? These are merely some of the questions which can be raised. To overcome these perplexing problems, Professor D'Amato attacks the classical construction and proceeds to reformulate the theory of custom. His formula consists of six requirements. The theory must be internally consistent, general but not vague, applicable to all the inductive evidence, parsimonious, objectively determinable, and claim-oriented. In a larger context the quantitative element of usage is replaced by acts or deeds; and articulation of a rule of international law, which contains several components and is the qualitative aspect of custom, is basically substituted for *opinio juris*. In this regard bilateral and multilateral treaties which contain generalizable provisions may generate rules of customary law and are no different from other acts or articulations which make up custom. Such a formulation "allows for the smooth working of change" (p. 98) in customary law.

Underlying the thesis are the suggestions that international law consists of only a few primary

rules and that custom is a secondary rule for finding the content of the primary or substantive norms; custom is the "most important" (p. 44) rule of law determination; it is "at the heart" (p. xii) of the international legal system. The authoritativeness of custom in turn is shown by initiative or imitation, the single act or the legal precedent, and reinforced by three elements—consent, estoppel, and reasonableness—elements which, while severely attacked by the author, are nonetheless accepted because they increase "the sense of legality" (p. 229) of custom.

In presenting what he describes as "a new approach" (p. xiv), Professor D'Amato strongly criticizes much legal thinking, regrettably on occasion in a sketchy or superficial fashion, an observation especially applicable to the McDougal-Lasswell conception of jurisprudence; and he advances certain views, particularly those on the law of treaties, which need further exposition and clarification. These omissions may well stem from lack of space, for the study reveals a good deal of erudition, though the excursion into custom in classical Roman law and English common law, while interesting, adds very little and could have been eliminated. This is all the more true in light of the author's repeated stricture that domestic law analogies are dangerous.

At the same time some statements and illustrations are too ambitious, while others do not stand the test of scrutiny. Among these is the observation that analysis of four cases of the Permanent Court of International Justice and three of the International Court of Justice include "all" (p. 121) the instances from The Hague Courts which indicate that conventional law may generate customary law.

Only much later, in the chapter entitled "The Concept of Special Custom," in which local custom is discussed, is a special rubric for cases on general custom found. Six instances, including two proceedings not mentioned in the earlier section, are recalled, and the claim is made that all of these have been examined in "preceding chapters" (p. 252). Nowhere is reference made to still another ruling, the advisory opinion on *Reparation for Injuries Suffered in the Service of the United Nations*, I.C.J. Rep. 1949 at 174, which reflects on this issue. Similarly, the author all too readily dismisses the holding of the Court in the *North Sea Continental Shelf* cases, I.C.J. Rep. 1969 at 3, that a purely conventional rule concerning the delimitation of the continental shelf had not established a customary regime. And he does so in spite of the fact that the Court knew, at the

time of the decision, that the Continental Shelf Convention, 449 U.N.T.S. 311, had received thirty-nine ratifications and accessions and that the equidistance method of delimitation had been employed on some fifteen occasions mostly in the form of bilateral treaties. Nor can it be maintained that the two Courts have adhered "tenaciously" (p. 248) to their precedents. Comparison between the *Eastern Carelia*, P.C.I.J. Ser. B, No. 5 (1923), and the *Peace Treaties*, I.C.J. Rep. 1950 at 65, opinions makes this clear. The language in the *Lotus* case, P.C.I.J. Ser. A, No. 10 (1927) holding that restrictions on the independence of states cannot be presumed on the one hand and the principle of collective fiscal responsibility advanced in the advisory opinion on *Certain Expenses of the United Nations*, I.C.J. Rep. 1962 at 151 on the other, provide a further illustration. Finally, the rulings on preliminary objections in the *Aerial Incident of 27 July 1955*, I.C.J. Rep. 1959 at 127, and the *Temple of Preah Vihear* cases, I.C.J. Rep. 1962 at 6, present additional evidence, as do the *South West Africa* cases, I. C. J. Reps. 1962 at 319 and 1966 at 6. Even more surprising is the assertion that during the height of the cold war, the Soviet Union and the United States "scrupulously" (p. 30) observed international norms on matters such as international airspace and embassies. The several applications on aerial incidents submitted by the United States to the International Court of Justice in the 'fifties attest to the opposite. A more striking example is the very event mentioned by Dr. D'Amato, namely, the U-2 flight. The electronic devices planted by the Russians in the U.S. Embassy plaque in Moscow and subsequently displayed at the United Nations by Ambassador Henry Cabot Lodge should also be noted.

Still, this is a thought-provoking book which should have wide appeal, and undoubtedly others will "join in the dialogue" (p. xiv) which the author has presented.

GUENTER WEISSBERG

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International Dissent: Six Steps Toward World Peace. By William O. Douglas. (New York: Random House, 1971. Pp. 155. \$4.95.)

The subject of this book is international cooperation, not international dissent as the title suggests. But this is its major strength. The vast areas of cooperation that lie submerged beneath the waves of international conflict are too often ignored, not only by journalists but also by professional students of international politics. Mr. Justice Douglas in his book properly

reminds us that we must concentrate more of our efforts on analyzing the dynamics of international cooperation. Unfortunately, however, he does not succeed in doing this himself.

The author suggests six "propositions" or "steps" toward world peace.

The first five are:

- (1) end all military alliances;
- (2) abolish all protectorates and colonies;
- (3) establish an international regime to regulate exploitation of the resources of the seas and ocean floor;
- (4) help developing nations enter the technological age;
- (5) establish rules of law governing international relations.

Douglas's sixth proposition, that the United States should recognize the People's Republic of China and that the Communist Government should be admitted to the United Nations as the sole representative of China, has already been achieved to a considerable extent. This latter move was solely within the discretion of the United States Government. The other steps, however, are not, and this points to the main weakness of the book, because the author discusses them as if they were.

It may be, as Mr. Douglas argues, that military alliances not only reflect but conduce to hostility and distrust among nations. There can be no doubt that the world's produced wealth could be better devoted to purposes other than the creation of massive military establishments. But he offers little insight into how to arrive at a world without military alliances. He does suggest that NATO, the Comecon, and the Warsaw Pact nations be united and that all of Europe be transformed into a nuclear free area. But for the most part, exhortation substitutes for analysis.

There is no question, moreover, that the community of nations should exploit the resources of the sea in a fashion that is just, reasonable, and ecologically sane. Nor are there many who would in principle disagree with the goal of enabling poor nations to close the technological gap that separates them from the more highly developed states. But since Mr. Douglas speaks of steps and of policy recommendations and not simply of desirable goals, he must be faulted for not describing the path as well as the destination.

A confusion between means and ends in fact prevails throughout much of the book, as does the absence of that type of middle-range reasoning that focuses on trade-offs, costs and benefits, and utility preference schemes.

These weaknesses become most glaring in Mr. Douglas's own bailiwick of the law. He quite correctly conceives of law neither as a set of immutable principles that exist over and beyond what states do, nor as sanctions imposed upon state behavior by means of punitive force. Laws, for him, are "cooperative regimes," those series of agreements on "procedures to settle differences," that sometimes occur, miraculously, in world politics. In defining law in such a manner, he points to a dynamic concept of law, as have a number of authors like Myres S. McDougal, Wolfgang Friedmann, Louis Henkin and Thomas M. Franck. Mr. Douglas's thoughts, however, at least as reflected in this book, remain rudimentary.

In his outline, for example, of the rules of law that should be adopted by international humanity, he specifies that all questions relating to boundary or territorial disputes as well as those pertaining to territorial and international waters, be submitted to international mediation and arbitration agencies. Another rule of law he advocates is the proscription of the sale of armaments to developing nations. Finally, he would have all governments outlaw invasion. There is nothing in this book, however, that shows how such "legal" rules can be transformed into practical possibilities. It is one thing to criticize the operations of national self-interest; it is quite another to demonstrate analytically how states might be induced to redefine national interests in ways that develop cooperation on a global scale.

The requirements of realism, however, should not obscure the value of this study. Mr. Douglas's book does underscore the fact that international law is largely comprised of restraints imposed by states upon themselves, sometimes even against their immediate self-interest, in order to protect systemic interaction, which is, after all, in the interests of all states. By calling our attention to the importance of systemic forms of international cooperation through and by which more durable laws can be developed, Mr. Douglas has rendered an important service.

EDWARD WEISBAND

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The United States and the Origins of the Cold War, 1941-1947. By John Lewis Gaddis. (New York: Columbia University Press, 1972. Pp. 382. \$12.50, cloth; \$3.95, paper.)

The Cold War has an ever increasing fascination for scholars. The questions posed by America's failure in Vietnam, recent works by Adam Ulam, Lloyd Gardner, Walter LaFeber,

Gaddis Smith, Thomas Paterson, Herbert Feis, Athan Theoharis, Gabriel Kolko, and others, combined with the growing availability of archival and manuscript sources, have established the early Cold War years as the most exciting and controversial period for historians of American diplomacy. While not definitive, John Gaddis's scrupulously researched new book will help bridge the gap between revisionists and traditionalists.

Professor Gaddis accepts some revisionist arguments and rejects others. He follows the view that American desires for a freely trading world ("open door") significantly influenced diplomacy, and his discussion of the Russian loan, lend-lease, and Bretton Woods reflects revisionist historiography. But Gaddis plays down economic motives, stressing instead a more general determination on the part of policy makers to profit from the lessons of the past. Traumatized by the failures of the 1930s, and rejecting isolationism, the Roosevelt and Truman administrations were determined not to repeat earlier mistakes. This time Germany would have to surrender unconditionally. There would be no reparations tangle. This time the United States would join an international organization to maintain peace, even if it required paying court to the sensitive Senator Vandenberg. Secret treaties were taboo; "there would be no 'Colonel House' business," said FDR (p. 13). Collective security and national self-determination were reaffirmed. And balancing this revival of Wilsonian ideals was Roosevelt's firm resolve to continue Soviet-American cooperation into the postwar world, even to the point of compromising certain issues such as Poland. While this American world-view encompassed definite economic values, Gaddis ascribes less importance to them than do most revisionist scholars.

"The most important cause" of the Cold War, according to Gaddis, was the "tension between the American principle of self-determination and Russian security needs" (p. 17). Roosevelt might have been able to square the circle in his own mind, but the public could not. Nor could Stalin fully appreciate the importance that Americans attached to free elections in Poland and elsewhere. Gaddis does not see any sharp break between Roosevelt and Truman on this issue, but he does show Truman as more influenced by anti-Russian advisers and more prone himself to make black-and-white judgments.

The development of American policy toward Germany, the second major focus of Soviet-American conflict, Gaddis argues, resulted not

from any conscious design in Washington, but rather from a complex combination of drift, bureaucratic competition, and military momentum. Not until 1946 did American statesmen begin to shape a German policy with Russia clearly in mind. In the case of atomic policy, Gaddis rejects Gar Alperovitz's thesis that Truman and Byrnes negotiated with the bomb on their hips, showing instead a growing realization that exclusive possession of the bomb could work no diplomatic miracles, a genuine desire to consider international control, and a gradual stiffening of the American position because of pressure from Congress. Indeed, in perhaps his best chapter, Gaddis contends that Truman's "get tough" policy actually began in February-March 1946—the result of pressure from influential Republicans like Dulles and Vandenberg, Stalin's famous "incompatibility" speech of February 9, Kennan's "long telegram" of February 22, Churchill's Fulton address, and the Iranian crisis. Thereafter "negotiations would continue, but future concessions would have to come from Moscow" (p. 356).

Were there alternatives to the Cold War? By stressing the role of Congress and public opinion, Gaddis disagrees with most revisionists. Hostility might have been avoided, he argues, had the United States started a second front in 1942–43, granted Russia a sphere of influence in Eastern Europe, given the Soviets a generous postwar loan for reconstruction, and relinquished the atomic monopoly. But neither Congress nor the public would have permitted such policies; hence, in William James's phrase, they were not "live options." Gaddis marshals impressive evidence to show that Roosevelt and Truman worried about congressional and public opinion and that the policies followed evoked a sympathetic response. He does not prove, however, that different policies and more farsighted leadership would not have worked. He plays down the extent to which Presidents can ignore Congress and manipulate public opinion, as evidenced by Vietnam. What Gaddis demonstrates is that a real Cold War consensus developed early, shared by statesman and man-in-the-street alike. There might have been more viable alternatives for American diplomacy in the Far East, about which Congressional and public opinion was less developed, but Gaddis deals with Asia only briefly.

The book is well-written and carefully documented, with notes at the bottom of the page. Although there is no bibliographical essay, Gaddis often makes incisive comments of a historiographical nature in the footnotes. It is a

book for historians, but no political scientist interested in the Cold War can afford to ignore it.

J. GARRY CLIFFORD

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The Foreign Powers in Latin America. By Herbert Goldhamer. (Princeton: Princeton University Press, 1972. Pp. xiv, 322. \$10.00.)

The United States and the Latin-American Revolution. By Martin C. Needler. (Boston: Allyn & Bacon, Inc., 1972. Pp. viii, 168. Price not indicated.)

One lesson to be learned from reviewing these books jointly is that it is not necessarily true that—as this reviewer shamefacedly admits he once believed—the stranger the bedfellows the more interesting, let alone exciting, the bed. The matter is not always that simple.

In *The Foreign Powers in Latin America*, Herbert Goldhamer has performed an interesting, useful, and possibly needed service. We live today in a world predominantly preoccupied only with the role of the United States in areas like Latin America. Goldhamer reminds us that Washington's is not the only finger in the inter-American *puchero*. There are other meddlers, other investors, other imperialists; and many of them are European. Even where diehards insist that the United States is the largest and most influential of these actors, Goldhamer at the very least provides us with a perspective for evaluating the range of the entire company of foreigners who complicate and otherwise influence the lives of Latin Americans.

We are therefore reminded that in the Caribbean there is not only a Puerto Rico but also a British West Indies, a French West Indies, and a Dutch West Indies. We knew that all along, to be sure; but a message of this undertaking is that it is sobering to be reminded of non-American influences in the Western Hemisphere. In assessing European and Japanese activities in Latin America, Goldhamer points out that a difference between those "imperialists" and the United States is that in general the former are not as concerned with national security considerations in Latin America as are the "North Americans." Soviet interests in the Caribbean immediately jump to mind. Here Goldhamer offers the interesting and probably valid judgment that the long-run consequences of Soviet involvement in Cuba have included a general cooling of Latin-American pro-Castro sentiment outside of Cuba without any significant

lessening of anti-United States attitudes in the Americas.

On the economic front, Goldhamer's study shows that, after all these years (his data begin with 1938), the chief non-American trader with Latin America is still Germany, with post-war Japan now in second place. The author explores the possible role of migration as an instrument of "foreign" influence in Latin America, but founders on the curious, long-known, but still little-understood proposition that most of the American republics have never attracted significant quantities of European immigrants. In the Americas Argentina has been second only to the United States in the exercise of this magnetism; otherwise, remarkably small percentages of Europeans have been persuaded to emigrate to the Americas over the years.

Goldhamer's data lead him to the conclusion, heavily economic in emphasis, that, in the years since World War II, the "foreigners" who have been most successful in achieving their objectives in Latin America have been the Germans and the Japanese; that the Soviet Union has been moderately so; and that the United States has been the principal loser.

Martin C. Needler's little book, *The United States and the Latin-American Revolution*, in one sense documents Goldhamer's proposition regarding "North American" losses in the Western Hemisphere. In another, and more fundamental, sense, Needler marches to a different drum. His work seems to be made up of more units of nostalgic ideology than of social science or—forgive the expression—realism. The book reads as though it were written in the late 1950s. Kennedy-type liberalism and faith in the Alliance for Progress are much in evidence. A similarity between Needler and me is that both of us were Kennedy Democrats. A difference is that he appears to believe that, having missed one boat, we should take it anyway. Would it really be cynical to attempt to book passage on a later vessel?

In any case, the contrasts between the two volumes are impressive. Goldhamer's is a serious study, a major undertaking; Needler's is a little book. The former is a systematic inquiry, employing reasonably scientific techniques and controls; the latter partakes more of the nature of an ideological statement. The former is intensively researched, despite Goldhamer's protestation that his is an "ignorancia casi enciclopédica" (p. vii).

President Nixon's name is invoked more by Needler than by Goldhamer. This is to be expected for a number of reasons, the most obvi-

ous being that one of these books is about the policy of the United States, and the other about the activities of non-American "foreigners," in Latin America. In addition to Needler, other students of the Nixon Administration have noted that Latin America is on the "back burner"—i.e., given only secondary attention—so far as present-day "North American" policy is concerned.

Despite the differences and divergences between the two books under review, they join together in underscoring the secondary character of their subject matter. Goldhamer takes advantage of this hiatus to give us a well-researched perspective, demonstrating that in today's world the various American republics are on everybody's—including the non-American "foreigners"—"back burner." This is enough to make Latin Americanists scream in anguish. But it is probably true. Whosoever, even Goldhamer, tells us the truth performs a constructive service. And Needler's message appears to be that we are still waiting for the boat we missed. To paraphrase an old anecdote, in one case the situation is serious but not desperate; in the other, it is desperate but not serious. Such are the ways of the "back burner."

Here are two books about the place of Latin America in today's world of international politics. Together they tell a story of secondary emphases and missed opportunities. Fundamentally, it is a true story. Read it and weep.

GEORGE I. BLANKSTEN

Northwestern University

Soviet Public International Law: Doctrines and Diplomatic Practice. By Kazimierz Grzybowski. (Leyden: A. W. Sijthoff; Durham, N.C.: Rule of Law Press, 1970. Pp. 544. \$18.50.)

This book is the first English-language work since 1935 to analyze in comprehensive fashion Soviet doctrines and practice in international law. Kazimierz Grzybowski, Professor at the Duke University School of Law, provides systematic treatment of Soviet views on international-legal theory, the state and recognition, jurisdiction, population, diplomatic and consular agencies, international organizations, treaties, and dispute settlement procedures. Drawing on his rich knowledge of international relations, particular as among East European states, Grzybowski illustrates Soviet doctrinal positions with numerous examples from Soviet diplomatic practice. He thereby keeps the book from becoming a dreary repetition of opinions of Soviet international-law publicists. Grzy-

bowski's extensive utilization of diplomatic practice is the book's strongest feature.

Professor Grzybowski's work, however, contains shortcomings of both a technical and judgmental nature. Grammatical and stylistic errors make the book difficult to follow at many points. The book's bibliography omits many English-language works that would be useful to a reader desiring to pursue more closely the topics covered briefly in Grzybowski's survey analysis. The book's title is open to question, since there is, in fact, no such thing as "Soviet public international law." Each country does not have its own public international law, but rather a set of doctrines and a history of practices with respect to a universal set of legal precepts. A title along the lines of Soviet "views" or "approaches" to public international law would be more accurate. In two instances, Grzybowski cites repealed enactments as representing current law. He cites the 1929 Soviet Merchant Shipping Code rather than the 1968 Code that replaced it (pp. 172-173, 174, 255) and the 1958 Comecon General Conditions of Delivery rather than the newer 1968 General Conditions (pp. 413-414).

Certain of Professor Grzybowski's judgments about the legality in international law of Soviet actions are questionable. He states that the Soviet refusal to recognize a right of innocent passage for foreign warships in Soviet territorial waters constitutes a violation of the 1958 Geneva Convention on the Territorial Sea and the Contiguous Zone, though most authorities agree that this Convention left that question unresolved (pp. 152, 158). In discussing the 1962 Cuban missile crisis, Grzybowski ignores the crucial legal issue, namely, the doctrine of "anticipatory self-defense" employed by the U.S. State Department to justify the U.S. blockade against Soviet missiles, and concludes, without foundation, that the Soviet removal of missiles from Cuba indicated a recognition by the U.S.S.R. of the legality of the U.S. blockade (pp. 177-178). In analyzing Soviet treatment of aliens, Grzybowski opines that "powers of the administrative authorities and courts in the Soviet Union are so vast that any action may be justified by adherence to the codes and regulations in force" (p. 85). Such an observation is more than a trifle exaggerated.

Professor Grzybowski underestimates the Soviet contribution to contemporary international law when he concludes that "the overall effect of Soviet doctrines on the organization of the modern community of nations is not as general as Soviet jurists and diplomats would

like the general public to believe" (p. 519). Commenting on early Bolshevik departures from traditional rules of international law, he suggests: "From the perspective of the present day, however, acts of the revolutionary government seem less radical as regards the transformation of the foundations of international order than they were proclaimed to be at the moment of the Revolution" (p. 100).

If those early departures seem less radical today, it is only because many of them have been accepted in the world community. State ownership of the means of production is now practiced in many nations to a greater or lesser extent, and this concept has caused revision of many traditional rules of international law. Similarly, the Soviet "pick and choose" approach to treaties concluded by the old regime following a social revolution has been pursued by many Asian and African states upon attainment of independence. Likewise, the wary attitude of these former colonies to international adjudication and to international organizations stems from the same motive that engendered Soviet uneasiness in these regards—a distrust of institutions developed by the West European states, staffed predominantly by West Europeans, and operated on West European principles. Thus, many current Third World attitudes toward international law find their first expression in Soviet statements and practices.

The People's Republic of China and the other Asian and East European socialist countries have also emulated Soviet doctrines and practices across a broad range of issues in international law. More generally, Soviet insistence on the use of broadly-based multipartite conventions has gone far toward undermining the traditional predominance of custom as the major source of international law.

Though Professor Grzybowski's book suffers from these shortcomings, it provides much useful information and much incisive analysis of Soviet views on international law, and it fills a long-standing gap in the legal literature.

JOHN QUIGLEY

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The Beginning of American Aid to Southeast Asia: The Griffin Mission of 1950. Edited with introductory chapters by Samuel P. Hayes. (Lexington, Mass: Heath Lexington Books, 1971. Pp. 334. \$15.00.)

As a major new effort in foreign assistance to Indochina impends, it is timely and interesting to read Samuel Hayes's compendium and evaluation of the start of the first post-World War

II aid efforts in Southeast Asia in 1950. The 1950 effort was launched by the visit to Southeast Asia of a U.S. government mission headed by Allen Griffin. Mr. Hayes was the deputy head of the mission. Coming in the wake of the Communist victory in Mainland China, the mission had the task of finding suitable projects for U.S. assistance that would "have immediate political significance," as well as laying the groundwork for an anticipated economic development program "of unknown dimensions" in the future.

To this end, the Griffin Mission visited Vietnam, Malaya, Burma, Thailand and Indonesia during March and April, 1950. Its reports and recommendations, which are published for the first time in this volume, proposed economic aid for the five countries totaling \$66 million initially, concentrated principally in agriculture (about 35 per cent), with smaller amounts in other fields including power and transportation (26 per cent), and public health (24 per cent). The Griffin Mission's recommendations were implemented with remarkable dispatch. Within a matter of months, country missions were on site in the several countries to start the programs, and to establish the procedures and projects which provided the basis for the continuing assortment of U.S. aid programs in Southeast Asia.

Mr. Hayes notes this record of speedy implementation with pride: "The fact remains that prompt action was desired and prompt action took place. It is a history not likely to be often repeated" (p. 58). It is not unreasonable to consider in retrospect whether one ought really to wish for repetition: perhaps more thought, caution, and deliberation need have turned out no worse.

The materials that are published in the Hayes volume are valuable reference sources. They reveal plainly the implicit premises that underlay much U.S. foreign assistance in the post-World War II era. The premises and implicit theories include: the "strategic importance" of Southeast Asia; the strength of internal nationalism as a source of political support in the Southeast Asian countries; the international and domestic political effectiveness of foreign economic aid; and the important catalytic role of foreign aid in internal economic development. These premises were not necessarily wrong in the 1950s, although they are, to put it mildly, unpopular in the 1970s. Nor, at the same time, can it be said that they have been fairly or critically analyzed, let alone tested—then, or since.

In light of subsequent experience, some of these premises look sounder two decades later than others. Yet even the ones that look sounder are oversimplifications; on the other hand, even the ones that seem *less* sound by hindsight, have some validity, too. What the premises share is a tendency to assert single and simple explanations for phenomena that have multiple and complex causes.

On the eve of the new programs of reconstruction and development in Indochina, a review of the Griffin Mission reports has particular timeliness. One cannot be sure of what lessons should be drawn from this background, but a few may be worth considering as the new aid effort gets under way: Don't expect too much from the programs, either economically or politically; try to be clear about criteria—what is aimed for, and what is reasonable to expect; try not to make it a U.S.-dominated "show"; make the number of U.S. personnel as limited as possible.

CHARLES WOLF, JR.

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Law and Policy in China's Foreign Relations:

A Study of Attitudes and Practice. By James C. Hsiung. (New York: Columbia University Press, 1972. Pp. 435. \$12.50.)

Although much has been written about the PRC's international relations, insufficient attention has been given to her attitude and practice with respect to international law. The present volume is thus especially welcome, for it addresses itself to the question, "To what extent does Communist China, armed with a militant ideology, accept the existing rules of the game in international relations?"

Composed of fifteen chapters, the book analyzes a wide range of vital issues concerning law and policy in China's foreign relations. It shows that international law has been used either to facilitate Peking's foreign policy, to support its claims to certain international rights, to protect Chinese interests and nationals abroad, to defend certain official conduct, or to help the carrying out of trade or other similar relations.

A professor of politics at New York University, the author writes with commendable objectivity and perceptiveness in his effort to evaluate the interaction of law and policy in the PRC's decision-making process. He correctly suggests that both in theory and practice, China has been quite selective and more pragmatic than is generally assumed, accepting certain norms of the established code of international

conduct but rejecting others. According to him, the widening gaps (1) between the PRC's doctrinal bellicosity and pragmatic cautiousness, and (2) between her ideological identification with the socialist bloc and her expanding ties with countries outside the bloc, signify an evolutionary (as opposed to dogmatically immutable) character of her foreign policy. Nevertheless, he cautions, it may take years before Peking can be expected to attain a level of maturity and flexibility that we would like to see.

One of the highlights of the book is its treatment of peaceful coexistence (PCX) and proletarian internationalism (PI), two fundamental principles of "modern" international law endorsed by the PRC and the Soviet Union. Through a careful analysis of their development over the years, the author points out that PCX and PI have always served the policy needs of Peking as well as Moscow, that the evolving meanings of the two doctrines have always followed the changing policy needs of the decision makers, and that Sino-Soviet divergencies over the application of the two doctrines have always reflected deeper differences in policy.

Equally interesting is the book's examination of the PRC's position in regard to the sources of international law. While there is little doubt about the PRC's recognition of treaties and custom as the major sources of international law, considerable confusion has existed about her attitude toward the resolutions of international organizations. The author successfully clarifies the picture by showing that Peking accepts certain decisions of international organizations as a source of international law so far as they embody a construed *opinio juris* (representing the consensus of a majority of the "nonimperialist" countries and subscribed to by the PRC herself).

On the whole, *Law and Policy in China's Foreign Relations* is a well-researched and well-written book. There are, however, some shortcomings. The organization of the volume, for instance, leaves something to be desired. To make the flow of discussion more smooth, Chapters IX and X on practical use of international law probably could have been either integrated into the other chapters or put at the end of the book. One also wishes to see a more detailed analysis of the PRC's stand on airspace and on the principle of self-determination. Another inadequacy appears to be the absence of any discussion of Peking's approach to the general question of human rights.

These "fringe" criticisms do not distract from the important contribution made by the book to our understanding of the PRC's foreign policy and world view. This volume should prove valuable to Chinese specialists and international lawyers alike.

SHAO-CHUAN LENG

University of Virginia

Development Today: A New Look at U.S. Relations with the Poor Countries. Edited by Robert E. Hunter and John E. Rielly. (New York: Praeger Publishers, 1972. Pp. x, 286. \$9.00.)

These papers—some of which have been published elsewhere—were originally presented at seminars of the Overseas Development Council (ODC). They vary considerably in terms of depth of analysis and sophistication.

The most provocative piece in the volume is "Foreign Aid: For What and for Whom," by Samuel P. Huntington. According to Huntington, "true believers" in foreign aid have done their cause a disfavor by repeated and fumbling attempts to find a grand rationale for foreign assistance. Huntington argues strongly that long range economic development of the Third World is not so important to U.S. interests that it ought to be the primary goal of our foreign aid programs. There is little evidence that a United States commitment to development of the Third World will bring political gains to the U.S., either in the short run or the long run, by strengthening political alliances or winning new allies. On the contrary, strains which arise in bilateral assistance relationships often have negative implication for U.S. foreign relations.

Huntington argues for a multi-objective approach to foreign assistance and a decentralization of aid programs. Assistance for long range economic development should be the function of multilateral development institutions such as the World Bank and the regional development banks. According to Huntington, legitimate objectives of bilateral aid, which should be administered by the State Department, include the following:

(1) Granting of economic assistance to a friendly government to help it consolidate its position after coming to power, survive temporary economic crises, or win a crucial election; or

(2) Granting of assistance to a government in return for something we want such as base rights, a U.N. vote, military cooperation, freer

access to the country's markets for U.S. exports or U.S. investments, or maintenance of a cease-fire agreement.

Military assistance, says Huntington, ought to be entirely a Defense Department matter. Separate authorizations and appropriations, rather than an omnibus Foreign Assistance Act, should cover the various elements of the program. Unfortunately, Huntington gives little attention to humanitarian and technical assistance, the focus of much of AID's current activities. His proposed dismantling of AID would leave these programs in limbo.

Huntington's essay is followed by an article by Willard Thorp, entitled "Foreign Aid: A Report on the Reports" which describes the evolution of thinking on foreign aid by various official commissions, the kind of thinking that Huntington feels is so misguided.

Robert Hunter discusses United States interests in Asia and the Middle East, the problem of violence in the Third World, and participation by LDC's in international cooperation on monetary and trade matters. John Rielly focuses on Latin America, the tension between United States and European roles in that part of the world, and U.S. security and trading interests in Latin America. Neither the Hunter or Rielly essays contains strident calls for foreign assistance; rather they represent low-key efforts to emphasize the extent of U.S. interests in the Third World, with suggestions as to the role that development assistance might play in furthering these interests.

The book ends with a section on the U.S. Congress and foreign aid. Representative Donald Fraser from Minnesota explains the lack of congressional support for aid as due not only to the Vietnam backlash and increasing concern with domestic problems, but also to the significant reduction in the last decade among aid-supporting liberals in Congress. Senator Frank Church outlines his opposition to U.S. bilateral, foreign-aid programs on the basis of what he regards as false premises advanced by aid advocates: (1) the existence of a Communist monolith that has to be checked in its imperialistic drives, and (2) the belief that foreign assistance can prevent a country from "going Communist." He also argues that our aid programs support undesirable dictatorships, maintain the status quo when radical change is required, and line the pockets of American exporters and contractors. Robert Hunter concludes the volume by rebutting Church's arguments, writing that no respectable proponent of foreign aid in

the last decade has used the Communist monolith argument or maintained that aid provides political guarantees. He does not see radical political change as a necessary precondition for economic development, and adds that exporter and contractor profits derived from foreign aid programs need not demean their usefulness to the recipient countries.

A number of other essays in the volume deal with trade, the multinational firm, violence and development, population growth and environmental stress, and employment and income distribution. One of the most interesting of these is an article by James Grant, "Jobs and Justice: Economic Growth Alone is Not Enough." Mr. Grant points out the vastly different levels of performance among countries in reducing unemployment, improving income distribution, and providing educational and health services for the lower economic strata within developing countries. He notes that the progress of some rapidly growing countries in achieving social goals would not have been possible without economic growth, and that success is often associated with broad access to foreign assistance, international trade, and foreign investment.

The articles in this collection cover a wide range of topics and vary in quality and interest. It is difficult to find a rationale for publishing them in a single volume. Nevertheless, the book is a useful addition to the library of anyone interested in foreign aid and economic development because of its five or six best essays.

CHARLES R. FRANK, JR.

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The Cyprus Question. By Leontios Ierodiakonou. (Stockholm: Almqvist & Wiksell, 1971. Pp. 311. Kr. 30.)

The study of international disputes can be pursued either on a substantive level aiming at the illumination of particular conflicts or on a comparative and theoretical level with individual cases studied in order to clarify questions of general significance. To be sure, no firmly founded comparative and theoretical projects can be undertaken in the absence of reliable case studies which therefore are always pertinent.

Mr. Ierodiakonou's book on the Cyprus Question falls within the first category of international studies. It is a work of good scholarship offering a balanced, well-reasoned and comprehensive account of the international activity provoked by the nationalist revolt in Cyprus in the 1950s. The narrative suggests that what began as a straightforward demand for self-determination on the part of the Greek Cypriots ended up, after increasing internation-

alization, in political deadlock evolving out of: (a) bad timing and poor tactics on the part of the Greek side; (b) manipulative British policies dictated by preoccupations with Middle-eastern oil resources and the exigencies of the Cold War; and (c) the Turkish government's efforts to cope with popular discontent by diverting attention from difficult conditions at home to nationalist grievances abroad.

Repeated recourse to the United Nations did not provide a way out of the stalemate which was further aggravated by continuing violence in Cyprus. Active diplomatic pressure in the interests of the Western Alliance led to the compromise of the Zürich-London Agreements (1959) among the Greek, Turkish and British governments, setting up the Republic of Cyprus. The outcome essentially amounted to abandonment of self-determination by the Greek Cypriots, while at the same time it constituted a net gain for Turkey and attainment of the British policy objectives (pp. 222-227).

Although the main focus of the study (chaps. 2 to 8) is on the international dispute of the 1950s, a substantial Epilogue (pp. 228-300) covers the application and collapse (in 1963-64) of the Zürich-London settlement as well as subsequent political developments in Cyprus. An attempt at a profile of local political dynamics (e.g., political alignments, interest groups, foreign influences, etc. (pp. 275-300)), suggests the importance of these factors for a sound understanding of the Cyprus Question.

In his otherwise useful survey of the literature on the politics of Cyprus (pp. 14-17), Mr. Ierodiakonou should have cautioned us that many of these books are biased statements of divergent points of view rather than reliable factual accounts. The documentation of the argument, though detailed and careful, suffers from some discrepancies. The most interesting source explored is the *Hansard*. Had the author also drawn upon the relevant Greek and Turkish parliamentary debates, his argument would have been more complete. As he claims on p. 17 it is indeed harder to obtain Greek and Turkish sources but this is not a serious excuse, especially since other scholars have already made fruitful use of the proceedings of the parliamentary bodies of Greece and Turkey. (Cf. for example Th. Couloumbis, *Greek Political Reaction to American and NATO Influences* [New Haven and London: Yale University Press, 1966]; C. H. Dodd, *Politics and Government in Turkey* [Berkeley and Los Angeles, University of California Press, 1969]; and more recently P. Terlexis, *Diplomacy and Politics of the Cyprus Question* [in Greek], [Athens: Editions Rappas, 1971.]

In the framework of the historical-juridic approach employed in this study, an attempt is made to discuss the Cyprus Question with reference to wider international issues such as the interpretation of the U.N. Charter and the application of the principle of self-determination. The discussion of this concept (pp. 40–44) is a good one, but it could have been much more pertinent if the author had not ignored certain important contributions on the subject (for example, W. Conner, "Self-Determination: the New Phase," *World Politics*, Vol. 20 [October 1967], 30–53, and V. Van Dyke, "Self-Determination and Minority Rights," *International Studies Quarterly*, 13 [September 1969] 223–253).

In general the author does not show any awareness of contemporary analytical approaches to international relations—approaches which could have provided remarkable insights into the problem of Cyprus. An initial effort to present the dispute as the outcome of the social and political forces at work in Cyprus, Greece, Turkey and Britain (chap. 1) is a good attempt in the direction of historical sociology, but the final product falls short of one's expectations. In particular the treatment of the nationalist *Enosis* movement in Cyprus, which has been a major driving force complicating the Cyprus Question, is quite unsatisfactory (pp. 18–23).

As a result, the analysis remains on the level of external facts (i.e., official policy) without coming to grips with the dynamic nature of the problem. An international dispute can be understood through an analysis of the factors that make it difficult for ethnic groups and nations to coexist in peace. Such sources of conflict include differences in culture and historical experience, psychological tensions, clashes in nationalist aspirations, socioeconomic antagonisms and political incongruities, discrepancies in levels of modernization and so on. Cyprus is a microcosm of broader Greek-Turkish conflicts, a microcosm in which several of these dynamic patterns can be studied with fruitful results both on the analytical and the policy level. For example, a systematic typology of sources of conflict could be developed to serve as the basis for the formulation of resolution strategies. In this way the study of the Cyprus Question would certainly acquire much wider interest.

One may conclude by noting that Mr. Iero-diakonou's book, though a reliable case study factually, does not carry us into new territory in analysis and interpretation.

PASCHALIS M. KITROMILIDES

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The Oder-Neisse Boundary and Poland's Modernization: The Socioeconomic and Political Impact. By Z. Anthony Kruszewski. (New York: Praeger Publishers, 1972. Pp. xviii, 245. \$16.50.)

World War II and the arrangements which followed immediately afterwards shifted the geographic location of a resurrected Poland dramatically westward. Six million Poles, the majority of whom resided in the rural East which was ceded to the Soviet Union, were moved to the Western and Northern regions acquired from a defeated Germany. Professor Kruszewski's book deals with the economic, demographic and sociopolitical changes which resulted from these shifts, and with their impact both within the "new" territories and for the nation as a whole.

This is a meticulously documented study based upon statistical and archival evidence, some of it previously unknown. In order to piece his story together the author had to overcome the usual difficulties encountered by an "outsider" doing research in Eastern Europe and the Soviet Union, not the least of which is often the lack of reliable statistical data, especially for the early postwar period. That he has managed so well is a credit to his patience and ingenuity.

Although the boundary changes, especially those involving divestiture of the Eastern territories, met with the resistance of the Poles (with the exception of the Communists), these have ultimately turned out, in the view of the author, to be beneficial to the sociopolitical and economic life of the nation. If within the economic framework of prewar Germany the territories acquired by Poland were considered relatively backward industrially, their acquisition marked a giant step for Poland on the road toward industrialization, urbanization, and technological development. According to Kruszewski, this, more than the official ideology and the concomitant sociopolitical revolution, has contributed to the transformation of Poland and to the changed character of its population. In addition to attracting those resettled from the East, the new territories served as a magnet for the rural youth who could not find a future on the traditional Polish dwarf farm, for first generation proletarians, and for re-immigrants from Western Europe and the United States. Others were forcefully resettled into the Oder-Neisse regions. The new settlers took up residence alongside indigenous residents, the so-called autochthons, that is, ethnic Poles—however strongly Germanized—who always lived in these areas. The newcomers have taken the places vacated by ethnic Germans who either

fled or were forced to move into a partitioned Germany.

According to Kruszewski, integration of the new territories into the rest of the country appears to have been smoother than its own postwar socioeconomic and political stabilization. The natives to the area, for example, the aforementioned autochthons, although having suffered under the preceding German administration and longing for a return to the Polish homeland, had envisioned a Poland far different from that which emerged from the ashes of World War II. They were old-style patriots, devoutly religious and conservative, and felt out of place in a militantly secular "people's democracy." Moreover, their dialect, heavily laden with German phrases, and the past service of their menfolk in the German armed forces, made them suspect in the eyes of the newcomers. Many of them are currently migrating to West Germany under terms of the Warsaw-Bonn treaty. The economy itself suffered destruction since the Soviet armies moved machinery and entire plants, as war booty, before handing over the region to the Poles.

In this situation of flux, the author maintains, the Church served as the mainstay of order and continuity while the Party, although lacking in broad social support and influence and frequently forced to seek the moral assistance of the Church, offered organizational and managerial muscle. In fact, the economic needs, on the one hand, and the unpopularity of the dominant Party, on the other—coupled with mutual mistrust—offered many otherwise politically "hostile" elements opportunities for developing their energies and talents along non-political lines, e.g., in technical management, education, non-Party activism, etc.

Kruszewski somehow refuses to draw generalized conclusions beyond his narrow subject-matter focus, although these suggest themselves almost naturally. More general conclusions, however, would have made the book more important to the reader interested in international politics, especially as it affects problems of development.

Thus, what becomes obvious is that minor states are very much at the mercy of the big powers, the megalomania of their native elites notwithstanding. Another conclusion concerns the use of boundary changes and population shifts as instruments in international politics. It was Winston Churchill who as early as 1944 advocated mass population expulsion as a pragmatic and just course of action. Kruszewski cites him as saying: "... expulsion is the

method which, so far as we have been able to see, will be the most satisfactory and lasting. There will be no mixture of populations to cause endless trouble as in Alsace-Lorraine. A clean sweep will be made" (p. 23).

Generally, a country such as Poland, economically underdeveloped, emerging from near total destruction in the aftermath of an exhausting war, has politically and economically two courses of action open to it in the early postwar years, neither of which is very appealing: It can turn to foreign capital for assistance in the task of reconstruction, thereby becoming an economic as well as a political colony, or it may attempt to exploit its own resources, including its own manpower, via a system of mobilization and regimentation. If the first alternative is rejected, the second may serve as an excuse for the type of repressive regimes which emerged in the immediate postwar years in Eastern Europe. In fact, the author reproduces in an appendix a hitherto unpublished OSS document which suggests that a "rigorous dictatorship would be . . . the most likely political framework of an economic policy involving changes as radical as may be necessary to lead Poland out of its prewar stagnation" (p. 212). The same document also maintains that "good relations between the United States and the USSR is the most important single condition" in the postwar period and, therefore, East European countries whose fate will be decided ultimately in the chancelleries of the great powers, have little to hope for by pushing for a wedge between East and West.

The author overstates perhaps the impact of the boundary changes at the expense of other change-inducing factors. Unquestionably, these changes and the desire to make these changes permanent have aided in gaining legitimacy for the postwar Polish system in the popular consciousness, and to the extent that the current leadership (and the Soviet Union) can portray itself as the major guardian of the present status quo (especially as related to the new national frontiers) it will enjoy mass support, although without love or admiration. While Professor Kruszewski evidences some ideological biases, his treatment of the subject is admirably sober, generally understanding and sympathetic although somewhat critical. It is refreshing because it departs so markedly from the emotionalism characteristic of much of the "emigre literature" when dealing with the problem of Polish frontiers, East and West.

JOSEPH R. FISZMAN

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Forecast for Japan: Security in the 1970's. Ed. by James W. Morley. (Princeton: Princeton University Press, 1972. Pp. 249. \$9.50.)

Although writing independently in this volume, the six authors of seven chapters began their study as an analysis for the U.S. Arms Control and Disarmament Agency. That is probably why they worked within the time-frame 1970-75. This allowed projections to be rather firm, yet in certain techno-industrial fields (nuclear reactors, fuel enrichment, missile and space capabilities, air and naval expansion) and related strategic options, projections to 1980 and '85 would have been helpful. There is some of this longer-range thinking in Donald Hellmann's chapter (V) on Japan's Asian environment and defence choices, in Morley's concluding "Forecast," and—by implication—in Kenneth Young's reasoned foresight as to Japan's interests and probable policies in Southeast Asia (Chapter VI).

Although Martin Weinstein's chapter (II) provides a history of conservative Japanese diplomatic and security policies since 1947, the book opens with Morley's brilliant depiction of the calculated economic parameters within which Japan's rearmament has been and will in the future be conducted. The particularly fresh contribution provides data about the significance of gross incremental capital-output ratio during 1959-68 and as estimated for 1969-75, Morley concludes that in terms of what that country's economy can sustain, its leaders (conservative during the foreseeable future, write Nathaniel Thayer and Frank Langdon in the third and fourth chapters) have the following options: (1) they could continue spending less than 1 per cent of GNP (under 8.5 per cent of total governmental expenditures) on military items and so attain middle-power status by the mid-seventies; or (2) just from economic considerations they could (by investing about 2 per cent of GNP) make their country a major power; or (3) with much more strain, controversy and international suspicion—and over a longer span—they could even match their status as an economic superpower in military fields.

All of the writers except Hellmann, whose analyses and projections are more "hard-nosed" and deterministic, think that Japanese conservative governments will continue to prefer economic and increasing political emphases in diplomacy and continuation of the first-mentioned pattern of security measures. This would enable Japan to develop much more adequate conventional defenses of its home islands. Only

Hellmann has expressed here a strong prediction about probable Japanese responses to any serious threats to its vital use of sea lanes (i.e., that in the future Japan will probably see the need to protect them). The other authors think that, if the United States is to play a significant though reduced role in East and Southeast Asian affairs, Japan will continue for some time to depend on the U.S. nuclear shield, and on offshore American air and naval power. Any serious reason to doubt American will to fulfill the responsibilities of its alliances in that area and the Western Pacific would tend to cause Japan to change its policy options. Hellmann is probably correct in positing that the more distant time when the People's Republic of China will probably become capable, by an array of ICBMs, of placing the continental United States under nuclear threat will be a crucial one for Japanese strategic policy makers.

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International Conflict and Social Policy. By Marc Pilisuk. (Englewood Cliffs, N.J.: Prentice-Hall, 1972. Pp. 199. \$5.95.)

In this slim volume the author, a professor of social welfare at the University of California, Berkeley, presents a subjective and poorly documented interpretation of the nature and purposes of American Foreign policy. His analysis covers such diverse topics as the origins of strategic thinking and its impact on the policy process, the role of the CIA in foreign policy, the exploitation and subjugation of the Third World, and the impact of the military-industrial complex on foreign policy.

The central thesis of this work is that the U.S. has developed a vast military and paramilitary foreign policy which reflects the interests of powerful corporate and bureaucratic factions. Many of the policies of these factions are designed to support reactionary anti-Communist regimes favorably inclined to the protection of American economic interests.

A reader familiar with the current debate over the nature and purposes of American foreign policy will recognize this thesis as representative of the "revisionist" interpretation of American foreign policy. On balance it adds very little to the elaboration of the revisionist argument and even less to our understanding of American foreign policy.

In his introduction the author informs the reader of his preference for the analysis of "concealed data" as opposed to the "systematic ordering of easily available data" (p. 4). The

methodological problems in the use of concealed data are considerable, and unless handled carefully, can pose significant problems for the author. The author does not discuss what he means by concealed data, and his method of dealing with them does little to inspire confidence in the objectivity of his analysis.

In particular, he fails to provide the reader with information on the method of inquiry followed in uncovering the data. As a direct consequence of this failure, the reader has no way of determining if the methods of inquiry are consistent with the canons of scientific method. One gains some insight into the methodology of the author by noting that the references listed consist almost entirely of secondary sources. Pilisuk does not indicate what information is distorted or concealed. Moreover there is no serious effort to document distortion and concealment.

The second major methodological weakness is Professor Pilisuk's tendency to assert factual statements which appear at first glance to raise fundamental questions but which upon further analysis are poorly supported by the data presented. This failure can be seen in the following examples:

In his analysis of the policy of the United States toward the United Nations he contends that "American domination of the United Nations is as pervasive as before" (p. 18). The evidence presented for this rather controversial assertion is the role of the UN in supporting U.S. policies in Korea and Hungary. These cases are supposed to prove the case of domination, and they are cited as typical of the past record. Yet the cases cited are from the 1950s. Is the author unaware of the profound changes which the UN has undergone since 1956? The author might have cited cases of comparable control over the UN in the 1960s. Or better still, he might have provided the reader with measures of the degree and scope of U.S. control over the years and across a range of issues. As it stands his thesis lacks adequate supporting data.

In his discussion of the policies of the United States in the Third World, Pilisuk asserts that "greater foreign capital imports are correlated with lower rates of economic growth" (p. 36, emphasis in the original). This thesis runs counter to the central premise of much of the literature on economic development and foreign economic assistance and, if substantiated, would constitute a significant finding. There are no data which demonstrate the statistical corre-

lation between economic growth and capital imports as asserted in the quotation. In the absence of such data this conclusion cannot be accepted as an objective and verified description of the relationship between imports of capital and economic growth.

Pilisuk offers case studies of American interventions in Greece and the Dominican Republic. His descriptive narrative of these cases adds little to the existing knowledge on these past events. The accounts rely heavily on secondary sources and do not reveal any new data. The interpretations are very one-sided and the explanations offered are oversimplifications. For example, the Dominican intervention is explained by reference to the ideological predispositions and economic interests of the principal policy makers. Important considerations such as the Johnson administration's concern over the domestic and international ramifications of the insurrection are not examined. These variables have no place in an economic interpretation of American foreign policy and are apparently rejected out of hand by the author.

On the plus side, the book contains interesting discussions of the military-industrial complex and the impact of strategic thought on strategic policy. These chapters may be of some interest to the general reader, although the specialist will find them of marginal value.

JOHN W. ELEY

Bowling Green, Kentucky

Southern Africa in Perspective: Essays in Regional Politics. Edited by Christian P. Pottholm and Richard Dale. (New York: The Free Press, 1972. Pp. 418. \$12.95.)

South Africa and the World: The Foreign Policy of Apartheid. By Amry Vandenbosch. Lexington: The University Press of Kentucky, 1970. Pp. 303. \$8.50.)

Southern African affairs are increasingly drawing the attention of social scientists as they search the globe for issues and action. If nothing else, Southern Africa has something for everyone—racial conflict, guerrilla insurgencies, authoritarian governments, strategic materials, valued geopolitical terrain, and lots of capitalist investment. These two books, to one degree or another, take up most of these issues as well as many others.

The Vandenbosch book focuses primarily on South Africa and claims to be an analysis of the foreign policy of apartheid—that is, South African foreign policy since the National Party

came to power in 1948. The book is really not very adequate except as an elementary primer on some public aspects of South African foreign policy.

Nearly half the book is given over to the period *before* 1948, for which Vandenbosch does little more than thoroughly review the ideas of General Smuts from his early anti-British/anti-African essay of 1899 (Vandenbosch, however, uses the South African terminology of "Bantu" throughout) to his postelection defeat comments in 1948. In a country with few foreign policy heroes, Vandenbosch is clearly stuck on Smuts, but there is little he can do with the inescapable fact that for all Smuts's high-minded idealism abroad, he was just another racist politician on his own turf.

For the period after 1948, Vandenbosch takes up one issue area after another—the High Commission territories, Rhodesia, the Commonwealth, South West Africa, and the external dimensions of South Africa's internal (eternal?) commitment to apartheid. Here he has drawn largely on United Nations debates and resolutions, in which much of the skirmishing about South Africa has occurred. That pretty much sums up the book. No effort is made to show how any one issue bears on any other. No indication is given of South African foreign policy priorities—regionally, continentally, or worldwide. Nothing is said about how foreign policy is formulated, what interest groups must be served, or for that matter whether South Africa even has much of a foreign policy beyond trying to keep enemies at bay and friends bought off. South African ties to the West—Britain, France, the United States, West Germany—are scarcely mentioned; Japan never is. Until the final pages, South Africa's outward strategy of soliciting friendly relations with black-governed African states is seldom noted, and nowhere is the outward strategy assessed in light of South African commitments to neighboring white states.

Vandenbosch takes a quietly critical view of South Africa, but he sees no point in pushing the whites, instead vaguely preferring to hope for self-generated change from within. If that was not possible during a half century of Smuts, it is even less likely now. The book is surprisingly narrow in scope, given its title, and somehow both tedious and without depth in those issue areas it does take up. There is little here for the specialist and not much more for the general reader.

Potholm and Dale have taken on the broad task of bringing together twenty-three original

essays (largely either by academic specialists or governmental spokesman) within a tentative framework suggested by subsystem analysis. Their intention is to link in a single volume the major perspectives on the future evolution of all Southern Africa. The goal was a difficult one to coordinate successfully, and this volume, conceived in 1968, took more than four years to get out.

The length of time between commissioning of the essays and publication has caused the essays to differ considerably in the period they cover; some go only until late 1968, while others take their analysis well into 1971. Needless to say, the twenty-three essays are not all of equal value; indeed, it is all but impossible to compare essays written by academic experts with those commissioned to convey a particular governmental view. Nonetheless, a few essays stand out.

Each of the editors has contributed an essay on the country in which his own work has concentrated (Dale/Botswana and Potholm/Swaziland), and both pieces lucidly and competently lay out the dilemmas that the countries face in their dealings with South Africa. Potholm's essay is set firmly in the framework of subsystem analysis. Dale concentrates on showing us that much about Botswana's politics depends upon the eye of the beholder.

Ronald Chilcote's essay, "Mozambique: The African Nationalist Response to Portuguese Imperialism and Underdevelopment," analyzes the stages of Portuguese imperialism in Africa. Chilcote directs our attention to the deep penetration of the Portuguese economy by Britain. These themes, using this methodology, badly need further exploration for all the countries of Southern Africa. It is really the great weakness of this volume that the nature of foreign investment and trade and the broad penetration of the region by Western powers have not been seriously considered.

Eschel Rhoodie's essay, "Southern Africa: Toward a New Commonwealth?" gives wide exposure to his thesis that because of various ties (largely economic), Southern Africa will develop as a "Third Africa"—a regional grouping as distinct as North Africa and the preponderance of black-governed Africa. John Marcum's essay, "The Exile Condition and Revolutionary Effectiveness: Southern African Liberation Movements," has justifiably drawn widespread attention; it is the most ambitious essay in the collection. Marcum has attempted to analyze the effects of environmental, technical, and existential problems on Southern African

revolutionaries in exile. In this richly documented essay, he draws some sobering conclusions on how the exile condition (if not in one way, then in another) generally serves to weaken and discourage revolutionary commitment.

Any number of other scholars have contributed sound essays in their particular fields of interest: e.g., Denis Worrall on Afrikaner nationalism; J. E. Spence on South Africa's outward policy; Douglas Wheeler on Angola; Yash Tandon on the OAU and Southern African liberation. They are solid contributions but generally do not go beyond much of the authors' other published work. Only a couple of the essays are very weak, and they will be readily apparent to any knowledgeable reader. On the whole the essays are well documented and the newspaper footnotes are a tribute to Dale's relentless editing (e.g., "*The Star/Johannesburg*", July 25, 1968, 2nd city late edition, p. 10, col. 9.")

Potholm's concluding essay, "Toward the Millennium," tries to pull together Southern African trends into what he calls scenarios for the future. Nine such scenarios are postulated. For the short term he sees the status quo prevailing; in ten or fifteen years he suggests an alteration of the Southern African system through the liberation of Angola and Mozambique. Toward the end of the century he suggests the possibility that externally induced change, backed by moderate levels of coercion, will finally reach South Africa. The Dale/Potholm reader brings so much material together that no one with a Southern Africa interest could fail to find it very useful.

LARRY W. BOWMAN

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The United States and Revolutionary Nationalism in Mexico, 1916-1932. By Robert Freeman Smith. (Chicago and London: University of Chicago Press, 1972. Pp. 288. \$12.00.)

Two conflicts are represented here: (1) between the rights of the Mexican people to institute revolutionary reforms and the rights of investors in the United States to protect their assets in Mexico, and (2) between the approach of the historian who wrote the book and the needs of the political scientist who reads it.

When Mexico began moving toward greater control of foreign-owned interests, Washington moved to protect the assets of investors from the United States. Professor Smith's chronolog-

ical description of the events of 1916-1932 is based on a careful search of written material—government documents, private diaries and memoirs—much of which was previously unpublished. More information was obtained from United States sources than from Mexican; south of the border, the author was allowed to see only some of the records in the archives, and reports that "these records were screened before they were delivered to me" (p. 271).

Nevertheless, the book does not manifest the common problem of United States' ethnocentrism. While the picture offered by the author focuses more on the U.S. side, this does not seem to be the result of a biased view. Rather, it follows from the author's interests, the availability of material, and the self-evident fact that the policies of the powerful United States generally had the greater influence on historical events.

The book starts off with background information on Mexico and the revolution. Chapter Two, entitled "The Problems of the 'Backward' Nation," is a good description of the attitude of the United States (and particularly of President Woodrow Wilson) toward what we now call developing countries. Each of the next seven chapters covers a chronological period; the names of some are highly suggestive—"Keeping Mexico Quiet, 1917-1918" (Chap. 5); "Confrontation Diplomacy and the Article 27 Syndrome, 1919-1921" (Chap. 7); "The Temporary Triumph of Finance Diplomacy, 1925-1932" (Chap. 9).

Smith gives us an insight into Washington's foreign policy-making process. We see politicians, lobbyists representing financial and oil interests, and public opinion in action. Mexican decision making is also described, but in my opinion, the portions on the United States are better—perhaps as a natural outcome of the more abundant written sources.

The second conflict mentioned at the beginning of this review—between the historian author and the political scientist reader—is caused by insufficient general comment and analysis. The book is good history, but it would be a better book for political scientists if the author had elaborated on the meaning and the importance of the data. There is no conclusion at the end of each chapter, nor at the end of the book.

Still, Smith does provide some commentary. For example, he thinks that the financial interests fared better than did the oil companies. "Today, Mexican bonds are sold on Wall Street, and the offices of major United States

banking firms greet one on almost every corner of Mexican cities. The same cannot be said for the foreign oil companies" (p. 265). In fact, the oil industry was nationalized in 1938, at the peak of an even more dramatic period (Cardenas's presidency); this book provides good historical background for the later events. The author also comments that: "The reluctance to use military force was one of the more important aspects of this competition, and directly influenced the extent and type of control which U.S. leaders were able to exercise in Mexico."

Much of the book is relevant to today's events. The current efforts of the Mexican government to redistribute income are again meeting unfriendly reactions from foreign capitalists who fear what they call "confiscatory policies"; these reactions put serious constraints on President Echeverria's plans for social reform. The book should be read by those who seek progress for Latin America, since it helps provide an understanding of how to deal with the Colossus of the North. It is a guide to the many decision-making forces in the United States Congress—the bureaucracy, mass media, public opinion, and private interests—through which Latin American political action can further the positive aims of revolutionary nationalism.

Although the lack of a summary chapter is a shortcoming, the reader can go back to the preface for comments such as "Analysis of the U.S. confrontation with revolutionary nationalism in Mexico gives, I think, some support to the view that attempts, no matter what the intentions, to control and dominate underdeveloped countries produce more ill will and complications than solutions." The book is an important contribution to the understanding of relations between the United States and Mexico. It also provides a good background for those interested in the political systems of the two countries, and the relations between them.

DAVID SCHERS

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Treaty Sources in Legal and Political Research: Tools, Techniques, and Problems—The Conventional and the New. By Adolf Sprudz. Institute of Government Research, International Studies, Number 3. (Tucson: The University of Arizona Press, 1971. Pp. 63. \$1.50, paper.)

The Institute of Government Research at the University of Arizona has been producing a series of occasional studies in such fields as American government, Arizona government,

comparative politics, international relations, and political theory. These have been useful studies that provide us with information and analyses usually not susceptible of being compressed to article length but too limited to warrant extension to book length.

Adolf Sprudz's review of treaty sources provides a valuable guide to primary materials essential for legal and political studies and, indeed, for studies of certain domestic problems, such as the condition of the environment, that are profoundly affected by the nature and effectiveness of agreements among governments. Fifty-seven pages of text guide the researcher to the vital collections of documents and provide him with indications of the sorts of information that is obtainable from the several sources. Overall, the picture presented is also a guide to deficiencies and, therefore, to the types of work still to be done if researchers are to be able to obtain treaty information with welcome speed and convenience.

Sprudz properly gives attention to conventional tools and techniques for obtaining and using treaty information. But he also reviews the computer-assisted treaty projects that were in progress at the time of writing. Brief descriptions are provided of the United Nations Treaty Series Project directed by Peter H. Rohn at the University of Washington, the U.S. Department of Defense International Agreements Project, the United Nations Computer-Assisted Treaty Index Project, and the Queen's University Treaty Project under Hugh J. Lawford. The last has sought to establish the most complete records possible concerning British treaties and their application to Canada and other Commonwealth countries.

However, as Sprudz points out in a brief conclusion, incompleteness of national and international treaty records plagues the researcher. The ideal solution would be the creation of a computerized all-encompassing worldwide treaty-data bank, updated continuously, and dispatching answers to inquirers by teletype. But the probability of states' changing their attitudes and policies is so slim that we must expect to put up with inadequacies that grow more pronounced as the volume and complexity of international agreements grows. To the extent that governments are as hampered as researchers by the inadequacies of outdated information systems, one wonders why we should expect sensible decisions to be made. Yet, as excellent monographs like the one under review serve to remind us of what is needed and what can be done to improve information systems,

we can hope that in time information will be more effectively managed.

WESLEY L. GOULD

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Hidden History of the Korean War. By I. F. Stone. (New York: Monthly Review Press, 1969. Second ed. Pp. xvi, 368. \$7.50.)

This controversial book, which was originally published two decades ago, has been reprinted, the publishers say in the new introduction, because it serves three purposes: it raises questions about the origins of the Korean war; offers a case study in war propaganda; and shows how the American military and the South Korean government did their best to drag out the war and disrupt the peace talks.

It is appropriate to review this book again, because, as the publishers suggest, some people, particularly anti-war groups, believe or suspect that there is a close parallel between the Vietnam war and the Korean war in all the above three respects. The analogy between the two wars has become more tempting after the disclosure of the Pentagon Papers.

This rather slim volume contains highly explosive arguments and observations. Stone's interpretation of the origins of the war is drastically different from the official version which most political scientists do not question. This well-known journalist attempts to show that John Foster Dulles, General Douglas MacArthur, President Syngman Rhee, and Generalissimo Chiang Kai-shek might have connived to create a Pearl Harbor or a Sarajevo in the Far East in order to "commit the United States more strongly against communism in the Far East" (p. 21).

Mr. Stone speculates, among other things, that North Korea would not have been so foolish as to choose that day and year (June 25, 1950) for invasion even if she had had such a plan, particularly for two reasons: First, at that time the anti-Rhee forces which were willing to negotiate unification were gaining strength, and second, the Soviet Union, which some American leaders believed instigated or inspired North Korea to invade South Korea, was absent in the Security Council where she could have prevented United Nations action against North Korea (p. 65). But a contrary view is equally plausible. On the first point, Stone fails to describe accurately the political situation in South Korea during this period. Contrary to his contention, the anti-Rhee forces held a view similar to Rhee's on the question of Korean reunification. Moreover, there was in my opinion an urgent need for North Korea to invade

South Korea because the so-called people's uprisings organized by the South Korean Communist Party had failed, and Communists were ruthlessly being routed by the combined forces of the South Korean army and police. Park Hun-Young, then Deputy Premier and Foreign Minister, according to some sources, urged Kim Il-Sung to save South Korean Communists. Park, who had himself organized and led the South Korean Communist Party (officially called *Namrodang*—South Korean Worker's Party), told the North Korean leader that if the North Korean army invaded South Korea, South Koreans would spontaneously rise against the Rhee government.

On the second point, John Spanier's interpretation is no less plausible or perhaps more convincing. Just as Mr. Stone suggests that the South Koreans with help of the American military maneuvered the Truman administration into a position from which it had no choice but to back their move, so Mr. Spanier hints that the North Koreans maneuvered the Soviet leadership into a similar position (*The Truman-MacArthur Controversy and the Korean War*, p. 23).

From the above speculation on the origins of the war, Stone develops his argument on MacArthur's delay tactics. He argues that MacArthur first hid the information that Chinese communists intervened in the Korean war, fearing that if the fact had been publicized, the American people would have opposed the invasion of North Korea by United Nations forces, and particularly by United States troops (p. 158).

MacArthur, we are told, later exaggerated the extent of Chinese intervention to stall peace moves which seemed to be brewing in Peking and Washington. He purposely risked collision with "Chinese hordes" in the famous Home-by-Christmas offensive because he felt that he would achieve his goal whether the campaign succeeded or not. The author believes that MacArthur's overall strategy was designed not to restore peace in the Far East, but to preserve tension in that area so that the United States would not vacate the region to communism.

The underlying assumption running through Stone's analysis of the MacArthur "conspiracy" is the perfect rationality of the Far Eastern Command. He pictures MacArthur and his aides as cool-headed and shrewd human beings who never made mistakes. Here we find a curious aspect of his reasoning: In his mind Chinese and Russian leaders are vulnerable to all kinds of tricks and deception, while American leaders are not.

This basic assumption runs through his analysis of newspapers and official documents. His analysis of official statements and news reports is truly ingenious and intuitive. One excellent example is his hair-splitting recounting of the number of communist troops in Korea as of December 28, 1951, estimated by the MacArthur Headquarters (pp. 223-24). He tries to show how shrewdly MacArthur exaggerated the threat of a new communist offensive by publicizing the false estimates of enemy strength. He points out that the MacArthur Headquarters contradicted itself by giving vastly different estimates even within a single week. His analysis proves, however, less that the MacArthur propaganda machinery was extremely shrewd and rational than that it was, as I have contended, rather unreliable and inefficient.

Even though his view of the origins of the Korean war is not convincing, it is still stimulating because it is the first major revisionist view of the Korean war. History is full of unanswered questions, and the history of the Korean war is no exception. It is almost inevitable that important historical incidents—and particularly, ideological disputes—are subject to different interpretations. The question of who started a war is as difficult as the question of what is aggression.

In conclusion, the author's journalistic intuition has done more harm than good to his painstaking effort to search the origins of the Korean war.

SANG-SEEK PARK

Hampton Institute

A New Isolationism: Threat or Promise? By Robert W. Tucker. (New York: Universe Books, 1972. A Potomac Associates Book. Pp. 127. \$2.25, paper.)

Robert Tucker is one of the most reasonable analysts of American foreign policy writing today. His recent book, *The Radical Left and American Foreign Policy*, has been widely read and highly praised because of its thorough understanding and fair rendering of contending arguments in the contemporary foreign policy debate. His new book, *A New Isolationism: Threat or Promise?*, is of comparable quality. The two works neatly complement each other; whereas the former book focuses on explanations of the past, the latter focuses on prescriptions for the future. In both works, Tucker shows himself to be a master craftsman of reasoned discourse; here, at last, is a man with a refreshing concern for valid arguments and a refreshing contempt for silly ones. Yet, as we

shall see, *A New Isolationism* is constructed upon an important confusion.

Tucker sensibly begins with a definition of his central term:

Isolationism is not to be identified with "quitting the world," something we have never done and will never do. It is not to be identified with the absence of all significant relationships but, rather, with the absence of certain relationships. As a policy, isolationism is above all generally characterized by the refusal to enter into alliances and to undertake military interventions (p. 12).

He then asks if the United States can pursue a new isolationism without jeopardy to or sacrifice of its vital interests; his answer is yes, and he proceeds to argue his case with a consideration of "strategic realities," "economic necessities," and "security in the greater-than-physical sense" as well.

In regard to strategic interests, he attacks two familiar arguments which have been used to deny the possibility of a new isolationism: (1) the old argument that a hostile power controlling Western Europe would upset the balance of power and would pose a direct threat to the United States and (2) the newer argument that the U.S. must maintain its alliances in order to prevent nuclear proliferation, because an increase in the number of nuclear powers means an increase in the probability of nuclear attack on the U.S.—the argument that a "nuclear peace is indivisible." He argues that nuclear missiles have made conventional balance-of-power calculations obsolete; "provided that America maintains the strategic forces necessary to deter attack, alliances cannot enhance a physical security that is no longer dependent on what transpires outside the North American continent" (p. 46). He also argues that the prime candidates for new nuclear powers (e.g., Japan, Israel) would be as prudent as the present nuclear powers and also that nuclear weapons have actually made "peace more divisible today than it has been in a very long time" (p. 54).

As for economic interests, Tucker argues that America is not in fact economically dependent on involvement abroad; even if it were, however, foreign nations in their own self-interest would continue trade and investment relations with the U.S., even if the American alliance structure were dismantled. And on the complex issue of "security in the greater-than-physical sense," Tucker offers an appropriately complex argument that takes into account different U.S. interests in the Third World, in Japan, and (by far the hardest case for his position) in Western Europe.

These and other arguments in the book are all put forward with a sureness and subtlety that my summary can hardly capture and in an eminently reasonable style and tone.

No book on such a topic can be free of flaws, however. I, for one, think that Tucker is far too sanguine about the Soviet threat to Western Europe, about the degree to which our lives would be diminished by a Western Europe under either a Soviet hegemony or an anti-Soviet authority, and about the dangers to the U.S. of nuclear proliferation. A foreign policy constructed on the assumption that all new nuclear powers will be prudent at all times is itself not a prudent policy.

Let us stipulate, however, that Tucker has succeeded in demonstrating that there are no good reasons for an interventionist policy. A critic might still ask are there any good reasons for an isolationist one?

Tucker does not give us a systematic list of American foreign policy failures since World War II, which would presumably justify a new policy of isolationism. But if he were to construct such a list, I suspect that it would be rather short. The Vietnamese War is a clear enough case, and perhaps he would add the Korean War, the military interventions in the Dominican Republic and in Lebanon, and the proxy interventions at the Bay of Pigs and in Guatemala. These cases would probably complete the list. (Beyond them, for example, in the economic and military assistance programs, it is by no means clear that the failures are significant enough to justify a new isolationism.)

Here it is important to distinguish between two aspects of Tucker's definition of isolationism, which he tends to fuse or confuse: a refusal to enter into alliances, and a refusal to undertake military interventions. In the perspective of almost thirty years since the end of World War II, it is doubtful that any of the American alliances has been the cause of a major failure in U.S. foreign policy. None of the U.S. military interventions were required by a U.S. alliance. The much-derided SEATO treaty had to be stretched and contorted to apply to the Vietnamese War, the OAS treaty similarly had to be stretched and contorted to even permit the Dominican intervention, and in the cases of South Korea and Lebanon there was no alliance commitment at all; similar considerations apply to the U.S. proxy interventions. Rather, the American system of alliance treaties has been innocuous at worst and judicious at best. Of course it is true that a military alliance inherently carries the risk of military intervention on behalf of the ally and against external ag-

gression (although not necessarily against internal rebellion), and continued adherence to an alliance could mean war in the future. But Tucker does not argue that this is a likely threat, and he leaves us with no good reasons why the U.S. should withdraw from its present alliances.

In regard to the second aspect of isolationism, a refusal by the U.S. to undertake military interventions within the internal conflicts of other states, Tucker has a strong case. His case would have been even stronger (but much less original) if he had focused his arguments for isolationism on this one aspect alone.

A New Isolationism is, nevertheless, a careful and reasonable statement of an historically important position. For more than thirty years, American scholars have seen "splendid isolation" as more sordid than splendid. With this book, Robert Tucker has restored to intellectual respectability an ancient and honorable tradition in the study of American foreign policy.

JAMES R. KURTH

Swarthmore College

The Rivals: America and Russia Since World War II. By Adam B. Ulam. (New York: The Viking Press, 1971. Pp. 403. \$10.95.)

"When it comes to political disasters, Americans are strangely unwilling to accept an error of judgment as an explanation. They tend to seek the answer in moral guilt," the author states on page 93 of this eminently readable book. He also approvingly quotes Napoleon (p. 318): "War is often the study of man, rather than men, who is of decisive importance"; the same probably holds true of foreign policies of this age." These two views inform the whole book which is in essence a rather personal and subjective report from the diplomatic and political battlefields of the period since the war.

Ulam's assessment of the postwar relations of the two superpowers is a scathing list of indictments rather evenly divided between the leaders in Washington and Moscow. The "great fault of American policy-makers in the immediate post-war era was *not knowing* [original italics] . . . how strong America was in relation to the rest of the world," and in the subsequent period "the main fault of American policy-makers was still to lie in *not understanding* . . . the internal weaknesses and tensions which gripped the communist world and which still offered great opportunities for American diplomacy" (p. 238).

Now, if American leaders are thus accused of grave acts of omission, the Soviet leaders are

accused of bungling through acts of commission. Ulam sees much of Soviet postwar diplomacy and political-military activity as seeking the erosion of a feared anti-Soviet capitalist alliance. The Soviet leaders, he suggests, were quite clear about their own military and economic disadvantage vis-à-vis the West, and sought to camouflage this weakness by means of ill-advised militant actions, which only resulted in over-reactions from the West. This curious dialectical vision of recent history, in which the worst of opposing political-military factors clashed sporadically and gave rise to even more terrible syntheses is seen by Ulam as having a dynamic of its own, deriving from the "mad momentum" of nuclear technology and the persistent failure to perceive the adversary's fears and intentions.

The historical terrain covered by Ulam is familiar. There is the drama of Teheran, Yalta and Potsdam; the duplicitous games played by Stalin, who presumably understood the Americans cherished illusions and predilections for moralizing, and above all, the Americans' sense of guilt: guilt over the delay of the Second Front (while the Russian people and soldiers bore the brunt of Nazi armies); guilt over the destructive role in the League of Nations (and thus the feverish efforts to make the United Nations work, at almost any cost). There was also the American obsession to win the war first and only then consider the problems of postwar political settlements; the moral double-standards of the Americans which motivated them to lecture their British allies on the evil of colonies, while conveniently overlooking the creation of a great Stalinist colonial empire in East Europe.

Ulam is rather democratic in his assignment of blame and fatuity in high places. If the Soviets badly desired an American withdrawal from international politics in the postwar period, then they could not have acted more fatuously than they did. There is almost a "design to fail," as perceived by Ulam, in Soviet policy: since the Soviets' great fear was a rearmed Germany, they actually made this terrible nightmare a self-fulfilling prophecy through Stalinist terror in East Europe, the Berlin blockade, and the Korean aggression; if the Soviets desired to keep Communist China in a dependent, "colonial" position, they subsequently destroyed that objective through the Korean war (which enhanced Mao's prestige and self-assurance), the de-Stalinization policies in East Europe, and the doctrinal vendettas in the late 1950s; if they sought to pressure the U.S. to reach a reasonable political-military accommodation in the

late 1950s, they sabotaged their own objective because American over-reaction to Soviet strategic threats and diplomatic blackmail resulted in a vast U.S. arms program which clearly established Soviet strategic inferiority.

Ulam stresses "failed expectations" and "misperception" in the relations between the two Rivals as a crucial factor in the deterioration of international politics, and as a key source of pervasive insecurity which led to intensified hostility. The Soviets had anticipated that the U.S. would return to its neo-isolationist position after the war, and instead they faced the Truman Doctrine, the Marshall Plan, NATO, a rearmed Germany and a massive U.S. presence in Europe; on the other hand, the Soviets had expected a strong U.S. interest in Kuomintang China, and instead saw a vacillating U.S. policy which resulted in the victory of the communists in that huge country; having absorbed this "lesson" of China, Stalin anticipated an easy conquest of South Korea by his "proxies," and instead saw the U.S. give its fullest commitment and risk war in that rather politically and militarily insignificant country; and similarly, having learned his "lesson" from the American fiasco in the Bay of Pigs, Khrushchev applied that lesson in 1962, during the Cuban missile crisis, only to realize once more the "unpredictability" of the U.S. leadership.

As for the American side, Ulam blames our anti-Communist hysteria, our moralistic stance toward our allies and enemies, and our technological-economic *hubris* as having heightened the threat-perception of the paranoid Soviet leaders and thus having effectively cost us several opportunities for stabilizing our relations with the Soviets at an early phase of the postwar period.

Ulam's treatment of Soviet-American relations seems to be premised in a particular kind of Realpolitik, leavened with a sense of ethical imperatives: to paraphrase a central theme of his book, the key factor in world politics is power; the Soviet leaders, who are expansion-prone, need to be contained by the application of power; American leaders failed to understand this basic axiom, and missed several opportunities to apply power in order to contain and discipline a weak Soviet Union. This is a rather questionable proposition: one may possibly suggest that it was Soviet weakness and insecurity that propelled the cold war, and as the Soviets became more powerful and more secure, that they also perceived the limits of power in a nuclear setting.

It is difficult to disagree with Ulam's analysis of the Revisionist school of history. He mar-

shals his facts, focuses on crucial timing and sequential events in order to debunk the mea-culpa trance of the ethical chic. If the U.S. went after empire in the postwar period, it was surely in a fit of absent-mindedness, or more properly, without a clear notion of the consequences caused by the simplistic anti-communism of the period. If there was a fatal flaw in our behavior, it was not the imperial urge of American leaders (Truman and Eisenhower are rather unlikely Caesars), but rather the collapse of neo-Wilsonian dreams of F.D.R. and his advisers, the realization that the U.N. was likely to become an unviable institution, and that Stalin and his successors were bent on exploiting the political vacuum in Europe created by the war.

Ulam's treatment of recent history stands apart from most schools and sects of historians. The book will undoubtedly appeal to those who are skeptical of the writings of the revisionists, the universalists, and the ideological sectarians. It will also appeal to those who are disenchanted with the systems-analysis whizz-kid-dery, bureaucratic mandarins, and all those quantifiers of politics and history who reduced the total flow of international politics to a new infelicity calculus, to technological banalities dressed-up as political profundities. However, to describe mid-twentieth century international politics in Carlylean or simple Realpolitik terms is rather archaic. For the publication of the book comes at a time when the old order of things, the world of the Two Rivals, is changing greatly. The old verities of the Yalta System no longer hold. The two Rivals are now being challenged by other aspirants to major power, who seek to change the rules of the game, and the two Rivals are finding it necessary to adapt to these challenges. It is not very useful to reduce the global relations of superpowers to a rather simplified game between several political actors who are considered either as dupes or machiavellians. It might be more useful to view these men as being possessed of great power but of dim vision, whose freedom of action is severely circumscribed by the parameters of nuclear technology, institutional-bureaucratic structures, troublesome alliance commitments and the need to maintain the delicate balances of mutual deterrence.

Professor Ulam's book, despite its intensely polemical tone, is an important addition to the literature of diplomatic history and international relations.

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Traité des territoires dépendants. Tome II: L'Oeuvre fonctionnelle des Nations unies. By Nicolas Veicopoulos. (Paris: R. Pichon et R. Durand Auzias, 1971. Pp. 501. No price indicated.)

This study is a useful source of information on United Nations activities regarding trusteeship territories since World War II. More than a dozen years ago, Veicopoulos published the first volume, which in 530 pages elucidated the theory underlying the United Nations exercise of trusteeship. Of equal length, this second volume traces the daily concern of all United Nations organs with the trusteeship territories. The goal of the author was to present a handy summary of the trusteeship question in the United Nations. The author has accomplished this aim.

My criticisms are mainly concerned with the methodology underlying the study. It is in the tradition of many European legal treatises, interested in the rules and regulations passed by an agency but without references to the outside world. The trusteeship question in the United Nations is described *in vacuo*; one would have no idea from reading the work that the membership of the United Nations has undergone significant changes in the course of the last quarter-century, that the cold war was raging, that the populations in both the mandate territories and the ruling countries became strongly affected by anti-colonial ideologies.

The granting of independence to the trusteeship territories is generally presented as a victory for the United Nations, the happy culmination of years of resolutions by the Trusteeship Council and other United Nations organs directing the trustee power to lead their charge to independence. In reality this end should be seen more as part of the decolonizing processes of all the empires. The lack of response which South Africa for instance has paid to Trusteeship Council and General Assembly resolutions on South West Africa proves that successful decolonization is due in greater part to the aims and goals of the colonizing power than to the endless United Nations resolutions.

Veicopoulos gives the impression that France granted independence to its part of Togo because of United Nations pressure. But in reality the causes were quite different. The British part had decided to join with the Gold Coast, which was soon to be independent. French Togo, like its British-ruled neighbor, had a mainly Ewé population; the French feared that the Ewé would want to join together the Gold Coast with British Togoland. After ruling their man-

date for nearly forty years, the French, it seemed, were destined to see Togo absorbed by the Gold Coast, an English speaking area within the orbit of British influence. Wishing to avoid this possibility, the French decided to give Togo independence, so that it was the African situation, rather than the debates in the United Nations which dictated the manner and timing of independence.

One actual function of the United Nations Veicopoulos does not cover, since he is only interested in the activities inside the glass building in New York; this was the encouragement the United Nations implicitly offered to nascent nationalist movements in the mandate territories. Knowing that there was an international body to which they could send their grievances and from time to time visiting missions to which they could present their views on the development of the territory, these movements gained a sense of importance. Also, the knowledge that the ruling power was responsible to an international body probably reduced respect for it and thus fostered the growth of nationalist movements. But these considerations are unfortunately outside Veicopoulos's purview.

If there were forces in the mandate territories affecting the administering power, there were also domestic forces in the latter shaping the trusteeship relationship. In the early 1960s the United States, Veicopoulos shows, was more closely and sympathetically listening to suggestions regarding its rule of mandate territories in the Pacific. Some of the idealism of the John F. Kennedy administration (or at least its desire to appear idealistic) and its desire to curry favor with the newly independent, recently colonized nations explain the liberalization of United States trusteeship policy, rather than, again, any particular accumulation of United Nations resolutions.

Veicopoulos uncritically accepts the United Nations goal of independence for all mandate territories. He does not seem to find strange, for example, the United Nations insistence on total independence for the tiny (less than ten square-mile) coral island of Nauru. In 1968 the 3500 inhabitants were given independence by Australia. As former Secretary General U Thant, certainly no friend of colonialism, pointed out, sovereignty might be too heavy a burden for small areas to carry. One might well ask how Nauru and for that matter a large number of other ex-mandate territories really can be independent. The former rulers still retain enormous influence.

Traité des territoires is solely based on United Nations documents; Veicopoulos depends on these sources so much that he even cites trusteeship documents for geographic information on the location of various territories. There are no interviews with former participants, and no study of the world press in order, for instance, to find out what the underlying motives were for some of the speeches and resolutions made on the trusteeship question. If a document was not published by the United Nations, it is not of any interest to the author.

While the book is a useful summary of United Nations resolutions, and decisions, it fails to give the larger context within which they occurred, or realistically to assess their impact. Admittedly, these were not the goals of the author. But lesser aims lead to little more than a dry summary of resolutions on trusteeship territories passed by various United Nations organs. One is left wondering what they meant.

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British Foreign Policy in the Second World War. Volume I. By Sir E. L. Woodward. (London: H.M.S.O., 1970. Pp. 640. £3.62½ p.)

The late Sir E. L. Woodward's long and detailed history of British Foreign Policy in the Second World War was originally written between 1942 and 1950 for official use, and revised thereafter. For a long time, the appearance of the full version was prevented; although an edition in one volume, which must have cost him an infinity of labor to compress, appeared in 1962. It is hard to understand why so prolonged a delay should have occurred, but it has at least two advantages: the author has been able to make use of many sources which have appeared in recent years, and now that the Archives of the British Government are open up to the summer of 1945, it has been possible to equip this edition with references to individual documents so that they may be speedily traced in the Public Record Office.

This volume consists largely of narrative. Sir Llewellyn Woodward's desire has been to provide material for judgment rather than to analyse or philosophise at length. He was not instructed to defend any policy or person, but, in paying tribute to Sir Winston's six volumes, observed that the latter had "rightly allowed himself a personal approach and a freedom of comment from which an official historian is debarred" (p. v). Sir Llewellyn has tried, and

with considerable success, to avoid the besetting sin of books on foreign policy, that of isolating a particular aspect and then concentrating upon it to the exclusion of almost all else. As Mr. Eden once remarked "there are no chapters in foreign policy; only chapters in books about it." Hindsight is carefully differentiated from information available when decisions were made; and a balance has been maintained between the isolation of particular themes, absolutely necessary unless a book on so large a subject is to be a jumble, and their interactions. Statesmen, we are reminded, have to make their choices amidst a welter of business, often with incomplete information, and never more so than in the period covered by this volume. No re-creation, however, skillful, can convey adequately the manner in which different forces and crises converge or the full play of the personalities; nor will the historian imagine that free access to huge archives will give "the whole story." Sir Llewellyn claims nothing of the kind. He acknowledges that much will be learned from archives and private collections not yet open. He remarks at one point that the minutes of the Permanent Head of the Foreign Office during this period, Sir Alexander Cadogan, are delusive in their quietness and modesty. The same judgment might well be applied to Sir Llewellyn's own work, to which he brought acquaintance with most of the leading personalities of the day, unique knowledge of the archives, lucidity, balance and fair-mindedness. His judgments, though infrequently interposed into the narrative, lack nothing in sharpness; there is, for example, a timely onslaught on the prevailing orthodoxy about unconditional surrender (Introduction, pp. liv-lv).

The introduction, a magisterial survey of the broad issues in British foreign policy during the Second World War and of the conditions in which it had to be conducted, all pressed into a little less than 40 pages, should be studied by anyone who picks up a volume of this series. Because there is comparatively little attempt at the evocation of atmosphere or description of character, what is written on those scores is the more effective. There is a mastery of understatement; a most hearty and unfeigned detestation of Nazi policy and ideology; a welcome insistence that it is no part of a historian's duty merely to poke fun at statesmen or deride their mistakes.

Sir Llewellyn explains how the Foreign Office was able to sustain its position far better in the Second World War than in the First. In the phase covered by this volume the formal

functions of the Foreign Office were of course much reduced. Sir Edward Grey used to say "don't rely on your foreign policy to protect this country," but successive Foreign Secretaries between the Wars, and especially after 1935, had been compelled to conduct a foreign policy without the means to make it effective.

"In wartime," Eden wrote, "diplomacy is strategy's twin." This volume therefore needs to be read in conjunction with those in the *Grand Strategy* series and with others published after the War, and especially with Professor Medlicott's *The Economic Blockade*. Sir Llewellyn interviewed many of the leading actors on the British side and took their comments on his draft into account; he had available not only the papers of the Foreign Office and of the War Cabinet, which he might have used more freely had he foreseen their early release, but also many of Mr. Churchill's private papers, including the correspondence with Stalin and Roosevelt, and the records of the principal conferences.

The events which this book records were of the most cataclysmic kind. The twilight war; the Russian onslaught upon Finland; the fumbings and hesitations of the allies over Scandinavia; the balancings of Mussolini; the collaboration between Russia and Germany; the collapse of France, and with it of the assumption upon which every British Government since the First World War had based its European strategy, namely that the French Army would provide the main bulwark against a resurgent Germany; and the abortive offer of an Anglo-French union followed by the recognition of General de Gaulle—all these are recounted in a dry, unemotional, almost matter-of-fact way.

For Britain, survival rested upon the ability to prevent Japan from breaking loose in the Far East, even at the price of closing the Burma Road for three months in the summer of 1940; upon the ability to secure material help from the United States, though it came late and in lesser quantity than Mr. Churchill had hoped; upon preserving the neutrality of Spain; and upon playing a waiting game with Russia. Mr. Molotov's contemptuous treatment of Sir Stafford Cripps is well described. Russia supplied raw materials and openly proclaimed the fact, in order to enable Germany to evade the consequences of the British blockade. Britain could do little therefore but keep Germany at bay, hold out against air attack, maintain mastery of the Channel and the North Sea, and take the risk of diverting substantial forces to the Middle East, the one theater in which she might be able to strike a serious blow on land

against the Axis. Not until 1943 did that campaign come to its successful conclusion, after many vicissitudes; whereas this volume concludes in 1941 with British warnings to Russia of impending attack and Mr. Churchill's broadcast offer of assistance, made because of his conviction that the invasion of Russia was a prelude to the invasion of Great Britain and therefore that the Russian danger was "our danger and the danger of the United States."

Let us hope that Sir Llewellyn Woodward's narrative will soon be supplemented by volumes of documents drawing not only upon the archives of the Foreign Office but also upon the papers of the Cabinet, the Ministry of Economic Warfare and the Service departments. Meanwhile, this book represents marvelous value at £3.62½ p.

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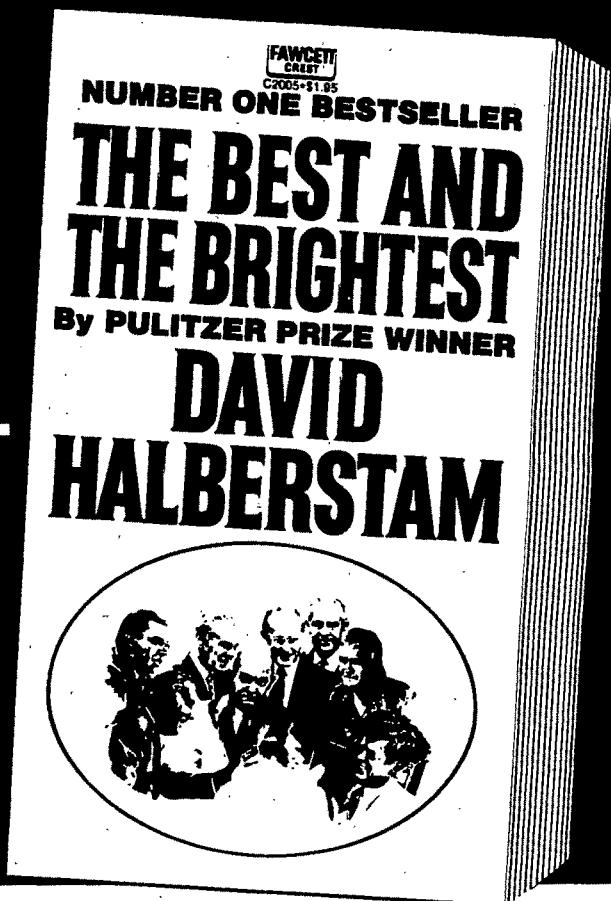


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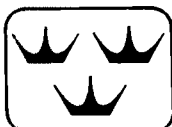
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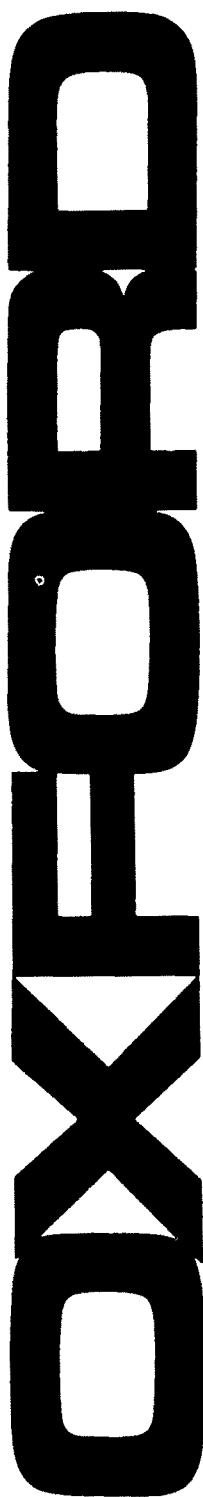
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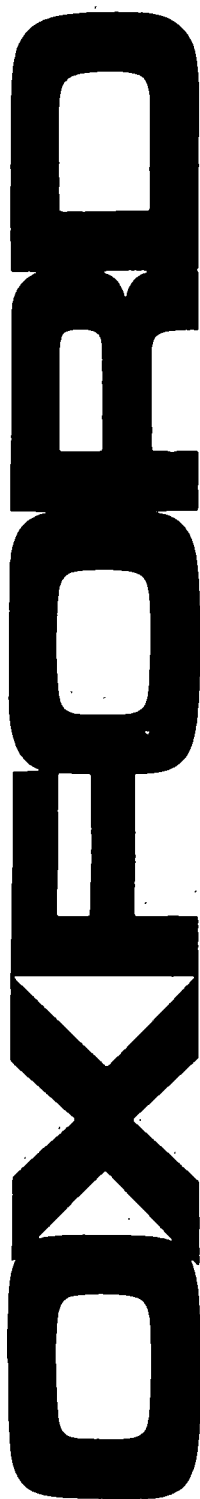
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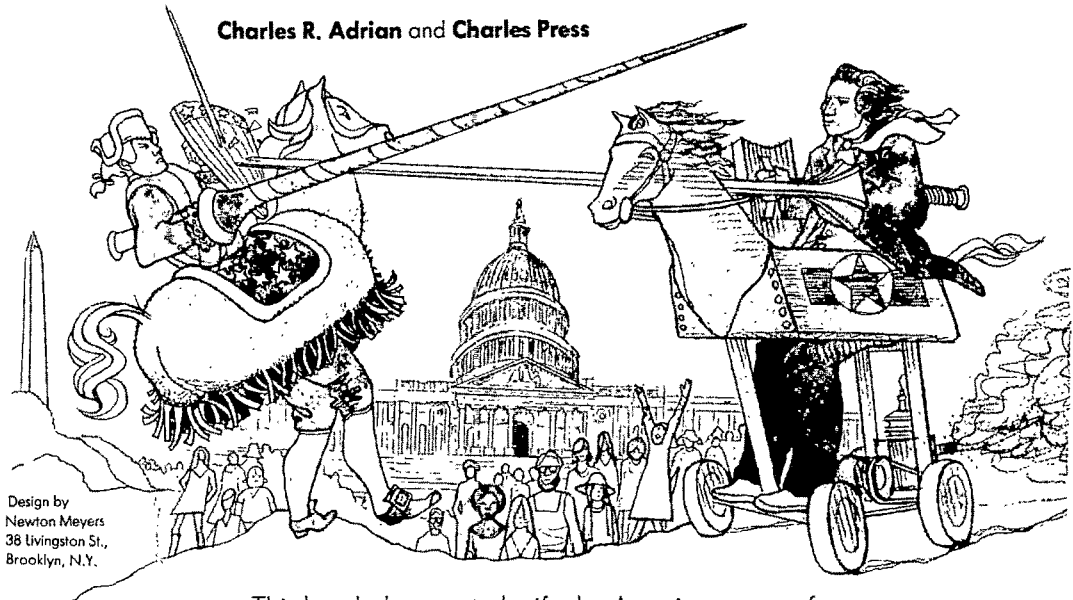
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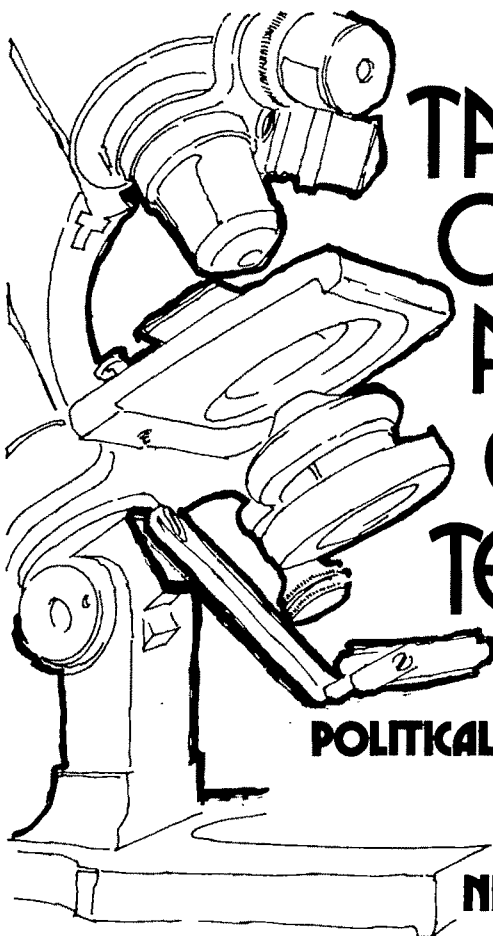
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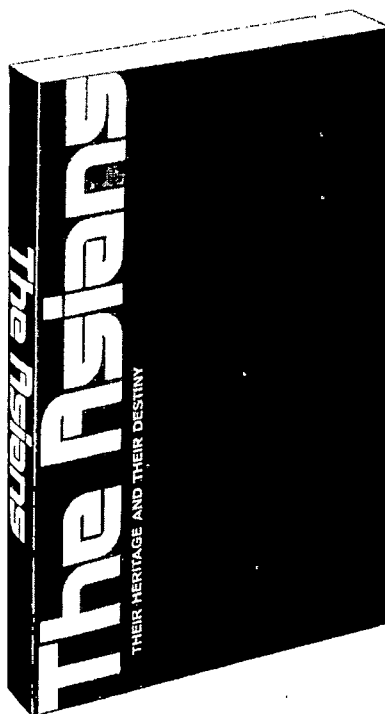
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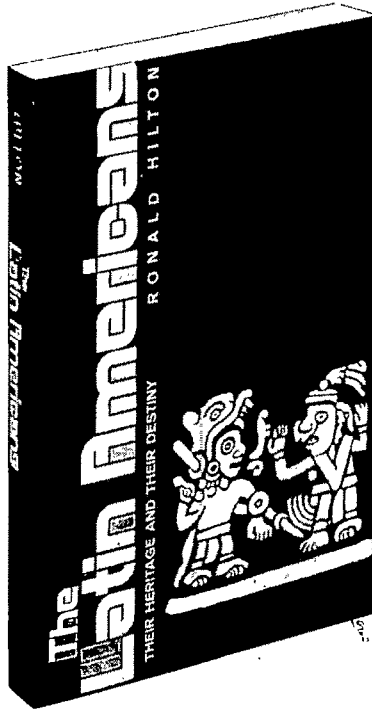
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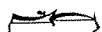


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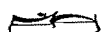


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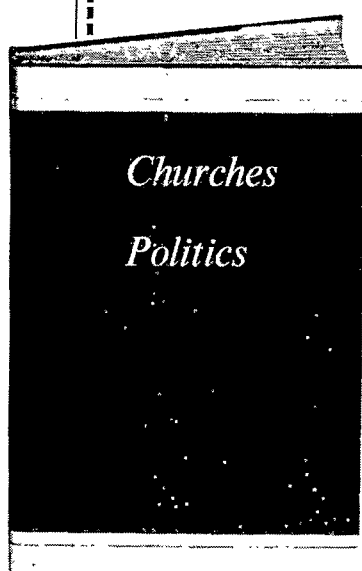
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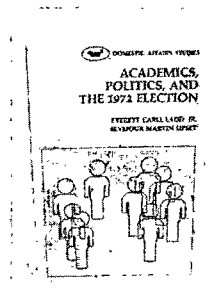
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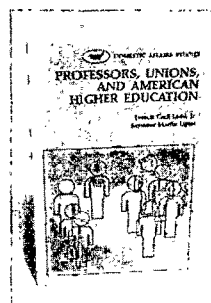


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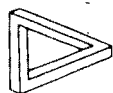
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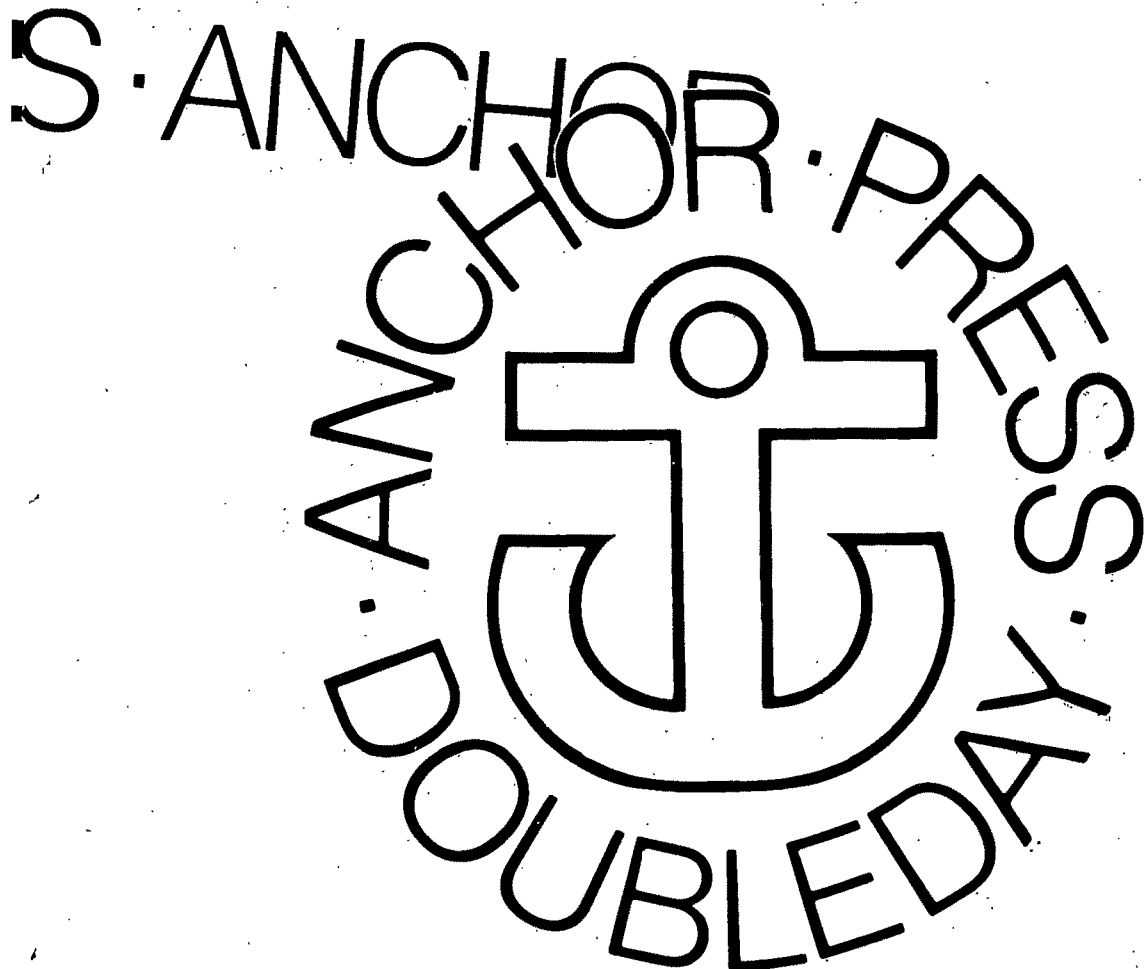
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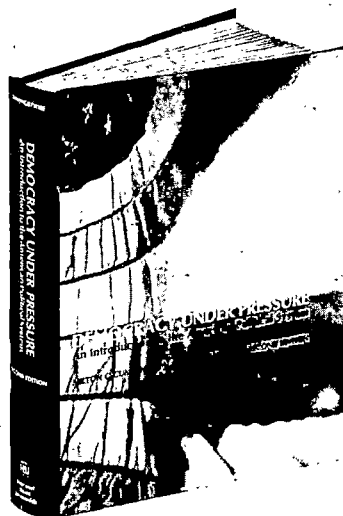
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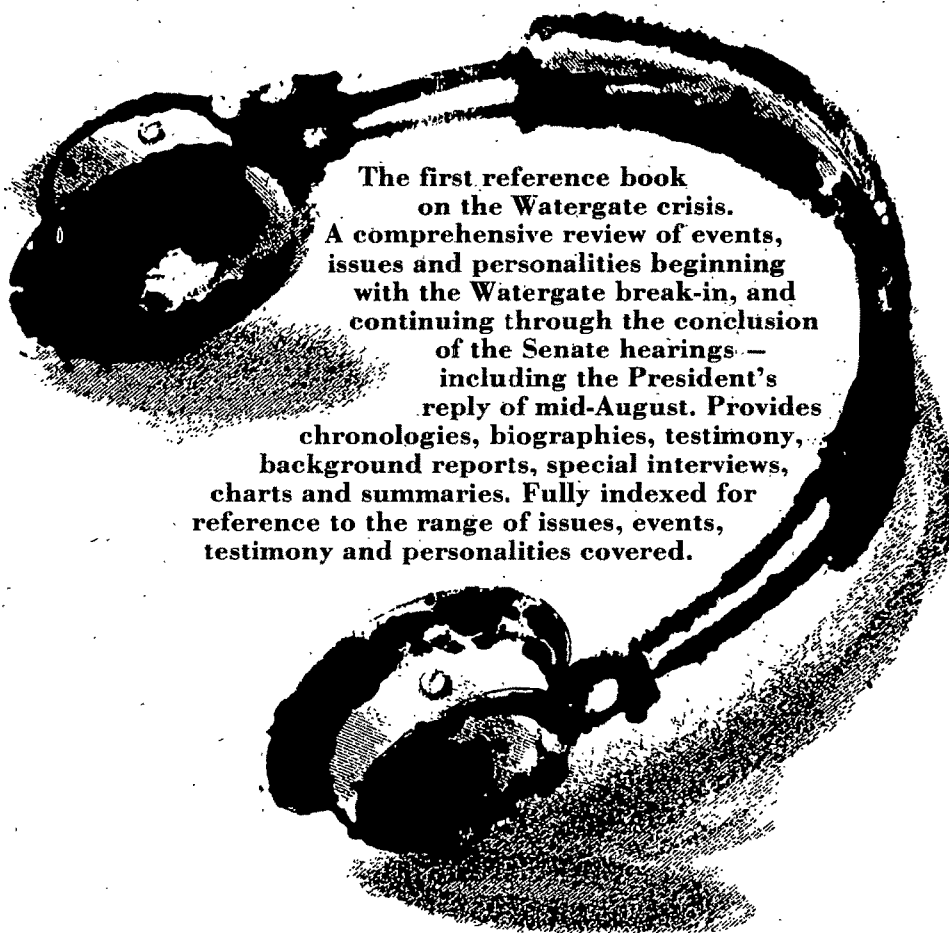
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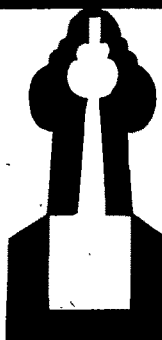
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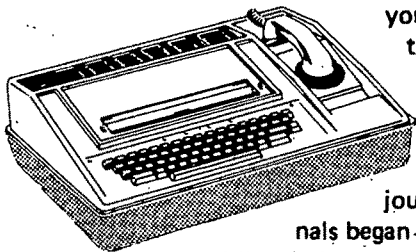
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